A

CYCLOPÆDIA

OF

THE PHYSICAL SCIENCES.

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TO THE

VERY REV. DUNCAN MACFARLAN, D.D.,

PRINCIPAL OF THE COLLEGE OF GLASGOW,

&c. &c.

Rev. and Dear Sir,

I do not know to whom a volume, intended to assist the Student in his progress in any department of Science or Literature, can be more fitly inscribed than to you. And your consent that I prefix your name to this Cyclopædia is the more cherished by me, because, while it enables me to have the honour of referring to the value of your watchful and enlightened superintendence of the University to which we belong, it also allows me the privilege of declaring the respect, esteem, and affection with which I am,

Rev. and Dear Sir,

Your obedient Servant,

J. P. NICHOL.

Observatory,
Glasgow, 30*th January*, 1857.

PREFACE.

THE volume now offered to the Public is, so far as I am aware, the only English one, containing—within a moderate compass—so large an amount of information concerning the condition and progress of Physical Science, and presenting, at the same time, every facility as to *reference*. I earnestly and respectfully hope therefore, that it will prove useful, especially to the Student. That it is what a book with such a title ought to be, I do not in the least imagine. Few living authors are capable of producing an adequate "CYCLOPÆDIA OF THE PHYSICAL SCIENCES." The time required to compose such a book, in the hands of even the best informed and readiest writer, would extend over a far larger space than our modern rapidity of Discovery and the press and hurry of modern Publication, will allow; and the difficulty of obtaining the right kind of assistance can be appreciated only by those who have required to seek it.

I shall not specify the defects of this volume, either as regards proportion or execution; for, they are apparent enough. I believe however, that I know them well; and, assuredly, I could have wished, for the sake of the Reader, that circumstances had permitted me to present him with a volume free from such defects. It is much more agreeable to me, to have the opportunity of thanking a few of the eminent friends to whose kindness it owes its excellences. The philosophical Student of Mathematics will find a series of letters on QUATERNIONS (in the Text and Appendix) which SIR WILLIAM ROWAN HAMILTON alone could have written,—so thorough are they, and so lucid. These remarkable letters will make known, probably for the first time, to many mathematical Inquirers on this side the Irish Channel, the nature, the power, and fertility of the method of Quaternions; and I believe they will be regarded in after times, with the interest with which we refer now to the best parts of existing scientific literature of this description, rich though that is.—A second most valuable contribution is due to the REV. DR. ROBINSON of Armagh. This very distinguished Astronomer and Physicist kindly wrote, at my request, the article SPECULUM; an article in which is presented, to the general Reader—also, I believe, for the first time—a critical account of the inventions and processes by which LORD ROSSE and others have succeeded in bestowing on the Reflecting Telescope a degree of perfection and a grasp, until recently quite unhoped for. Dr. Robinson's article, while succinct, is yet so full and precise, that I am fain to indulge the hope of its stimulating and guiding further enterprise.—To ARCHIBALD SMITH, Esq. of Lin-

coln's Inn, my best thanks are offered for an ample and very instructive article on a question with which his name is intimately associated, viz., the disturbing effect of the *Magnetism of Ships* on the COMPASS.—My colleagues, PROFESSORS WILLIAM THOMSON and RANKINE, have also given me their assistance. To the former I owe many invaluable suggestions, besides his notice on THERMO-MAGNETISM—(a subject which, in its present form, is almost wholly a creation of his sagacity and genius)—and his *Essay in the Appendix* on the ELECTRIC TELEGRAPH: and the originality and power of the latter will be recognized in articles HEAT, ELASTICITY, VAPOURS, STEAM ENGINE, &c. —I am deeply indebted to my friend DR. JOHN TAYLOR of the Andersonian University. He has written many articles on Electricity, and on those portions of Optics that have relations with Physiology and with Photography. Nor do these contributions comprehend the amount of my obligations to him.—I have been favoured, besides, with aid from MR. J. R. NAPIER of Glasgow; from MR. R. RITCHIE, C.E., and MR. JAMES ELLIOT, of Edinburgh; and from MR. KER, one of our recent BREADALBANE EXHIBITIONERS: but I must emphatically specify the services of another and younger Breadalbane Exhibitioner—MR. WILLIAM JACK, now of St. Peter's College, Cambridge. This well-informed and very promising Student has contributed numerous separate articles; and I must say farther, that without the help of his untiring industry, it would have been difficult for me to have gone through the almost intolerable labour inseparable from the producing of a volume like the present one.

Turning from *Men* to *Books*, I shall candidly confess, that when I assented to MR. GRIFFIN'S proposal that I should edit such a Cyclopædia, I had it in my mind that I might make the *scissors* eminently effective. Alas! on narrowly examining our best Cyclopædias, I found that the scissors had become blunted, through too frequent and vigorous use. One great exception exists, viz., the *Penny Cyclopædia* of CHARLES KNIGHT. The cheapest and the least pretending, it is really the most philosophical of our scientific dictionaries. It is not made up of a series of treatises, some good and many indifferent, but is a thorough *Dictionary*, well proportioned, and generally written by the best Men of the time. The more closely it is examined, the more deeply will our obligations be felt to the intelligence and conscientiousness of its Projector and Editor.—But I acknowledge many debts to various separate works in different departments of science. I have spoken frequently in the text of the volume on *Practical Astronomy* by PROFESSOR LOOMIS of New York;—once more, I beg to recommend it as the best work of the kind in the English tongue. May I record the wish, that Professor Loomis would translate the invaluable volumes of BRÜNNOW and SAWITCH? A new Mathematical Dictionary by MESSRS. DAVIES and PECK has recently been published in New York. It is a great advance on BARLOW'S Dictionary, and has many claims on the attention of the Student. I have extracted a few articles from its pages: as I have done also from the useful and pleasing introduction to the *Modern Geometry*, by PROFESSOR MULCAHY of Galway.—In Physics, there is not at present, in English, any standard *general* work. To DR. LLOYD'S Lectures on the Theory of Light, I have been largely indebted, especially in the article on THIN PLATES, and part of the article on DOUBLE REFRACTION. SIR DAVID BREWSTER'S recent volume on Optics has often guided me in the exposition of phenomena; as well as his original Memoirs: and of course the *Répertoire* of the ABBÉ MOIGNO has proved a constant source of information. It is

scarcely necessary to allude to the services—of which I have freely availed myself—rendered by the Abstracts of Memoirs, &c., which are found in our English and Foreign scientific periodicals, viz.:—*The Edinburgh and London Philosophical Magazine*, the *Annales de Physique et Chimie*, and *Poggendorf's Annalen*. Finally, let me acknowledge the obligations I am under to the exquisite and instructive Physical Atlas of ALEXANDER KEITH JOHNSTON, Esq.

The statement now made may convince my Readers that I have not neglected appliances likely to afford adequate aid. I wish I could have lessened farther the very large amount of what—as a glance over the contents of this Cyclopædia will show—must rest on my own responsibility. Owing to the ceaseless but irregular pressure necessarily falling on the Editor of such a volume, I have not been able to restrict myself to the treatment of those parts of Physical and Mechanical Science, with which previous acquirement had rendered me rather familiar: nay, I have, in some cases, felt obliged to delegate the treatment of portions even of these.—So much for the building-up of this book.—May I solicit that it be received in the spirit in which it is offered?

The advantages of Alphabetical arrangement in such a work, are undoubtedly largely counterbalanced by the broken, incomplete, and therefore unsatisfactory character of many of the separate articles. This inconvenience has been obviated, as far as practicable, by distinct references from article to article: and several essential references omitted in the text—are indicated and supplied in the preliminary table of *Errata*.—I must allude however to two matters of greater consequence. It has not been possible to do justice—even comparative justice—to the Inquirers who have advanced and are rapidly advancing all departments of Physics. When the names of special Inquirers are mentioned, it is because of some very noticeable peculiarity in their relations to the subject. I should indeed have rejoiced had space permitted the introduction of Historical considerations, or of attempts to appreciate and offer tribute to the worth of Contemporaries. Neither have I desired to pronounce absolutely concerning those few physical questions now under active discussion. The writers of the articles connected with such questions may not have thought fit, in every instance, to conceal their *leanings;* but the intention has always been to state fairly the nature of the conflicting views.

J. P. N.

OBSERVATORY, 30*th January*, 1857.

PREFACE TO THE SECOND EDITION.

IT is necessary to state the circumstances under which the Second Edition of the CYCLOPÆDIA OF THE PHYSICAL SCIENCES is issued. The revised sheets were passing under the Author's hand when it was arrested by death in the midst of this, his latest effort; but such arrangements had been made for completing the work, that the Editors have had only difficulties of detail to surmount in carrying into execution what they knew to be the Author's intentions.

A few errors in the First Edition have been corrected, some omissions supplied, and several articles materially enlarged, to make room for the more recent results of experiment and speculation. Among other subjects which have received a more extended treatment, it is right to specify that of "Equations," which has been ably and fully expounded by MR. KER. The short notice of "Electrical Egg," which formerly appeared, has been replaced by an article of considerable length, in which the subsequent important discoveries under that head are discussed. PROFESSOR WILLIAM THOMSON has kindly revised and amplified his articles on "Electrometer," and the "Electric Telegraph." A valuable addition has been made to that on Frauenhofer's lines in the account which PROFESSOR STOKES has given of his discoveries with reference to the Invisible Spectrum.

Several entirely new articles have been added. PROFESSOR RANKINE has supplied those on the "Conservation of Force," and the "Skew Arch." We are indebted to DR. TAYLOR for an interesting essay on the "Atmospheres of the Planets;" to MR. HEMMING, of London, for a history of "Decimal Coinage;" and to MR. P. E. DOVE, for an article on "Rifles."

The whole Work has thus been considerably increased; and we have only to express a hope that enough has been done to render it yet more worthy of the support with which it was formerly favoured.

GLASGOW, *January*, 1860.

SUPPLEMENTARY REFERENCES.

[*The Reader is requested to insert, or notice, the following references from Text to Appendix.*]

Heating of Buildings, page 431. See Note concerning Ventilation in Appendix.

Imaginaries, page 458. See Imaginary Expressions in Appendix.

Light, Velocity of, page 506. Somewhat discrepant statements are found in various articles in reference to the actual velocity of light. The best *Astronomical* determination of it is still perhaps that by Struve, which gives it as 191,515 miles per *second.* The physical determination by Fizeau gives a higher number—nearer to the original one of Römer. But whatever the excellence of Fizeau's experiments, we must take our absolute determination from astronomical methods.

Stereoscope, page 812. See Appendix.

The following Articles will be found in the Appendix:—

Atmospheres of Planets.
Phase.
Phosphorescence.
Polynome.
Pulley.
Screw.
Steam.
Steam-Boiler.
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LIST OF MAPS AND PLATES.

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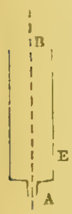
CYCLOPÆDIA

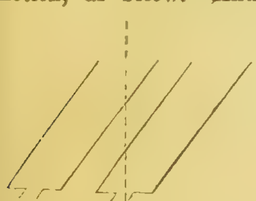
OF THE

PHYSICAL SCIENCES.

Aberration: a term used to designate three optical phenomena of much importance. They are:—

(1.) *Aberration of Light;* a very curious apparent displacement of the stars; one, to which any object exterior to the earth, and unaffected by the motion proper to that orb, would appear subject. Its nature may be illustrated, simply, as follows:—Imagine a shower of rain, falling, in the absence of wind, perpendicularly; every drop would evidently fall right through the axis of a tube held right up or in a perpendicular position, provided that tube were at rest. The vertical position of the tube would thus correspond with the direction of the rain, and indicate it. But if the tube were in motion, say in the direction of E, the rain drop entering at the centre of its top at B would no longer come out at the centre of the end at A: on the other hand, it would strike, or tend to strike, before issuing, on the *side* A B of the tube. In such a case, if an observer or an experimenter desired to catch, at the middle of the tube's end A, the drop which entered at the middle of the end B, he would require to *incline* the tube towards the direction of the tube's motion, as below. And the degree or amount of the requisite *inclination* would evidently depend on the swiftness of the tube's motion, and the swiftness of the fall of the drop. Now, should an observer in this case require to judge of the direction in which the drop falls, by the direction of the tube, he would evidently fall into great error should he suppose the direction of the *former* to be identical with the direction of the *latter;* or if he did not apply a *correction*, for the purpose of avoiding the error occasioned by the forward motion of the tube.—The circumstances we have just imagined, are precisely those under which we are constrained to look at any object unaffected by the motion of the earth. Whatever *light* is, it is propagated in straight lines with a definite velocity of 195,000 miles per *second;* and thus is in so far analogous to a falling rain. And when we look at a star, *we are not at rest.* The earth partakes of various motions,—the two leading ones known to us, being its *annual* motion in its planetary axis, and its *diurnal* rotation. The direction or inclination of the tube or telescope through which we look, therefore, can never correspond exactly with the direction of the light coming from any star or external object; and if we judged the direction or place of the latter to be identical with the direction of the former, we should go wrong. Further still, the error in question would be a *constant* and *uniform* one, if the motion of the earth *were always in the same direction;* but as we move in a *curve* around the sun, that direction must be always varying: *i. e.* the star, as determined by the position of the observer's telescope, must, during the course of the year, seem removed by small quantities towards *each side* of its *real place.* In fact, every star appears to describe a small annual ellipse around its true place; the major axis of which is $20\frac{1}{4}''$ of space. This ellipse, however, is described irregularly, owing to the varying velocities of the earth; but as its entire character is now ascertained within very small limits of error, the *correction* is easily made.—The *diurnal* aberration, or that depending on the *rotation* of the earth, is very small, and is also accurately determined.—It will readily occur to the thoughtful student, that other aberrations of this kind may exist,—depending, for instance, on that motion of translation through space to which we cannot doubt that the sun, with all his planets, is subject: the determination of the amount of that, and of its corresponding *aberration*, is one of those feats whose accomplishment will inspire and reward the ambition of future times.—The discovery at once of the fact and cause of the annual aberration is due to the illustrious Englishman, Bradley. The best practical discussion of it, we owe to a recent continental astronomer; one whose eminent taste and exact genius place





him by the side of Bradley — the lamented Bessel. See the *Tabulæ Regiomontanæ.*—— The explanation now given merely assumes the fundamental fact that light is transmitted in straight lines. But considerable difficulty arises so soon as we accept the modern physical theory of Light. Under the doctrine of Undulations, the direction of propagation, or the direction in which the luminous object as seen, is normal or perpendicular to the front of the wave; and the theoretical question is, in what manner a wave, propagated through a motionless Ether, is affected by the motion of a body such as the Earth, sweeping through that Ether? Modification clearly must take place, else there were no aberration whatever; but what is that modification? how can it be reconciled with the geometrical theory of the propagation by waves? The difficulty evidently consists in determining in what manner the direction of the normal of a wave can be affected by the motion of the body which that wave reaches. Professor Stokes has fully discussed this problem; nor can anything at present be added to his memoirs and those of Professor Powell. He supposes that the Earth and Planets *drag* along with them a portion of the Etherial Medium; so that while the Ether at their surfaces is at rest with regard to these surfaces, its motion gradually diminishes with distance, until it takes on the repose of the universal Ether beyond. All aberrations may be explained on this hypothesis—even that which may, in after ages, be found dependent on the motion of translation of the Sun. Other contributions have been made to this subject—for instance, those of M. Radické.—For Mr. Stokes's memoirs, see *Transactions of Cambridge Philosophical Society.*

(2.) *Aberration of Refrangibility.* White light is composed of a number of heterogeneous rays, of different colour. Their refrangibility is unequal, and, therefore, when they traverse a lenticular glass, they have on its axis as many sets of foci as there are colours. The images produced at these points are superimposed, more or less, and the edges fringed with indistinct colouring. The aberration holds, both in length and breadth, the less refrangible rays, unite at foci farther away than the others, and constitute the aberration in length. The larger cover to some extent those whose light is most powerful, producing an indistinctness of colouring, and sometimes a sort of variegated ring. This gives rise to the aberration in breadth. See ACHROMATIC TELESCOPE (*q. v.*)

(3.) *Aberration of Sphericity.* Consider a spherical cap, forming a concave spherical mirror. The opening of the mirror will be the angle formed by two radii from the centre of the sphere to two extremities of the diameter of the rim. Let us suppose a luminous point situated on the axis, that is, the line joining the centres of all the concentric circles of the cap, with the centre of the sphere of which it is a part. The rays emitted by this point will be reflected and reconcentrated at one or more points of this axis. If the angle which all the rays make with the axis be under 3° or 4°, they will be reflected upon one point or nearly so, and that point is called the focus. But if the range of the angles of emission be sensibly higher than this, there would be a *line of foci;* one point serving for reconcentrating all the rays upon *one* of the concentric circumferences of the cap; and the focus for *each* such circumference being nearer the cap, the greater that circumference is. We would have thus a *line of images* of the luminous point; and this effect is termed the *aberration of sphericity.* See TELESCOPE, REFLECTING.

Absorption: etymologically, a "drinking in;" technically, the physical assimilation of one substance—*ponderable* or *imponderable*, according to still current phraseology—by another substance; as if the first substance got retained among the pores of the second. There are three classes of phenomena to which the word *Absorption* is usually applied.

(1.) *Absorption of one Ponderable by another:*—for instance, *gases* by *water* or *charcoal.* See *Cyclopædia of Chemistry* passim. Absorptions of this description seem to have been no unimportant agencies, in modifying even the solid crust of our globe.—In reference to the absorption of gases by liquids,—by far the most important of all cases of absorption,—the recent researches of Bunsen leave little to be desired. The special phenomena to be observed and the problem to be realized are as follows:—"When a gas and a liquid, exercising no reciprocal chemical action, are placed in contact, a portion of the gas disappears, or is absorbed by the liquid. The quantity absorbed depends on the temperature of the two masses, and on the nature of the gas and of the liquid;—the problem is to determine the part played by each of these elements in the production of the observed effect." We subjoin Bunsen's leading results. (1.) Suppose, *first*, that a liquid of unit-volume, is in contact with an *indefinite* atmosphere of a determinate gas, it will always absorb the same quantity of the gas, provided the pressure and temperature remain the same. Taking the temperature and pressure at zero (temperature at 32° and pressure 30 inches), the numerical expression denoting the quantity absorbed is termed the *coefficient of absorption* of that gas by the liquid. If the temperature is constant, and w the weight of gas absorbed, under the zero pressure, the weight of the quantity absorbed under any other pressure, P, is found to be $w\ \frac{P}{30}$. Any other volume u of the same liquid will, under such circumstances, absorb $u \cdot w\ \frac{P}{30}$.—Again, let the indefinite atmosphere be composed of different

gases, whose coefficients of absorption are respectively α, α', α'', &c. If v, v', v'', represent the proportions of each gas in the mixture, it can be shown that αv, $\alpha' v'$, $\alpha'' v''$, &c., are the measures of the volumes of the different gases absorbed by the liquid under any given pressure; and, consequently, that in a unit-volume of the compound gas absorbed, the following quantities of the several gases will be found, viz.:—

$$\frac{\alpha v}{\alpha v + \alpha' v' + \alpha'' v'' +} \text{ \&c., } \frac{\alpha' v'}{\alpha v + \alpha' v' + \alpha'' v'' +} \text{ \&c.,}$$

(2.) *Secondly*, If the superposed atmosphere be *limited*, the pressure will change as absorption proceeds, and the problem becomes somewhat more complex. Suppose that the atmosphere at first, under the pressure P, contained v and v' volumes of two gases, whose coefficients of absorption at zero temperature are α and α', and let x and x' designate the unknown final pressures of the two elastic fluids. The volumes absorbed, reduced to the pressure P, will evidently be

$$\frac{u \alpha x}{P} \text{ and } \frac{u \alpha' x'}{P}.$$

At first, one of the gases under the pressure P occupied a volume equal to v. There remains then unabsorbed a quantity capable of occupying, under the same pressure, a volume expressed by $v - \frac{u \alpha x}{P}$; and as this gas really occupies a volume $v + v'$ under the pressure x, we derive from the law of Mariotte the equation

$$v - \frac{u \alpha x}{P} = \frac{(v + v')\, x}{P}; \text{ whence,}$$

$$x = \frac{v\, P}{v + v' + u \alpha}.$$

The quantity of this gas not absorbed, therefore, is

$$v \cdot \frac{v + v'}{v + v' + u \alpha}.$$

And so of the other gas, or of any element of a more complex combination.—(3.) Bunsen next examines the effect of the variation of temperature, and, after elaborate investigation, he constructs a table, of which the following is an extract. It gives the coefficient of absorption of the gases by water under pressure 30 inches, and at any temperature t, Centigrade:—

Nitrogen, ...	$c = 0{,}020346 - 0{,}00053887\, t + 0{,}000011156\, t^2$
Hydrogen, ...	$c = 0{,}0193$
Ethyle gas, ...	$c = 0{,}031474 - 0{,}0010449\, t + 0{,}000015066\, t^2$
Gas from a marsh, ...	$c = 0{,}05449 - 0{,}0011807\, t + 0{,}000010278\, t^2$
Methyle gas, ...	$c = 0{,}0871 - 0{,}0033242\, t + 0{,}0000603\, t^2$
Carbonic oxide, ...	$c = 0{,}032874 - 0{,}00081632\, t + 0{,}000016421\, t^2$
Olefiant gas, ...	$c = 0{,}25629 - 0{,}00913631\, t + 0{,}000188108\, t^2$
Carbonic acid gas, ...	$c = 1{,}7967 - 0{,}07761\, t + 0{,}0016424\, t^2$

After exposing the theory of the subject in the general manner indicated above, Bunsen proceeds to develop certain valuable applications of his formula. Referring for his processes—which are exceedingly simple—to his second memoir, in the *Philosophical Magazine* for 1855, vol. i., we shall here make room for the more important of his results.—(a), Supposing that in the case of a mixed gas, the following quantities are known, viz.:—α, the absorption-coefficient of the first gas; β, the absorption-coefficient of the second; V the common volume of both gases before absorption under pressure P; V_1 the residual volume after the absorption under pressure P_1; and, lastly, the volume h of the absorbing water: then if

$$W = V\,P$$
$$A = (V_1 + \alpha\, h)\, P_1$$
$$B = (V_1 + \beta\, h)\, P_1$$

we have

$$\frac{x}{x + y} = \frac{W - B}{A - B} \cdot \frac{A}{W}$$

and

$$\frac{y}{x + y} = \frac{A - W}{A - B} \cdot \frac{B}{W},$$

from which x and y, the original volumes of the two gases, reduced to pressure 1, may be determined. In the memoir referred to, the formula is tried on two unknown mixtures of hydrogen and carbonic acid gas; and for the sake of comparison, the same gas was analyzed by the ordinary endiometric methods. The following are the results: The first experiment gave—

	Hydrogen.	Carbonic Acid.
1. By endiometry, . .	0·9246	0·0754
2. By absorptiometry, .	0·9207	0·0793

By the second experiment on other proportions of the same gases, Bunsen obtained—

	Hydrogen.	Carbonic Acid.
1. By endiometry, .	26·81	73·19
2. By absorptiometry, .	26·67	73 33

The correspondence in both cases is certainly sufficiently remarkable to establish the value of absorptiometry.—(b), But this mode of analysis may go much farther; it may determine also the *nature* of the component parts of an unknown gas, as well as their proportions in the compound, when once the absorption-coefficients of all the gases are determined. Bunsen justly remarks that any general re-agent, to distinguish between the constituents of a gaseous mixture, has hitherto been wanting. The quantitative composition of a gas

as determined by endiometry, is mixed up with hypotheses as to its qualitative constitution. "If," he says, "analysis shows the presence of a mixture of marsh gas and hydrogen, it is uncertain whether we are not experimenting upon mixtures of methyle and hydrogen, or of methyle, marsh gas, and hydrogen." It is, however, easy, by means of the law of absorption, to remove these doubts, for "the absorption-coefficients serve as re-agents which cannot be found in other modes of gas analysis—presenting the peculiarity that they not only show the qualitative, but, at the same time, the quantitative composition of the gas. Our author, indeed, develops easy formulæ, by which, should an unknown gas be a mixture of an unknown volume x with another volume y of another unknown gas, it is possible, after three absorptiometric experiments, to determine, *first*, what gases are present in the mixture, and, *secondly*, in what proportion they are present.—(*c*), Bunsen next lays down rules for determining, in the most precise manner, the alteration which a mixture of gases undergoes by contact with water; and he passes to consideration of those highly interesting phenomena which accompany the evolution of gas in mineral springs,—phenomena that cannot be fully understood, unless by help of the law of absorption. Passing over more delicate inquiries, it is plain, for instance, that in all springs that contain carbonic acid gas alone in solution (and these are by far the most common), the limit or maximum of the gas depends definitely on the temperature of the spring, the depth of its shaft, and the height of the spring above the sea. The pretensions of all springs can thus be rigorously tested. Sigwart, for instance, gave out that the "Fürsten Quelle" in Imnau contained 2,500 cubic centimetres in the litre; the maximum is only 1373·2. By the same unerring guide, we are led to determine the maximum amount of carbonic acid gas that can be carried down from the atmosphere to the surface of the earth, as well as the mode of its distribution over the different zones of terrestrial climate. This quantity diminishes as the temperature rises; and therein seems to appear an effort of nature to assist the tardy vegetation of the cold north by a richer nourishment, and by a sparing supply to keep the luxurious growth of the tropics within limits.—Bunsen proceeds with the rationale of the great productiveness of mould, rich in summer, and of the advantages derived by the agriculturist from breaking up his land. He promises to extend his researches farther,—to scrutinize the phenomena of the oceanic atmosphere and of the absorption of air by the blood.

(2.) *Absorption of Caloric or Heat.* The technical terms of the sciences generally originate in theories or hypotheses: they express not only the fact but some supposed explanation of it. But although it is often convenient to retain them after the discredit of the speculation in which they originated, the student requires to be on the watch lest they carry into his mind somewhat of those exploded notions, and thus affect injuriously his train of thinking.—*Absorption of Heat* meant originally as follows:—A ray or beam of heat falling on any substance, part of it enters that substance and warms it, or is *absorbed;* while another part is rejected or reflected by that substance, and passes off through space. The class of inquiries for which it really stands, on the other hand, may be defined thus: Different bodies exposed to the same heating influence become heated with various facilities,—is there any *law* regarding such facilities, and is that law connected with other relations of these bodies to the phenomena of heat? Two distinct inquiries demand attention,—the *first*, omitting reference to the opacity or transparency of bodies with regard to heat—their *athermancy* and *diathermancy;* and the *second* relating especially to these latter properties.—*First.* The general *absorbing*, or, as it has also been termed, the *admissive* power of bodies with regard to heat, has been the subject of laborious investigation by Leslie, Dulong and Petit, Melloni, and more recently by Provostsaye and Desains. It seems to depend mainly on two elements,—the nature of the *body's surface*, and the nature of the *heating source.* 1. The nature and amount of the influence of body's surface, on its capability of being warmed, may be inferred from the following results, obtained chiefly by Provostsaye and Desains. The numbers represent the quantities absorbed of 100 incident rays of heat:

Lamp black	100
Black lead	100
Writing paper	98
Common glass	90
China ink	85
Rock salt	72
Silvered glass	27
Mercury	23
Polished iron	23
—— zinc	19
—— steel	17
Platina, slightly polished	24
—— in thin leaves	17
Tin	14
Mirror metal, polished	14
Brass, highly polished	7
Copper	7
Gold	5
Silver, polished	3

Numerical determinations of this kind are of high importance in the arts, and of easy application. The following general conclusion is, however, of higher interest. A heated body communicates heat in two ways,—through contact, or by that mode of influence from a distance, which has been called *radiation* or *emission.* Now, the *absorbing* or *admissive* quality of bodies, and their

radiating or *emissive* faculty, are *directly proportional;* a body when heated radiates with an energy exactly measurable by its facility in absorbing; a conclusion indicated by Leslie and established for all circumstances and all temperatures by Dulong and Petit. Experimental determinations of absorptive power, are thus determinations of the emissive power also.—2. This absorbing faculty of bodies varies to some extent with the nature and temperature of the source of heat; so that no numerical evaluation is precisely accurate unless in reference to one specific source of heat. Science is indebted to Melloni for the first exact presentation of this curious phenomenon. Its nature will appear in the following table; where the numbers in the vertical columns represent the relative absorbing powers of the substances named, when subjected to the specified sources of heat:—

Surfaces.	Lamp.	Incandescent Platinum.	Copper at 750 dgs	Cube at 212 dgs.
Lamp Black,....	100	100	100	100
Indian Ink,.....	96	95	87	85
Black Lead, ...	53	56	89	100
Isinglass,.......	52	54	64	91
Rock Salt,......	43	47	70	72
Polished Metal,..	14	13·5	13	13

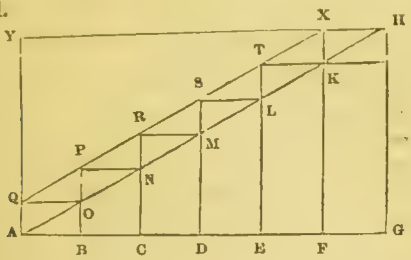
The variations are sufficiently striking. Melloni takes a theoretical view of the phenomena, and is attracted by analogies drawn from prevalent theories of light. Just as a ray of white light is a sheaf of various rays separable by means of their different refrangibilities—(see SPECTRUM) —so he imagines it with a ray of ordinary heat; each ray in the sheaf having its peculiar relation to the body on which it impinges. And as the heat issuing from different sources, and with different temperatures, may contain various proportions of these simple rays, the apparent anomalies expressed in the foregoing horizontal lines may thus seem explicable. It cannot, however, be conceded that Melloni's remarkable experimental results establish the truth of his theory.—*Secondly.* The term *absorption* has likewise been generally applied to that loss of radiant heat which occurs on its passing through bodies,—akin to the apparent loss of light on its passage through semi-opaque and coloured substances. Melloni has recently given to this class of phenomena the much more correct name of *athermancy* and *diathermancy;* nevertheless, we shall describe them under the old and better known terms. The following are the leading facts at present established. The quality of bodies which causes their absorption of heat, has no relation to transparency or opacity; that is, to the quality which causes their transmission or absorption of radiant light. For instance, rock salt has no power of absorption, being purely diathermanous, while a screen of alum absorbs a very large portion of the ray, especially if the source of heat be of low temperature; black glass, too, and discs of quartz, would, with smoke, transmit no light, while they are much less absorbent of heat than alum. No body yet known is so perfectly diathermanous as rock salt; it absorbs no sensible portion of radiant heat, although the plate be an inch thick: all other substances absorb more or less of the transmitted beam; and the absorbed proportion varies with the thickness of the plates and the source of heat, according to very complex laws. Generally speaking, rays proceeding from sources of a low temperature, contain a large proportion of absorbable rays; and in regard to the thickness of the plate, the quantity absorbed increases rapidly as the thickness increases. To this increase, however, there is a limit; for after the plate attains a certain thickness, its power to absorb seems incapable of further augmentation,—as if—to recur to the words of our former theory —the sheaf of rays proceeding from each source were composed of elements variously absorbable, some being completely absorbed by plates of slight thickness, others yielding only to greater thicknesses, while the remainder resist all absorption. There is, in fact, a striking analogy between the phenomena of absorption of heat, when passing through media, not perfectly athermanous, and the transmission of radiant light through coloured glasses. For instance, light, which has passed through a red glass, is not further absorbable by red glasses, while a disc of violet glass will absorb or destroy it all; so, while additional thicknesses of alum absorb no more heat, the interposition of a thin plate of green glass will destroy the whole of the ray transmitted through a thin plate of alum;—the effects of the combination, or superposition of different screens, being, in either case, independent of the order of superposition. — Compare DIATHERMANISM, DIFFUSION, RADIATION, REFLECTION. Reference is also made to *Melloni's Remarkable Papers*, reprinted in *Taylor's Scientific Memoirs.*

(3.) *Absorption of Light.*—The subjects comprehended under the foregoing term, are the obscurest in all Optics. They have not been fully explored by experiment, and their theory is far from satisfactory. They may be ranged under three heads.—*First.* The *natural colour of bodies*, has until lately been attributed to an absorption. The white or natural solar ray being shown by the prism to be a sheaf of rays of different colours, it was supposed that every material substance has the faculty to absorb or appropriate part of the natural ray, and to reject or reflect the residuum; and, as this reflected residuum might consist of one primary colour, or any combination of primary colours, the notion offered a *prima facie* highly probable account of that infinite luxuriance of hue which distinguishes external nature. But the speculation will not stand the test of inquiry. Analyzed by the polariscope, the coloured light, reaching us

from material bodies, turns out not to be *reflected* but *refracted* light,—emanating, therefore, from the bodies' *interior*. And, still further, it is maintained, on grounds far from untenable, that the white or ordinary ray falling on any body which appears coloured, is really *reflected white*; so that there can be no decomposition of the ray, or one set of colours *absorbed* and another set *reflected*. The whole of this curious subject is discussed under COLOURS. It is certainly not proved as yet that a *bona fide* absorption has aught to do with the phenomenon.—*Secondly*. The term *transparency* is only relative. No body is so transparent that light passing through it, shall emerge, of the same quantity and quality, as when it entered. And the loss or modification which the ray undergoes is, in common language, termed *absorption*. One instance will suffice to illustrate the case. Through a piece of polished smalt blue glass, the colour transmitted is mainly blue. But to define the effects of transmission, it is needful to look not at the compound white light, but at the *spectrum*, through such a medium. If the glass be of extreme thinness, all the colours are seen, although the eye can recognize differences in their relative intensity. But when the thickness of the plate reaches the twentieth of an inch, we discern very striking and singular appearances. The spectrum seems no longer *continuous*, but consists of several detached portions, separated by broad black intervals; the rays filling these spaces in the perfect spectrum being in some way extinguished. If the thickness of the plate is increased, the black spaces become broader, till at length all the colours between extreme red and extreme violet are wholly destroyed. If these semitransparent media were perfectly transparent with regard to one primitive colour, and opaque to all others, the resulting phenomena would be simple; but as each thin medium is very imperfectly transparent with regard to any colour, and variously so with regard to the different colours, the phenomena of absorption are most complex; the same medium, when altered in thickness, appearing to transmit quite different colours. Take, for instance, a thin hollow glass wedge, and fill it with muriate of chromium; white paper seen through the thin edge will appear of a fine green, but as we look through a greater thickness, the green tint grows livid, and passes through a brownish hue to a deep blood-red. No one has contributed so much to the facts of this subject as Sir David Brewster. We owe to him especially the discovery of those remarkable powers of the nitrous oxide and other gases, to which we shall afterwards refer. Compare SPECTRUM.—*Thirdly*. The remarkable discovery by Frauenhofer of dark lines in the solar spectrum, naturally originated much novel and curious speculation regarding light. The phenomenon itself is fully described under FRAUENHOFER'S LINES, as well as the effect of sundry gases just alluded to. Suffice it here, that its similarity to the so-called absorptive effects of *coloured glasses*, suggested at an early period of these inquiries a corresponding explanation: the absent rays were spoken of as *absorbed* by the gases in the *latter* case, in the *former* by the *atmospheres* of the sun and other stars, from which the defective light issued. This theory naturally enough led to a strict examination of the solar ray during an annular eclipse; for then the light reaching the earth pierces through an unwonted thickness of our luminary's atmosphere: but as no unusual defect is discernible under such circumstances, no confirmation of the theory of absorption has hence been derived. In the opinion of many physicists, a better explanation of the entire class of phenomena can be drawn, on the basis of the *Doctrine of Undulations*, from what is termed *Interference*. We shall discuss this fully under INTERFERENCE and COLOURS, *q. v.* The student is referred further to the numerous, and most interesting researches of Sir David Brewster.

Accelerated Motion. The simplest mode of motion, as we conceive it, is when the moving body passes through equal spaces in equal times—a mode to which the name of *uniform motion* is rightly appropriated. It is evident, however, that many other modes are conceivable; for instance, the body may move quicker and quicker the longer its motion is sustained—in other words, it may move over a greater space during the second *second* than it did during the first—over a greater space during the third *second* than it did during the second, and so on. Motion of this description is termed *Accelerated Motion*. It is clear, also, that an infinite variety of accelerations is possible, inasmuch as we can conceive an infinite variety in the *laws* according to which the *acceleration* takes place; that is to say, during any *second* of time, the swiftness may be supposed increased only *as much* as during any other *second*, or, that increase may follow any other law. The case is much the simplest when the acceleration in a given time is *uniform*—the Motion being hence termed *uniformly accelerated motion*; and this is the only mode of accelerated motion presented to us by the simpler phenomena of the universe. When a heavy body falls from a height it is found to traverse 16 1-12th feet during the first *second* of its fall, and at the end of that *second* it has acquired a velocity capable of carrying it through 2 $\times$ 16 1-12th feet, or 32 1-6th feet during the next *second*. During that second, however, it is found to move through 32 1-6th + 16 1-12th feet, or 48 1-4th feet, so that it has been again *accelerated* by the same quantity as before, viz., 16 1-12th feet, and the same acceleration holds during every successive *second*. The *laws* of such a motion are easily determined. In the first place, geometrically. The motion which most closely resembles *uniformly accelerated motion* would be one, where

for very small portions of time, the body in question moves *uniformly;* and at the expiry of each of these portions receives a uniform increase of speed.



Suppose, for example, the line A G, representing the time in which a body moves, to be divided into smaller equal portions at B, C, D, E, F. Suppose, further, that for the whole space of time represented by A B, the body moves with the velocity B O (perpendicular to A B); and that at that moment it receives an increase of speed sufficient to make it move for the next interval with the velocity represented by N C. At the termination of this again, it will be set to move over the third interval with a velocity M D, and so on, the lines B O, N C, M D, L E, F K, each perpendicular to A G, and differing each from the one immediately preceding by an equal amount. The geometrician will readily see that the summits A, O, N, M, L, K, H will be along one straight line. Those who are not acquainted with geometry may readily convince themselves by drawing the figure accurately. Since, then, we have here a case of motion very analogous to that whose *laws* we are seeking, let us observe the results. In all *uniform* motion, the space described is measured by the product of the number of units of time by the number of units of speed. If, for example, a body move uniformly for nine seconds, with a velocity such that it will move through three feet each second, it will have moved through 27 feet at the end of its course. Suppose, then, A B to be the number of seconds, and B O the velocity, the space moved through, would be measured by the product of A B and B O. But, this is also the measure of the space covered by a rectangle described with these, as adjacent sides. Hence, the number expressing the area of the rectangle may be taken as the number expressing the space of motion. Let us complete, then, the rectangles Q B, P C, R D, S E, T F, X G, Y G. Then, according to this law of uniform motion, the spaces described in the first, second, third, &c., portions of time will be measured by the numbers expressing the spaces Q B, P C, R D, S E, T F, X G, which together make up A H G, together with series of small triangles A Q O, P O N, &c. Now, the geometrician will again see that Q, P, R, &c., lie upon one straight line, and that these triangles are in consequence less when put together than the one figure A Q X H. Hence, the space described would be greater than the space A H G, and less than this in addition to A Q X H. So much, then, for this case of motion. It, however, is not exactly the motion we wish to know about; but if we suppose the intervals of the increasing of speed smaller, we should have a closer resemblance to it. If the intervals were smaller, we would have a figure similar in every respect to the one given; the only difference being, that the breadth of A Q X H would be less. If we go on, continually making the intervals of acceleration smaller and smaller, we should approach infinitely near the case whose laws we are in search of. The results at which we would arrive would of course approach the result in that case; and if we imagine the intervals infinitely small, we may suppose ourselves actually to have arrived at a *uniformly accelerated motion.* The result in this ultimate case would be, that the space described would be measured by something between A H G and A H G added to a figure analogous to A Q X H. We easily see, however, that this figure constantly diminishes as BO diminishes, and that it again becomes smaller with the interval A B. Hence, where A B is infinitely small, A Q X H would become infinitely small also, and the space could only differ by an infinitely small quantity from A G H. The space described is thus measured by A H G virtually. The space, therefore, which is described by a body moving with a uniformly accelerated motion, from an initial velocity = o, to any ultimate velocity, will be *half* that described in the same time by a body moving uniformly with the velocity to which it attains. The velocity (represented by the perpendiculars B O, N C, &c., at the points indicating the time) is evidently proportional to the time—that is, the velocity acquired at the end of four seconds will be four times that acquired at the end of one second. Finally, the space described in the time represented by A D, will bear to that described in the time measured by A G, the proportion of $AD^2 : AG^2$. This is evident geometrically; but the ordinary reader will readily comprehend it. In this case the length A D is half of A G, and the height D M is evidently half of G H, so that the space A D M will be $\frac{1}{2}$ of $\frac{1}{2}$ of A G H, or $\frac{1}{4}$ of it.—The method of the differential calculus is much more simple, and produces the same results. We shall not enter at all into an exposition of it, but merely give the steps. The velocity of any body moving irregularly will be found by dividing the infinitely small space traversed at the moment for which we wish to know the velocity, by the infinitely small time in which it is described. Now, in uniformly accelerated motion, this velocity will be $= a+gt$, where a is the velocity at the commencement, if any, g the velocity acquired in each second, and t the number of seconds after the commencement at which we note the motion. Hence $\frac{ds}{dt} = a + gt \therefore S = at + \frac{gt^2}{2} + C$, and if we commence to calculate motion from the point of starting, $S = at + \frac{gt^2}{2}$. We have included in these latter formulæ a case which is

not at first the same as that which we have hitherto considered. It is that of a body which commences to move with a velocity of its own, and which retains that velocity according to the law of inertia. In this case we have simply to cónsider what space would be passed through by the body due to the uniform motion which this force impresses upon it, and add it to the motion which the force of gravity has impressed upon it. —*Retarding forces* are also frequently spoken of. When we have a body in motion against a force constantly pulling it backward, we have to consider how far the body would move forward in consequence of the motion which the primitive force has impressed on it, and to subtract from it the effect of the retarding force, calculated exactly as above, as a *uniformly accelerating force* acting in the opposite direction.—The laws of *uniformly accelerated motion* are so simple, that natural philosophers have taken it, along with *uniform motion*, as the two standard species of motion. Wherever they wish to examine the consequences of any complex law of motion, they consider that motion as compounded, for an instant, of one which is perfectly uniform depending on the velocity at that instant, and of one uniformly accelerated. The details of the process of decomposition of motion belong to the higher dynamics, and we shall not enter into them.

Accelerating Force (technical). That force which is applied in any given case to the unit of mass is called the accelerating force. Its value depends on the proportion of an infinitely small increment of the velocity of any moment, to an infinitely small increment of the time $\left(\frac{dv}{dt}\right)$. This expression is quite general, and applies also to the case of a retarding force, which is considered as a negative accelerating force, and is measured by the proportion of the increment of the velocity, (which in this case becomes negative, indicating a decrement), to the increment of the time.

Accidental Colours. See Colours.

Accidental Point. See Vanishing Point.

Achromatic. In the article on the aberration of refrangibility, we have indicated that the different coloured rays of which white light is composed are differently refrangible. When, therefore, light direct, or reflected, passes through media, and becomes refracted before reaching the eye, the rays will be separated one from the other, and the image of the object become confused. Achromatism compensates for this, by making the luminous rays pass through different diaphanous substances, possessing different refracting and dispersive powers. By dispersion we mean—that there is a difference between the *indices of refraction* (*q.v.*) of the component colours of a ray of decomposed light, when passing through refracting media. The dispersive power of any substance is measured by the quotient of its dispersion by the mean index of refraction (which depends on the mean light of the spectrum), diminished by unity. There are substances possessing unequal refractive powers, with equal dispersive powers. The accomplishment of the aim of achromatism was long considered impossible. Newton himself thought that refracted light must always be decomposed. Euler pointed out that in the human eye—which is only an optical instrument—this difficulty was in fact overcome—and suggested the possibility of imitating the manner in which it was so. Hall, in 1733, constructed achromatic lenses, but did not publish his discovery. Dollond, however, arrived at the same discovery in 1757, independently, and published it. He combined flint glass, or artificial crystal, in the arrangement of his lenses, with crown glass, or good ordinary window glass; and he found that by a suitable curvature, given to his object glasses, the image of an object might be seen through them distinct and colourless. It is easy to calculate what relation ought to subsist between the refracting angles of two substances, in order that, when used together, they may neutralize the natural dispersion of the various coloured rays. Prisms, which transmit light without colouring it, are called achromatic; and lenses are so, when they form in their foci colourless images. Achromatism is, however, not by any means complete. It has become sufficient, in Art, for practical purposes; but it is still dangerous for the scientific observer to trust implicitly to it. The proportion of the partial dispersions of the two substances used is not constant through all their extent; and the indices of dispersion vary from one colour to another in the same luminous ray. The nearest approach to achromatic perfection, is made, when we employ a considerable number of prisms, of different substances.

Achronical (α, *not*, χρόνος, *time*). Denotes the point of time of the sun's rising or setting; the commencing, or terminating, of a reckoning of time. In modern astronomy it is confined to the time of the sun's setting. A star, therefore, is said to rise or set achronically, when it rises or sets, at the moment of sunset. The word used to denote the moment of the sun's rising is *cosmical*; and the star is said to rise or set *cosmically*, (κόσμος, order, harmony), when it does so at the moment of the sun's rising. When, again, the exact moments of the rising or setting of the star and sun are not identical, though nearly the same, the star is said to rise or set *heliacally* (ἥλιος, the sun).

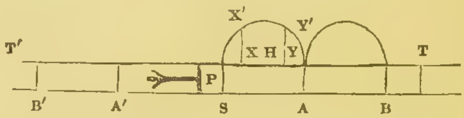
Acoustics,—the physical science of Sound. As in every similar case, the *sensation* of sound must be separated from its material or physical *antecedent* or *cause*. These two phenomena, although indissolubly connected, bear no resemblance to each other: a sound, as we generally understand it, has no more likeness to the material changes that necessarily precede the sensation, than a colour has to the vibrations of the ether or molecules that are the antecedents of

every sensation connected with sight. Now, it is with these material antecedents that physical science alone concerns itself. The fundamental proposition of Acoustics is this,—every sensation of sound is preceded by the vibrations of some external material body:—without this, as an antecedent, no sound is ever heard. The subject therefore may be divided into three chief parts:—I. A study of the general relations between the vibrations of external bodies and the sounds which we hear as their consequences. II. A study of the mode in which a knowledge or sense of such vibration is conducted to the human ear. And, III. A study of the different or special kinds of vibration which are effective in producing sounds. We shall take up these points in their order.

I. The General Relations between Sounds and the Vibrations of External Bodies.

1. Let us first endeavour to form a distinct idea of what is meant by the vibration of an elastic body,—an object that may be accomplished by aid of a very simple illustration. Suppose a piece of thin elastic metal, A *s*—say a piece of the main-spring of a watch, having one end fixed at A, while the other at *s* is free. By a pressure of the finger bend the spring, so that the end *s* shall reach the position *e*, and then by removal of the finger set it free. The spring will rebound toward *s*, but it will not attain rest on reaching its original position, A *s*. The velocity attained during the rebound will carry it to A *e′*, corresponding on the other side with A *e*. From this new position it will return with an equal rebound; and, precisely like the pendulum, it would, were its elasticity perfect and if no external obstruction existed, oscillate or vibrate for ever between these opposite positions. The time occupied by the spring between its quitting one position A *e*, and again returning to it, is termed the period of one oscillation or vibration; and it has been shown to be a law, that if the vibrations are small, that is, if *e* is not far from *s*, this period—(*all other things remaining the same*)—is constant; in other words, these vibrations are *isochronous*. Now, what we have imagined to happen in the case of this spring virtually occurs whenever any elastic body is caused to vibrate,—be that body a solid mass, a string in a state of tension, or an elastic fluid. When a bell tolls, for instance, each molecule of it is passing to and fro, between two such positions as *e*, *e′*; and the important law holds here also, as indeed in every other case, that under the same circumstances, the vibrations are *isochronous*.—It will be convenient that we observe also in this place, how each vibration of an external body, be it what it may, must originate

or create an undulation in the surrounding air, which undulation will form itself into a wave, proceeding outwards on all sides from the place of that primary vibration. Suppose, for instance, that P is the vibrating molecule—that is to say, a molecule alternately advancing and receding; and for the sake of simplicity, let us liken it to a piston, moving in the long open tube T T′, through the space P S, and again backwards to P. (The linear dimensions of the piston must be considered as very small in relation to the length of the tube.) Had the air within the tube been rigid, the forward motion of P to S would of course have merely forced a corresponding quantity of that air out of the tube at the end T. But the atmosphere being elastic or compressible, the impulse given by the motion of P will *compress*, in the first place, that portion of it next P. Let



this effect be supposed to have reached A, when the piston stops at S;—the air formerly occupying the space P A will now be compressed within the smaller space S A. The effect of the action of P will just have begun, or rather be on the point of beginning, at the vertical layer or stratum A Y, and at the same time it will at the same moment be just ceasing at the vertical stratum at S—the pressure of P then ceasing. The molecules in the vertical strata between S and A will, however, be subject to compression, and in consequence to motion; and near the middle of that line they will be subject to the greatest compression, and therefore to the greatest velocity. Their state may be represented to the eye, if we raise ordinates X X′, Y Y′, &c., representing the degrees of compression at X Y, &c., and connect their extremities by a curve.—Should the piston remain at S, the force accumulated by compression within the space of air S Y, will discharge itself on the next layer A B, and produce there a state of things exactly resembling its own,—in other words, it will propagate itself onwards, giving rise to a new or second undulation, which in its turn must generate a third, more advanced still; so that the primary disturbance, caused by the advance of P, must result in the creation and propagation of a wave that will advance, as we shall see hereafter, with a definite velocity on all sides through the whole atmosphere. It is easy to see that during the subsistence of the process now described, another—in so far opposite in character—has been occurring on the other side of the piston P. At that place the air occupying a space sensibly equal to S A, must have been rarified; and at the close of the second interval this portion will return to rest, while the portion occupying A′ B′ will come to be in motion. Aerial pulses will thus be produced on every side. Since small

vibrations are *isochronous*, these pulses or waves must also be isochronous, and of a length determined by the length of the vibrations of the sonorous body. The pulses in question are commonly named pulses or waves of sound.

2. Seeing, then, that the invariable antecedent of the sensation of sound is a body in a state of vibration, communicating waves or vibrations corresponding to its own to the atmosphere, and thus diffusing its effects, we easily recognize it as the *general* problem of acoustics, to ascertain what special effects belong to the *nature* of the body vibrating; to the *energy* or *force* with which its molecules oscillate; and to the *rapidity* of these oscillations.—(*a.*) As to the influence of the nature of the body vibrating, no satisfactory physical solution has yet appeared. Two different musical instruments, communicating to the air the same number of vibrations in the same time —each vibration being of equal strength—do indeed affect us with sensations very different. Who does not recognize the difference, for instance, between a note of a flute and the same note of equal loudness produced by the violin? And yet in all known physical attributes the aerial oscillations originating in the action of these instruments correspond to a nicety. The full solution of the difficulty may depend on physiology; nevertheless, there must be some decisive physical cause antecedent to the difference of effect on the sensorium.—(*b.*) The energy or *amplitude* of the primary impulse or vibration, or the contrary, exercises an influence that is intelligible enough. It is clear that this *amplitude* has nothing to do with *rapidity*:—the mainspring of our first illustration, for instance, might have been made to deviate much farther from A *s* than A *e* is, in which case the sweep or amplitude of its oscillations would have been larger, although *their number in a given time would not have been augmented.* Now, on this amplitude, that attribute of a sound which we term its *loudness*, depends. A note of a chamber organ differs, in this respect alone, from the same note rolled out in York Minster, or by the organ in St. Paul's.—(*c.*) There remains the question as to the *rapidity of vibration*,—a question of profoundest interest, one concerning which physics can do but a part, for we soon enter the region of physiological psychology. It is on this primary physical element that the characteristic depends which induces us to divide sounds into *grave* and *acute.* The *first* thing that strikes one when entering on this curious subject is the immense compass of the human ear, or rather of our sense of hearing. Savart's experiments demonstrate that sounds are audible, although so extremely *grave*, that they originate in vibrations, seven or eight of which only, occupy a second of time; while, on the other hand, by the same surprising faculty, we can detect the acute note proceeding from a vibrating body giving forth twenty-four thousand waves in the same small portion of time. Despretz has recently gone much farther, —he thinks we can detect vibrations so rapid, that no fewer than 36,850 are executed in a second. The rapidity of vibrations like these is indeed infinitesimal, if compared with that of the undulations which seem to give rise to the phenomena of vision. At the same time the compass of the eye is small compared with that of the ear: the extremes in the former case being in the proportion of 458 to 727, while in that of the latter they are, to take the most unfavourable statement, as *one* to *three thousand.* And yet who shall say that our human ear recognizes all possible or even all existing sounds? Multitudes innumerable of the voices of this unfathomed universe may pass us, never to be heard;—our portion of it, in so far as sense is concerned, is probably that of the creature of an hour—not much greater than what belongs to the Ephemeron.—Passing, however, from the contemplation of *limits*, let us ask next whether sounds produced within those limits are susceptible of satisfactory classification, or can be supposed to be rationally connected with each other? The reason or ground of the classification now to be explained undoubtedly belongs to the necessities of the human sensorium, or to demands made by the constitution of our perceptive faculty: an analysis of those grounds, and the discussion of their finer relations, constitutes the Science of Music. The physical laws or characteristics of the subject may be simply, as well as very briefly explained.—(1.) A compass of vibrations, in which any velocity or rapidity is taken as the starting point, and which includes all vibrations up to those having a rapidity double that of this initial or fundamental one, is called an *octave.* There is of course no limit to the number of octaves, ascending and descending, except the foregoing limits of audibility. But their construction, or their comparability, in the musical language of different nations or composers, must evidently depend on an agreement as to a diapason or fundamental note. Singularly enough, this has not hitherto been a thoroughly settled point. Savart found that the fundamental *la* springs from 440 complete vibrations in a second; but we find that the actual *la's* of the grand opera of Paris, of the Italian theatre, and of the theatre of Berlin, originate respectively in 433, 434, and 441 vibrations. Accurate science may thus correct and aid even the highest æsthetics, —*Mundum regunt numeri!*—(2.) All sounds issuing from vibrations, whose rapidity is related according to the natural numbers 1, 2, 3, 4, &c., are termed *harmonic* sounds; in other words, they may be sounded together inoffensively, and even pleasingly. Savart considers that he discerned a physical reason for this. When a cord is set in vibration it appears that, besides the fundamental sound, all the sounds corresponding to these numbers are audible at the same time; that is to say, mingling with the full vibration

of the cord, there are separate and secondary vibrations round the various points in which the cord would be cut, were we to divide it into two, three, and four parts successively. The figure below will illustrate this condition of complex vibration for the ratios 1, 2, and 3.—(3.) The

octave is divided, however, into other intervals, and these divisions constitute what is termed the *gamut*. These divisions technically are—

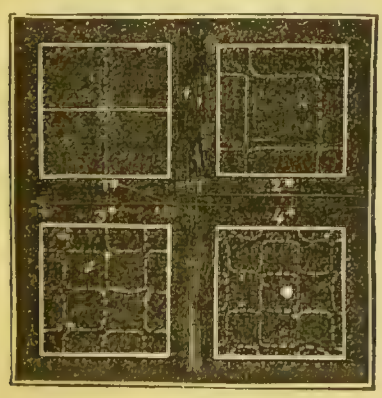
ut,	rè,	mi,	fa,	sol,	la,	si,	ut,
1,	$\frac{9}{8}$,	$\frac{5}{4}$,	$\frac{4}{3}$,	$\frac{3}{2}$,	$\frac{5}{3}$,	$\frac{15}{8}$,	2,

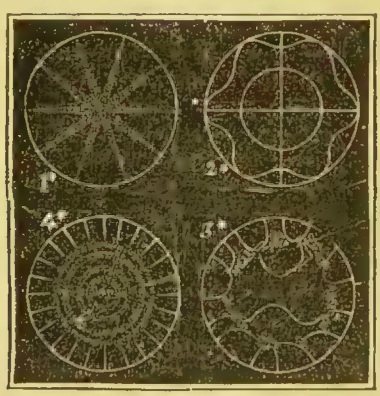
—the numbers in the second line indicating the vibrations of each note that occur in the same time. The same intervals belong to every octave —referring them to the primary *ut;* that is to say, if the primary *ut* of an octave be 4 instead of 1, each of the foregoing numbers or ratios must be multiplied by 4. There is another gamut used in music for special objects—called the minor gamut; its intervals are—

ut,	rè,	mi ♭,	fa,	sol,	la ♭,	si ♭,	ut,
1,	$\frac{9}{8}$,	$\frac{6}{5}$,	$\frac{4}{3}$,	$\frac{3}{2}$,	$\frac{8}{5}$,	$\frac{9}{5}$	2,

Airs written in gamut minor, are sad and depressed, and are easily distinguished from music belonging to gamut major.—Why the ear will recognize no sounds produced by other sets of vibrations, as *musical*, the Science of Acoustics cannot explain, nor does it attempt it. Its concern is not with the subjective influences of vibrations, but with the material antecedents of these remarkable issues.

3. Phenomena so singular, depending on the number of the oscillations executed by the molecules of a vibrating body in a given time, it is clearly of much consequence that these velocities become susceptible of exact measurement. And we shall soon discern that other problems, also of great importance, depend for their solution on our minute acquaintance with the character of such vibrations. Fortunately, modern experimental science has furnished many delicate expedients by whose aid even changes thus evanescent, are rendered palpable alike to eye and ear, and so thoroughly fixed, that they can be subjected to a rigorous evaluation. We shall describe a few of these ingenious and effective procedures.—Perhaps the most interesting, and that which promises the most varied results, is the method originating with Chladni, and extensively employed by Savart, while investigating the vibrations of plates or rods. While such are vibrating, motion seems very unequally distributed among their particles. Strewing fine sand over the vibrating surface, Savart found that it accumulated over spots and lines, which he termed nodal spots and lines,—it being clear that there could be no vibration there. The following diagrams show these lines of repose, as manifested under varying circumstances:—





Experiments of this kind require much care, in order that they succeed. Savart suspended the plate lightly by a clasp of wood at one of its edges; and he caused it to vibrate, not by striking it, but by bringing quite near it another body already in definite and known vibration. The same process may be applied to a bell or glass. Nearly fill a glass with mercury or water; set it then in vibration, and the surface of the mercury or water will indicate nodal lines, as well as the character symmetrical or otherwise, of the waves of sound. At present, of course, we merely indicate the means of ascertaining the existence and distribution of such nodal lines.—In the *second* place, a method has to be mentioned, recently proposed by M. Lissajoux—the exhibition of the curious and rather brilliant results of which has recently attracted much notice in Paris. It is at once simple and ingenious. Suppose a mirror attached to one of the prongs of a common tuning-fork, and that a ray of light is made to fall somewhat obliquely on that mirror, it is clear that the direction of the reflected ray will alter with every vibration of

the fork, should it be made to vibrate. If now this reflected ray be made to strike on a screen, the vibrations of the fork will become visible in the rapid motions of a point of light upon that screen. By aid of this very simple contrivance, M. Lissajoux has been able to render the vibratory movement of solid bodies distinctly visible; to manifest the optical composition of vibratory motions taking place in the same direction; to compound two vibratory motions at right angles to each other; to manifest the accordance of two diapasons separated by any musical interval whatsoever; and to offer the easy means of fixing on a constant or invariable fundamental sound. The applications of this method are indeed most numerous: its radical fault is, that it does not provide or leave any abiding trace of its indications.—The *third* class of instruments to which we shall refer are of a different, and, in so far as the attainment of their object is concerned, of a yet more perfect kind. That object is to measure the exact number of vibrations taking place in a second, that are required for the production of the various musical sounds. *First*, we have the *vibroscope* of Marloye,—the rudest, perhaps, of all. But it acts simply;—it consists in arrangements which permit a point at the end of a vibrating rod to impress marks on a rotating cylinder, whose surface is coated with lamp-black. *Secondly*, we have Savart's plan by toothed wheels. He impresses what vibrations he pleases on a thin plate, by causing it to be struck by the teeth of a wheel moving at any given velocity. The number of strokes per second is the measure of the vibrations, whose sounds or notes are heard. *Last*, and unquestionably the most perfect of all, is the *Sirène* of Cagniard la Tour. This exquisite instrument is described under article SIREN, *q. v.*

II. THE PROPAGATION OF SOUND.

The previous section has merely established certain very general principles, as the foundation of the Science of Sound. The first special question that occurs is probably this,—With what comparative velocities are these waves of sound propagated through space by the different vibrating substances? For instance, take a rod of iron of 100 feet in length, and a tube of the same length, filled with air; communicate to the end of each the same vibration, or, what is the same thing, let the ends of both be in contact, when a vibrating motion is communicated to either,—by which of the two will that vibration be first conveyed to its other extremity; what will be the difference of time, and on what physical element does that difference depend? It is unquestionable that every vibrating body in contact with another must communicate to it a system of waves isochronous with its own; but the velocity of the advancement of these waves through space, must depend on some physical character of the body to which the vibration is communicated.—Let us briefly look at this important subject, as it is presented by Experiment and by Theory. (1.) The atmosphere being the main agent in the propagation or diffusion of all sounds with which we generally come into contact, the velocity with which they pass through it, became quite early a matter of anxious investigation. The important fact was soon discovered that, speaking generally, sounds of all *notes*, grave or acute, pass through strata of air with sensibly equal speed: the degree of completeness with which they are transmitted varies with the variation of ordinary circumstances, but not—sensibly at least—the speed of transmission. Probably the most exact definite determination of this velocity, as yet effected, is owing to the experiments made between Montchery and Villejuif, in 1822, under care of the *Bureau des Longitudes*, at Paris. Good experiments of the same sort have been repeated by Moll of Utrecht; and the result is, that through a bed of air of the temperature of 32° Fahrenheit —air of course virtually dry—a wave of sound travels at the rate of 1090·2 feet in one second of time. Now that telegraphic communication is so exact and extended, experiments of this description might be repeated under very favourable circumstances. The velocity of propagation varies even in the case of different elastic fluids or gases. The following numbers may be depended on within small limits of error:—In the vapour of alcohol, the velocity of propagation is 430·8 feet per second; for the vapour of fluoride of silicium, we have 555; vapour of sulphuric ether, 590·1; vapour of chlorohydric ether, 656·2; sulphurous acid gas, 697·5; cyanogen, 757·6; protoxide of azot, 845·6; carbonic acid gas, 846·6; sulphohydric acid, 953; olefiant gas, 1051; bioxide of azot, 1071·7; air, 1090, &c., &c. These velocities become still more varied when we scrutinize corresponding phenomena in the case of liquids and metals or rocks. According to Collodon and Sturm, sound is propagated through water four and a-half times more rapidly than through air; and M. Masson has recently informed us that the transmitting quality of the different metals is represented by the following numbers,—the velocity in air, or 1090·2 feet per second, being taken as unity:—pure lead, 3·976; pure gold, 6·27; cadmium, 7·55; silver, 7·96; platina, 8·41; palladium, 9·81; zinc, 11·14; copper, 11·52; cobalt, 14·25; steel, 14·88; nickel, 14·98; iron, 15·11; aluminium, 15·375. The phenomena involved in these varying velocities of the transmission of the waves of sound are illustrated on the grandest scale on the occurrence of a submarine shock, giving rise to an earthquake. First of all, the wave of sound propagated through the solid rocks reaches and terrifies the dwellers on the neighbouring shores of the ocean; after an interval, a sound reaches them through the sea, along with its terrible waves; and at the close of the convulsion the agitated atmosphere bears its evidence of the vio-

lence that has occurred.—(2.) Turning to *Theory* in quest of a satisfactory, or at least a probable explanation of such varieties, we meet with a few formulæ, which—regarded merely as general theorems—are of very great importance, and nearly circumscribe the subject.—(*a.*) In the case of solid bodies, Laplace long ago established the formula—

$$v = \sqrt{\frac{g}{e}}$$

where v, the velocity of sound, g, the intensity of gravity, and e, the elongation undergone by a wire or rod of the solid a *mètre* in length, when stretched by a weight equal to its own weight. The *Mechanical Theory of Heat* establishes another form of the relation, viz. :—

$$v = \sqrt{\frac{g\,j\,c}{2\,d}}$$

where j is the mechanical equivalent of heat, d, the coefficient of linear dilatation, and c, the specific heat of the body.—(*b.*) In the case of fluids, whether they be liquids or gases, supposing that the temperature of each particle remains the same during the transmission of sound—we have—

$$v = \sqrt{\left(g \cdot \frac{d\,\mathrm{P}}{d \cdot \frac{1}{\mathrm{V}}}\right)}$$

where P is the pressure and V the volume. This is the formula reached by Newton: but it gave a velocity smaller than the actual velocity by one-sixth of the latter. Laplace detected the error, and corrected the formula. It is this—

$$v = \sqrt{\left(g \cdot \frac{d\,\mathrm{P}}{d \cdot \frac{1}{\mathrm{V}}} \cdot k\right)}$$

k being the ratio $\frac{\mathrm{K}_p}{\mathrm{K}_v}$, or that of specific heat at constant pressure and specific heat at constant volume. The action of heat during the propagation of the pulse or wave is the cause of the difference,—necessitating the introduction of the ratio k. Laplace concluded that the formula applied to gases only; it is appliable, however, to all fluids.—It is singular enough, while Laplace's investigations all rest on the old static doctrine of caloric, that notwithstanding the admitted incapacity of that doctrine, their results exactly correspond with consequences flowing from the modern Vortical Theory. (See Art. HEAT, § 17.) A sufficiently striking illustration of the caution which should precede our acceptance of physical hypotheses, on the mere ground that certain of their results correspond with actual phenomena.

ACO

III. THE NATURE AND EFFECTS OF SPECIAL DESCRIPTIONS OF VIBRATIONS.

We shall now treat briefly of the different kinds of vibrations, and refer subsequently to certain peculiar modes of producing vibrations and musical sounds.

I. The known kinds of vibration are three,—transverse vibrations, longitudinal vibrations, and vibrations originating in the torsion or twisting of a rod.

1. The best and most palpable illustration of *transversal* vibrations is the case of a stretched cord, like the string of a violin, a piano, or a harp. If the string, when in tension, is pulled sideways out of its place, it instantly starts back, through effect of its elasticity, and a vibration commences in a transversal direction, or in a direction perpendicular to the length of the string. Any elastic body may be made to vibrate in this manner. The tuning-fork vibrates transversely, and so do those plates whose nodal lines have already been referred to. The vibrations of a bell are transversal likewise.—(*a.*) The theory of such vibrations, in the case of musical cords—which, indeed, forms the whole physical theory of a large class of musical instruments—may be said to be complete. One part of it is summed up in the following theorem:—"*The number of the vibrations of a stretched cord in a given time is proportional*; 1st, *inversely to the length of the cord;* 2d, *inversely to its diameter*; 3d, *inversely to the square root of the tension;* 4th, *inversely to the square root of the density.*" If diameter, tension, and density, remain the same, *the number of vibrations will be inversely as the length of the cord.* It is easy to find from this theorem the precise character of the cords that will yield the various musical notes; but as tension plays so large a part in conferring this quality, the adjustment of the cord is usually entrusted to the hand and the ear. During the vibration of a cord, another very singular phenomenon is manifested, which greatly increases the richness of the sound issuing from it. The cord sends out not only its fundamental note, but *the whole or many of its harmonics, along with it.* An experiment, imagined by Sauveur, furnishes the solution of the puzzle. At a point in a musical string—say one-third of its length from one end—place the moveable stop, and press the string down upon it very gently with the finger. By a stroke of the bow set the third-part into vibration; it will be found that the other part of the string vibrates also—not as a whole, but as consisting of two parts, and exactly as if by another moveable stop the whole string had been subdivided into three parts. Withdraw the stop, and to these three partial vibrations a vibration of the whole string will be superadded. Sauveur concludes that a musical cord does not, when struck, merely vibrate along its entire length, but that each of its halves, each of its third-parts, each of its fourths, vibrates also; and that there-

fore the fundamental note is necessarily accompanied by its harmonics.—(*b.*) Of the transversal vibrations of plates little is known, save the existence of those nodal lines. No satisfactory theory has been offered in account of them. We may probably soon learn interesting facts from the process of M. Lissajoux. Savart employed these vibrations in investigations concerning the structure of various bodies, such as pieces of wood, crystals, &c. It is clear that variations or irregularities of elasticity may thus be easily detected.

2. *Longitudinal* vibrations are executed in the direction of the length of the vibrating body. The molecules of cords, rods, &c., may, by due precautions, be made to vibrate in this way, and to produce sound. M. Marloye has even constructed a musical instrument on this principle, in which twenty rods so vibrating, give out separate notes. But the chief and, indeed, only important illustration of it, is the case of wind instruments, in which columns of air, enclosed in tubes, vibrate and produce rich musical sounds. It is the longitudinal vibration of the air within the tube of the flute or the pipe of the organ, that evolves their sweet and imposing harmonies. The phenomena of these vibrations were satisfactorily investigated long ago by Daniel Bernouilli. We have no space for the interesting analysis of this eminent geometer, and must be satisfied with a simple enunciation of the laws he discovered. They are six in number:—*First.* All cylindrical or prismatic tubes *of the same length* give out the same notes, if (all being open or all closed at the end opposite the end where the impulse is communicated) their length is ten or twelve times their diameter, and the material of which they are made is sensibly rigid. The *quality* of the sound and its *intensity* vary slightly with the nature of the substance of the tube.—*Secondly.* If a *closed* sonorous tube is made to sound by being blown into with greater or less force, it will give out different notes; and if its fundamental note, or the gravest note it can give, be represented by unity, the other notes will rigorously and undeviatingly follow the series of odd numbers, 3, 5, 7, &c.—*Thirdly.* An *open* tube will also give out different sounds, according to the force with which one blows into it; but these notes are represented by 1 (the fundamental note), and the entire series of natural numbers, 2, 3, 4, &c.—*Fourthly.* The fundamental note of a *closed* tube, and that of an *open* one of the same length, are always in octave. The note of the closed tube being the grave one (1), and the note of the open tube the acute one (2).—*Fifthly.* When an open tube gives the sound 2, one may effect an opening in its middle, and even take away half of it, without its note experiencing any change: so likewise if it gives the note 3, openings may be made, dividing the tube into three parts. These phenomena forcibly recall Sauveur's experiments on the division of vibrating cords.—*Sixthly.* Between each of the portions into which a sonorous tube may be divided without change of note, there is a thin motionless *slice* or *stratum* of air—called a *node of vibration.* The existence of these nodal strata can be easily proved. For instance, within a shut tube that gives out the second note, a piston may be placed at the first third of its length, without at all altering the note. But were the piston placed anywhere else, the note would not remain unchanged.—Such the leading phenomena of musical wind instruments.

3. Vibrations of *torsion* are probably often produced, but they have not been turned to use. It is needful to recognize them, however, as a phenomenon in molecular physics. Their existence and musical effect may be manifested as follows:—Fix a cylindrical rod at one end, hold it in the hand at some other point, and rub it gently with a bow in a direction perpendicular to its axis. The contact of the hand prevents all *transverse* vibrations, so that the friction of the bow must cause a torsion, giving rise to synchronous vibrations perpendicular to its length. The sound produced is much graver than what would be due to any longitudinal vibration; so that being neither transverse nor longitudinal, the molecular oscillation produced must be circular, or due to torsion.

Although the modes of vibration now described are quite separate and distinct, it is not probable that any actual body ever vibrates purely according to one single mode. They who are conversant with the more modern progress of the Undulatory Theory of Light, will recollect how much of the completeness of that theory is now owing to the recognition of the co-existence of different kinds of vibrations. A phenomenon that sorely puzzled Savart, viz., the existence of inexplicable nodal lines in bars, apparently vibrating only longitudinally, has just been traced by M. Terquem to the co-existence of transversal vibrations. It is the most difficult duty of the artist who constructs musical instruments to provide against such interferences; but the task is lightened by the tendency of all neighbouring vibrating movements (especially if some one of them is greatly superior in intensity) to throw themselves into harmony.

II. As a close to this article, we shall refer briefly to two peculiar classes of phenomena—recently attracting considerable notice—by which musical sounds are produced.

1. The fact that on the contact of bodies of different temperatures musical tones are evolved, seems to have been noticed by Schwartz so long as 1805, and afterwards, in the same year, by Professor Gilbert of Berlin; but the subject did not receive continued or systematic attention until the year 1829. In that year Mr. Arthur Trevelyan happened to be engaged, in the course of some inquiries, in spreading pitch with a hot trowel, or plastering iron. The iron being too hot,

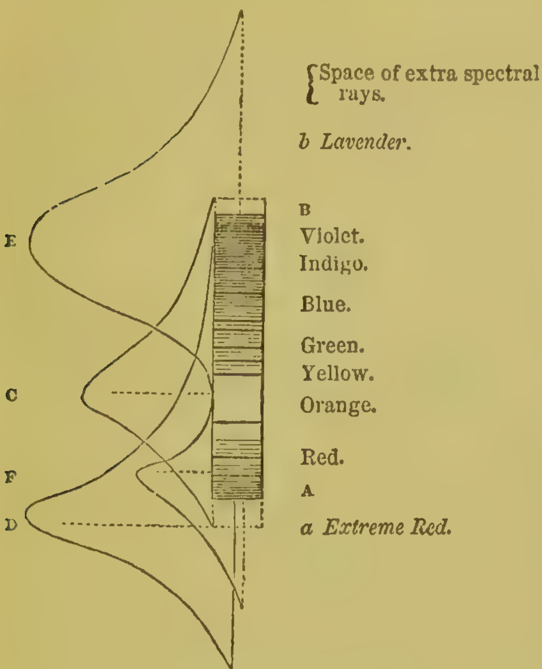
he laid it slantingly against a block of lead, when, to his great surprise, it emitted a shrill note, compared by him to the chanter of the small Northumbrian pipes. Inspecting the iron narrowly, he found it in a state of rapid vibration. It was early established by Trevelyan, Reid, and other experimenters, that almost any of the metals will produce musical vibrations, if a bar of it be laid upon a block of lead or tin—the bar being heated, and the block cold; or if a cold bar of lead or tin be laid upon a hot block of these metals. The explanation, however, was not so easy, and remained until recently a matter of doubt. Professor Forbes of Edinburgh, ever awake to new discoveries, gave the whole subject an elaborate, although not an exhaustive examination; and on the ground of what he considered a number of general laws, he felt obliged to refer the curious phenomena to "a new species of mechanical agency in heat." There is no need, however, of any new species of agency. Sir John Leslie very early suggested the true and simple explanation. Faraday afterwards confirmed it; and in 1854 Dr. Tyndall, with the sagacity that so largely belongs to him, conducted a series of researches that leave nothing further to be accomplished. It is clear that when the hot body first touches the cold one, the cold mass will expand, and throw off the hot mass; the latter will then descend or fall back: the process must immediately be repeated; and a succession of taps will ensue, rapid and powerful enough to evolve musical sounds. The superior efficacy of lead or tin in this case, was ascribed by Faraday to its great expansibility, combined with its feeble power of conduction. If the explanation be correct, the phenomenon must be an unlimited one—certainly not confined within the limits of Professor Forbes's laws. His first law was this—"*These vibrations never occur between substances of the same nature.*" But Dr. Tyndall has shown that iron will vibrate on iron, copper on copper, brass on brass, silver on silver, tin on tin, &c., &c. Again, Professor Forbes asserted that "*both substances must be metallic*:"—Dr. Tyndall proves that the production of the tones depends not on the substance of the rocker, but, as Faraday's view would lead us to expect, on its powers of conduction. For instance, silver, copper, and brass, placed on the natural edge of prisms of rock crystal, fluor spar, or rock salt, gave clear, and, in some cases, very precise tones. No fewer than twenty non-metallic substances were examined by Professor Tyndall, and distinct vibrations always obtained. Lastly, Forbes asserts that "*The vibrations take place with an intensity proportional (within certain limits) to the different conducting powers of the metals for heat,—the metal having the least conducting power being necessarily the coldest.*" The overthrow of the first law carries with it the overthrow of this one; for between two pieces of the same metal there can be no difference of conductive power; but Tyndall further shows that it is incorrect in every particular. The theory of Leslie and Faraday therefore stands.

2. The other class of sounds are produced by those curious singing tubes. It has long been known that if an open tube of some length be so held over a jet of burning hydrogen that the flame shall just be within the tube, a musical note is produced, or the tube begins to *sing*. Various explanations were early proposed; but here also Faraday first reached the truth. He refers the sounds to successive explosions produced by the periodic combination of the atmospheric oxygen with the issuing jet of hydrogen gas. Dr. Tyndall has again brought his originality and exactness to confirm and illustrate this explanation. He has shown further, the dependence of the pitch of the note produced, on the size of the flame. The note given out is, indeed, the fundamental note of the tube, or rather one somewhat higher, in consequence of the increased temperature of the vibrating air; but the size of the flame must be such as to enable it to explode in unison either with the fundamental pulses of the tube, or with the pulses of its harmonic divisions. The remarkable part of his paper, however, is its way of manifesting the keen sympathy between these flames and the human voice, or any neighbouring musical instrument sounding the requisite note. "I placed," says Professor Tyndall "a *sirène* near the flame. The latter was burning tranquilly within its tube. Ascending gradually from the lowest notes of the instrument, at the moment when the sound of the sirène reached the pitch of the tube which surrounded the gas flame, the latter suddenly stretched itself, and commenced its song, which continued indefinitely after the sirène had ceased to sound."—The facts have also been noticed by M. Schaffgotsch. See also articles Ear, Echo, Harmonics, Hearing, Siren, Sound, Tartinis Beats, &c.

Actinism. The remarkable phenomena brought out by *photography*, have shown the existence of solar influence due neither to the Rays which produce Heat nor those which produce Light. This third class of Rays has been named the *Actinic* Rays; and the force in question *Actinism*, or Ray-power. The relation of the Actinic, to the other Rays, in the *spectrum*, or as they are separable by virtue of their different refrangibilities, is exhibited by the diagram in next page, taken from Hunt's *Treatise on Photography.*

From A to B in the diagram is the Newtonian spectrum; recent discoveries extend the coloured spectrum, as distinctly visible, much farther. The curves, C, D, and E, represent the relations of the forces of *Heat*, *Light*, and *Actinism.* The Actinic or chemical rays, are at their *maximum*, in that part of the spectrum where the illuminating power is the least, and *vice versa;* but there appears a second or subsidiary Actinic maximum within the

heating rays at F. A complete account and analysis of the solar beam, will be found under the articles REFRANGIBILITY and SPECTRUM:

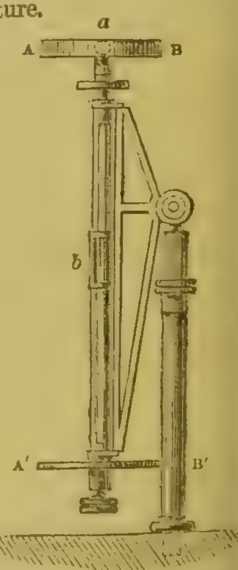


E, Actinism, or chemical radiant power. C, Light. D, Heat.

suffice it to remark in the meantime, that this Actinic Force is almost inversely as the illuminating power: not only do the most luminous rays contain no Actinism, but they tend to *prevent* chemical change;—an inorganic body exposed to the *yellow* Ray, *resists* the influence of Actinic Rays likewise thrown upon it. The reality of this antagonism very distinctly appears in the relation of different climates, hours of the day, &c., to the processes of Photography. The difficulty of obtaining pictures by action of the sun, increases as we approach the equatorial regions; they are more easily obtained in spring than in midsummer, and in the morning than at noon. It would seem also that the Actinic force undergoes modifications, and is subject to cycles, probably not less important than those which are the subjects of Photometry and Thermometry: the cause of such changes, however, has not yet been ascertained. The Actinic Rays have evidently closest relations with the progress of vegetation; to them also is due, the phenomenon of Phosphorescence; and they quicken Electric action.

Actinograph, Dynactinometer, Photographometer. Names given to instruments devised for measuring and recording the variations of the *Actinic* or *Chemical* force of the solar ray. Several forms of these are described in *Hunt's Treatise on Photography.* A classical instrument of this kind, of easy use in observation, is still a desideratum.

Actinometer. The temperature of the earth or of any point above its surface, reckoned by the thermometer, is not a correct measure of the heat sent by the Sun. Much of the latter heat is not transmitted, but absorbed by our Atmosphere; and the Atmosphere becomes in this way a heating source, the intensity of which is indicated by the Thermometer as we generally employ it. To detect the true quantity of heat emanating from the Sun, we must find how much has been absorbed by the Atmosphere, and if possible ascertain what would be the elevation of temperature, could our aerial envelope be for the moment destroyed. The results of this inquiry are stated under *arts.* SUN and RADIATION: the instruments used in the inquiry are termed ACTINOMETERS, HELIO-THERMOMETERS, and PYRHELIOMETERS. The first instrument of the kind was devised by De Saussure: one, similar in principle, but of great delicacy in action, was proposed by Sir John Herschel. We select, however, the instrument called by Pouillet the *direct Pyrheliometer*, for description in this place. Its chief part is a vessel of silver, A B, with thin sides, blackened on its upper face so that its absorbing power be a maximum, and containing about 1500 grains in wt. of water. This apparatus is adjusted so that the direct rays of the sun strike upon it. A thermometer, *a b*, whose stem, passing into C D, the support of the silver vessel, is thus protected from solar action—indicates variations of temperature.



To insure that A B be perpendicular to the incident rays, a corresponding disc is placed below it, and the instrument is adjusted when the shadow of A B exactly falls on the disc A′ B′:—An observation with this Actinometer is made as follows:—The water having attained the temperature of the surrounding air, the apparatus is kept in the shade very near the place where the trial is to be effected:—it is placed so that it receive the full heating action of the sky, without any direct solar rays falling on it; and, during four minutes, a record is kept from minute to minute of its cooling or heating. During the next or fifth minute, it must be adjusted behind a screen so that if the screen were removed, it would receive the direct rays perpendicularly: at the end of the minute the screen should be suddenly removed. Then during five minutes the process of heating must be observed from minute to minute. At the end of the fifth minute replace the screen; restore the apparatus to its former position; and once again observe the cooling, from minute to minute, during other four minutes. The ele-

vation of temperature, t, due to five minutes of direct solar action, will then be

$$t = e + \frac{c + c'}{2}$$

e being the whole elevation of the temperature of the apparatus during the four minutes of its exposure to the sun, and c and c' the cooling observed during the first two sets of four minutes which preceded, and followed the exposure. The theory of the treatment is evident. Pouillet has also proposed an actinometer or pyrheliometer with a lens. It consists of a lens of two feet focal length, at whose focus is a silver vessel containing about 9000 grains in wt. of water, a thermometer being within the water. The shape of the vessel and the disposition of the lens are such, that for any altitude of the sun, the rays can be made to fall perpendicularly alike on the lens, and on the face of the vessel in its focus. The experiment in this case is made precisely as in the former: but the use of the instrument with the lens is preferable during winds. It will appear from this description, that observation with the Actinometer or Pyrheliometer is still of the nature of an *experiment;* neither so direct nor so easy as observation with the Thermometer or Barometer.

Action, the Least, *Principle of.* The advance of all Sciences is in this wise. The facts accumulated are grouped at first by partial generalizations, and the difficulties of investigation overcome by partial methods,—methods and generalizations gradually rising in power and extension, until they lead, as their term, to the fundamental law connecting all the facts, and offering a method for the treatment of every possible problem. The history of *Rational Mechanics*—especially of its main department, *Dynamics*—exhibits throughout, such phases. As its exigencies multiplied, they were classed; and partial methods became discerned, capable of application to each class. These methods were termed *principles*, and were often attempted to be established on distinct, and even *à priori* grounds—sometimes on considerations connected with *Final Causes:*—they are, in reality, only Theorems of greater or less generality, which can now be readily deduced from that more extended and grasping conception of the subject of Rational Mechanics, which we mainly owe to D'Alembert and La Grange. The more important of these are known under the appellations of "*The Conservation of Living Forces* (*virium vivarum*);" "*The Conservation of the Movement of the Centre of Gravity;*" "*The Conservation of Areas;*" and "*The Principle of the Least Action.*" The character of the first three is explained under the heads *Force*, *Centre*, and *Areas:* it is with the last one alone, that we have at present to deal. The history of the principle of Least Action is curious and instructive. It had been noticed by Ptolemy, that a ray of light passing by reflection from one point to another, adopts the *shortest possible course*—a truth flowing directly from the fact that the angle of reflection is always equal to the angle of incidence. On the discovery of the Law of Refraction by Snell, and its promulgation by Des Cartes, the French geometer Fermat—to whom we owe an important step towards the discovery of the Differential Calculus—ascertained that a similar result might seem indicated by that law of refraction; viz.: that while the two media through which the ray passes continue the same, the *sine* of the angle of *Incidence* has a constant ratio to the *sine* of the angle of *Refraction.* Assuming that the ray moves more swiftly through a rare medium than through a dense one, Fermat asserted that in order to pass *in the shortest time* from a point in a rare medium to a point within a dense one, the ray must move in a *broken line*, continuing as long as possible within the rare medium; and his analysis enabled him to discover that the permanency of the ratio of the sines made the *Time* a minimum. Incited by these two singular facts, the imagination of Maupertuis led him to the abstract conception, that Nature must ever act with a view to the *highest Economy of Force;* and the advanced state of Mechanics taught him to translate this metaphysical notion into the important theorem known by the foregoing name, viz.; that the course of a body subjected to the action of any set of forces is necessarily such, that the integral of the product of the velocity of the body by the element of the curve shall be a *minimum.* Long and angry controversy ensued: geometers finally discarding the *à priori* reasonings of Maupertuis, but accepting his principle as a remarkable and useful truth, capable of being demonstrated by inferior and legitimate mechanical Theorems. The property was further generalized, defined, and properly rectified by La Grange, who showed, moreover, that the so-called *Economy of Force*, is merely an accidental and partial concomitant of the true Theorem—sometimes having reference to the *space* described—sometimes to the *time* occupied, and often predicable of neither. The analytical expression as laid down by Maupertuis, and completed by La Grange, need not indeed be a *minimum.* It is only true that its *Variation* is always *o;* so that, for aught we know, the integral may be a *maximum.* The name, therefore, of the *Principle of Least Action*, should be allowed to follow the abstract reasonings of Maupertuis; and in this instance, as in all similar ones, nothing invalidates the dictum of Lord Bacon, that Final Causes are like *Vestals*—not legitimately productive.

Adhesion. The force which unites two substances placed in mutual contact, is called the *adhesive force.* It differs from *cohesion*, which acts only between the constituent molecules of the same body. Its power is measured by the weight which is required to separate the adhering substances. A striking illustration of the power of adhesion will be obtained, if we press together

two pieces of lead, having similar surfaces, well scraped. A very considerable weight will be required to separate them. If, instead of merely placing two bodies together, we make the one slide for a few seconds rapidly over the surface of the other, the adhesion will be stronger. The air between the substances is partially, in that way, expelled. If we could expel all the air between two bodies, we should have a pressure of 15 lbs. per square inch of the surfaces, parallel and equal to the adhering surface, keeping the bodies together (atmospheric pressure). Several artificial compositions have been manufactured in order to produce a permanent union between two bodies. Generally, these bodies are of the same kind, and the composition is so made as not readily to break up itself, and at the same time to adhere strongly to each. Marine glue is probably the most powerful of these preparations. When a substance is dipped into any liquid, a film of the liquid generally adheres to it. This shows that adhesion obtains between solids and liquids. The phenomena of capillarity are partly explicable in the same way. Adhesion is also to some extent produced, when one body is forced into the mass of another. Its difference from *cohesion* (*q. v.*) is perhaps rather hypothetical than real.

Æolian Harp. A musical instrument by which sounds are produced, when a current of air passes over the strings. It was first constructed by Kircher, who gives a full description of it in his *Musurgia Universalis*, (9. 352.) The following method of construction is not exactly his; though very nearly approaching it. It is found to give more distinct tones than are produced by his method. Make a box of thin deal, equal in length to a window in which it is to be placed, about 4 or 5 inches deep, and 5 or 6 wide. Glue on it, at the extremities of the top, two pieces of wainscot about half-an-inch high, and a quarter of an inch thick, to serve as bridges for the strings; and within side, at each end, glue two pieces of beech about an inch square, equal in length to the width of the box. Into one of these bridges fix as many pegs as there are to be strings, and into the other fasten as many brass pins. String the instrument then, fixing them by the pegs and the pins respectively, and tune the strings into unison. The strings must not be drawn very tight. Place the instrument then, in the sill of a window where there is a brisk current, and we shall have all the notes of the diatonic scale commingling together, sometimes in the most exquisite harmonies. The explanation of the Æolian harp is easily found in that part of the article on Acoustics, which refers to the vibration of cords, and which gives an account of the different notes which intermingle in the case of any one vibrating cord. (See ACOUSTICS). Vide *Young's Inquiry into the principal Phenomena of Sounds.*

Æolipile. *Æoli Pila.* The ball of Æolus. An instrument first mentioned by Vitruvius (1. 6). It consists of a ball of metal, to which a pipe is attached by a screw. The ball is filled with water, and heat is applied, when steam issues from the neck with considerable force. The instrument was employed to illustrate the origin of winds. It is only now interesting from its having been the first recorded instrument where anything was made to depend upon the motive power of steam.

Aerolites or Meteoric Stones. The extraordinary meteors to which the *Aerolites* belong are distinguished into three classes. 1. *Falling Stars* or *Shooting Stars;* 2. *Globes of Fire*, or *Bolides;* and 3, *Aerolites* proper, or those solid masses that fall to the surface of the earth through the air. The complete discussion of the questions stirred by this remarkable subject, we shall give under the article METEOR: in this place we propose simply to explain the distinctive character of *Aerolites.*—After much pardonable incredulity, the fact is now accepted that stones and mineral masses do fall to the Earth through the atmosphere. The general unbelief was overcome by the systematic inquiries and report of Biot, undertaken by desire of the French Academy regarding the shower of Aerolites that fell near l'Aigle in Normandy, on 26th April, 1803. A list of all recorded phenomena of this description was afterwards drawn up by Chladni, to whom we owe our earliest systematic knowledge of the singular subject. Chladni's catalogue is necessarily very imperfect, and does little more than establish the fact, that the phenomenon cannot be called a rare one. Most of these occurrences must be unrecorded: for instance, when the stones fall into the Ocean, or when they have fallen unnoticed, as must have been the case with the large mass discovered by Pallas in Siberia, and with other such masses, as travellers have found at the Cape of Good Hope, in Mexico, Peru, or Baffin's Bay, &c., But within recent years we have had several falls substantiated, as matter of fact—as clearly as in the case of the Aerolites of l'Aigle. On the 1st of October, 1857, about 4.45 o'clock in the afternoon, Baron Seguier was, with some workmen, in an avenue of his chateau of Hautefeuille, near Charny, in the department of Yonne. They were suddenly startled by several successive detonations in the air, quite unlike thunder, and by marked atmospheric agitations: on returning to his house, the Baron found that glass had been broken. He learned immediately that a shower of Aerolites had fallen at the same moment a few leagues from Hautefeuille, in the commune of Des-Ormes. Hastening to the spot, he found that a mason working at the time on a scaffold had witnessed the fall, and been nearly struck by one of the Aerolites, which buried itself several inches in the earth at the foot of his ladder. Seguier presented this stone to the Academy of Sciences; and stated that a proprietor of Chateau-Renard had seen at the same hour a globe of fire passing rapidly through the air towards Vernisson—in the

precise direction of the locality where the Aerolites fell. The stone in all respects resembled those of l'Aigle;—the important and really novel part of the phenomenon was, however, the identification of the fall, with the *Bolide* seen at Chateau-Renard.—On the 9th of December, 1858, a similar event occurred at Clarac, in the canton of Montrejean, in the Haute-Garonne. The people were first attracted by a sudden and alarming explosion; and at the same moment the air was filled with millions of sparks, followed by a shower of ashes. A very large stone fell near the church, striking the farm-house of the widow Jeanne-Marie Caperan. This stone was so hot as to burn the hand. It resisted fracture at first, but the peasants succeeded ultimately in breaking it up. Another and still larger fragment was afterwards found at d'Aussun. It had penetrated the earth nearly four feet, making a hole upwards of a foot in diameter. The mass weighed about 100 lbs. avoirdupois. This stone was also broken up by the peasantry; but the Abbé Froment, professor of the seminary of Polignan, who assisted at the extraction of the Aerolite, secured a large fragment of it. It wholly resembles the fragment of the Aerolite of Clarac, now in possession of the Curé of that place.—The nature of these masses has been determined by chemical analysis. They contain the following elements:—Oxygen, Hydrogen, Sulphur, Phosphorus, Carbon, Silica, Chromium, Potassium, Sodium, Calcium, Magnesium, Aluminum, Iron, Magnetic Nickel, Copper and Tin. The following are the chief types in which these elements are found combined:—1. *Metallic Iron*, containing small quantities of Nickel, Cobalt, Magnesium, Manganese, Tin, Copper, Sulphur, and Carbon.—2. *Sulphuret of Iron.*—3. *Magnetic Iron.*—4. *Olivine.*—5. *Silicates* and other earthy substances.—6. *Chromate of Lime* in small quantities.—7. *Oxide of Tin.*—The Aerolite of Montrejean was carefully analyzed, and gave, a magnetic substance—Iron or Nickel; Sulphuret of Iron; Peridot, Labrador; Hornblende, &c. One remarkable circumstance remains to be noticed. An Aerolite fell about 10 o'clock in the evening, near Kaba-Debreczin, on 15th April, 1857; and in this M. Wöhler has found a small quantity of ORGANIC MATTER, akin to *parafine:*—a fact wholly unexpected, and going far to establish the *planetary* character of these singular masses.—As to the origin of such Meteors, three hypotheses have been proposed. *First*, That they are of atmospheric origin like rain or hail;—meteors formed by aggregation.—*Secondly*, Laplace thought that they might be projected from volcanoes in the *Moon*. *Thirdly*, Chladni and others believe them small Planets, or fragments of Planets moving through space, which on entering our atmosphere, lose their velocities and fall to the earth. The first hypothesis is untenable. The second, in the form given it by Laplace, equally so.—We shall return to these speculations under the more general article METEORS: but it seems fitting to subjoin here the leading conclusions recently arrived at by Reichenbach, after an analysis of facts probably the most complete hitherto made. (See *Poggendorf Annalen* for December, 1858). The following are Reichenbach's chief propositions:—(1). Twelve Meteorites or Aerolites, at least, fall on the earth daily, or, 4,500 every year.—(2). Many of them are small; others of great size, some as large as a horse, others of the size of a house, and a small hill. He suspects that some of our large masses of dolerite have had a meteoric origin.—(3). The composition of these masses closely resembles that of the volcanic or plutonic rocks of the earth;—the substances entering into their composition belong without exception also to the composition of our Planet. (4). The *mean* specific gravity of these Aerolites is rigorously equal to that of the Earth; so that on every side there seems a relationship between these two formations. With rather a stretch of fancy, Reichenbach asks, may not our Earth itself be a mass of Aerolites?—(5). The satellites of our Solar System, the Asteroids, and the large or primary Planets, have similar relations as the Aerolites: their respective magnitudes, weights, &c., do not differ more, than Aerolites do with one another.—The point to which Reichenbach's speculations converge is this;—all our planetary bodies—including the Earth, have had what in one sense may be termed a meteoric origin; i. e., *these Aerolites are simply small Planets; and, as such, are constituent and essential parts of our System.* We descend from the larger Planets to small Aerolites gradually and consecutively. These occupy, indeed, a middle space between the Asteroids and the Comets. Reichenbach had previously attempted to show that Aerolites are merely Comets that have passed, by condensation, from a mass of *impalpable dust* to a solid state;—a supposition which accounts well in so far as Comets are concerned, for facts so puzzling as their transparency, their inability to refract light, the polarization of their light, and the circumstance that the light they emit is reflected solar light: in a word, the theory is, that a Comet is merely a swarm of solid grains or molecules far from each other, exceedingly light, transparent, luminous by reflexion, perfectly mobile, and floating through empty space. But an analysis of his own superb collection of a hundred and forty Aerolites showed Reichenbach further, that they consist for the most part of very small spheres or spherules, united by an amorphous substance; or what is nearly the same thing, that they are aggregates of millions of spherules, at one time probably existing *freely* and apart in space. If then, previous to this aggregation or condensation, such a *swarm* had a motion of translation, what could it be except a cometary mass?—It is certainly neither our wish nor our function to pronounce dogmatically on

speculations so closely connected with the obscurest portions of cosmogony; but, in this case, they will not improbably succeed in fulfilling one primary object of all such hypotheses, viz.: they may stir inquiry, and strengthen the belief that even the most strange anomalies are all subject to Law.

Aeronautics. The history of aeronautics commences at a very early period. Archytas, the famous philosopher of Tarentum, constructed a dove able to maintain itself in the air, and Aulus Gellius mentions that this result was accomplished by enclosing in its body an air lighter than the atmosphere. Roger Bacon, about the beginning of the 14th century, attempted the construction of a machine which should give man the power of sustaining himself in the air, and of directing his motions while in it. Cavendish in England, and Lichtenberg and Pickel in Germany, experimented also upon vessels filled with hydrogen. These attempts were made in the laboratory, however, and came to no practical result. Joseph Montgolfier was the first who made an aërial voyage. His attention was excited by observing that a paper bag, which he had thrown on the fire by chance, mouth downwards, rose up in the air. He repeated the experiment, and always with the same success, and the idea occurred to him of raising a balloon by filling it with heated air in the same way as the paper bag was raised. He put his idea into execution at Avignon, in 1782, and more publicly at Annonay, June 5, 1783, in presence of the states-general. Pilatre des Boziers and the Marquis d'Arlande were the first to trust themselves to the balloon, and to risk a voyage in the air. They passed over the city of Paris in their voyage. This, as it was the first, was also the last voyage with balloons filled with heated air. The weight of the fuel to be taken up was considerable, and the danger of the flame dilating in the upper layers of the atmosphere, where the pressure is smaller than at the surface, was very great. In fact, the balloon of the two voyagers caught fire, and they narrowly escaped death. Besides, a rapid descent while the fuel was unexhausted might be very dangerous to buildings of all sorts. Still further, the ascent depended upon the lightness of heated air; and if the air were heated much above the boiling point of water, the balloon would be apt to burn. In consequence of these disadvantages, the Montgolfiers (so called from their inventor) were abandoned. But M. Charles, about this time, repeated the experiments of Cavendish regarding vessels filled with hydrogen gas, and carried them into practice. MM. Charles and Robert rose (December 1, 1784) from the garden of the Tuileries. The gas, from some imperfection in the balloon, beginning to leak out, they descended; Robert left the balloon, and Charles alone reascended; and rose about five times as high as Pilatre des Boziers. This experiment was decisive. Since then very many balloon ascents have been made. None are more celebrated than that of MM. Biot and Gay Lussac, from the important scientific information regarding the state of the upper regions of the atmosphere which it gave. M. Gay Lussac, in a second ascent alone, rose to a height of about 23,000 feet. The barometer stood at a trifle less than half its height at the surface of the earth ($\frac{328}{673}$ being the exact fraction). The temperature also was very considerably changed. When he commenced to ascend, his thermometer stood at 66°·74 Fahr., and at the greatest elevation it had fallen to 14°·9 Fahr. This fall was experienced in the course of a few minutes.—The æronaut usually takes with him several bags to serve as ballast, and in order that, by throwing himself clear of them, he may as rapidly as possible rise to a higher elevation. It is necessary for him to be especially careful not to fill the balloon too full of the gas. In that case, when the pressure round the sides becomes less, as it does in the upper air, there is a violent tendency to explosion. The balloon is usually provided with a stop-cock, by which the gas can be let off in case of such danger. All the materials of the balloon should be tried before they are used, with a view to ascertain their capacity of resistance. The form of balloons depends very much on the purposes for which they are intended—whether for rising, or moving about horizontally in the air, or wandering about at the will of the wind. The power of directing a balloon is still a desideratum. Perhaps a very close study of the common structure of fishes, and of the manner in which they direct their motions of ascent or descent, and horizontal motion, might reveal something of the true answer to this problem; just as the consideration of the structure of the eye suggested to Euler the possibility of achromatism. One practical direction is, that the æronaut should first rise above the region of currents, where, if he can succeed in obtaining a very small directive force, he will reach his end. Balloons, with metallic plates, instead of the ordinary silk or taffetas covering, have been constructed. They were safer than any others, it was thought, against breakage by the distention of air. It has been found practically, that, in consequence of our not being able to procure them homogeneous, they are exposed to far more numerous chances of rupture than the ordinary balloons. The balloon has been employed to enable military men to observe the operations of an enemy; but it is found not well suited for this. The only really practical purpose to which it has yet been turned, is that of enabling us to arrive at such a notion of the state of the upper regions of the Atmosphere, as we could not otherwise attain.

Air Engine. In the manufacture of engines for our great public works, and for our extensive travelling requirements, there are several very important desiderata, the nearer the approximation to which becomes, the more valuable our

engines must be considered. Thus, every engine requires to be provided with safeguards, so that the carelessness of attendants may produce the least possible damage either to the engine itself, or to the building in which it is placed, and the people who may require to be near it. Safety-valves are usually provided, so that when the steam in the boiler has reached, in any way, so high a pressure as even to approximate to danger of explosion, a free passage may be provided for it into the external air. Then, again, it is necessary that an engine should be moderately light in many cases. In stationary buildings this is not so necessary; as the driving engine may usually be placed on the ground, and so may be of any weight. Even, then, however, the important consideration of economy would suggest that there be no unnecessary masses of material employed in its construction. Where, on the other hand, the engine is to be put in a steam-boat, or used on a railway, this consideration becomes of the utmost importance. In such cases the moving force resident in the engines, has to drag the weight of the engine itself along, and thus wastes its capabilities in mere reflexive action. Thus, if a locomotive can be constructed 30 tons in weight, to do the same work as one of 40, besides the important diminution of first cost, the latter engine will, after all, be able to drag a much smaller train, since it has 10 tons to haul behind it, in addition to its proper load, more than the other. A consideration of still greater importance in the construction of a good engine is its economy of fuel. The cost of an engine we have already seen to be of the very utmost importance, but it would be mistaken economy which would make a machine of less capability, or more likely to go wrong, serve in place of a good though expensive one. The first outlay is once for all. But if with two different engines the same effect can be produced, by the consumption of less fuel in the one case than in the other, a permanent advantage is on the side of the former. There is a great daily difference in the expense of "feed," and as, before long, in a large engine, that expense comes to be greater than that of the machine itself, it is better to have one which will consume less fuel for the same work, even although heavier and more expensive. In steam-boat and locomotive engines this is eminently so. The same considerations of expense hold here as before, with the additional important consideration, the lightness of the invariable load. The locomotive engine has always a tender attached, with the coals and water which it carries. If two engines can be wrought to the same extent, doing the same work, the one carrying five tons of coal for its journey, and the other ten, it will be clear that the first can carry precisely five tons additional of extra load. Part of the power of an engine is always wasted in moving its own machinery, and in carrying the load of fuel which its machinery requires. The more these are economized, the more has the engine to spare for its proper work. In a steamboat this is especially note-worthy. A steamer consuming some 20 tons of coal per day (one of very moderate size would do more), must, for a whole day, drag about this load, losing 20 tons of freight by it. If she has, moreover, to pass across a sea, where she cannot get supplies readily, she has to take in coal for her voyage, perhaps 500 tons, and as at her starting she must haul this with her, she loses a large amount of freight. If, still further, she have a longer passage to perform, one of perhaps 70 days, as to Australia, her load of fuel for the journey may become more than she is able to carry, even without any freight. She must, therefore, carry perhaps only one-third of it, losing so much as one-third of her capacity for freight, and requiring to turn in, probably much out of her way, twice for a fresh supply. Frequently there is no place on the route where a good supply of fuel can be immediately obtained, and even when there is, the time spent in taking it in, is a serious drawback to our employment of the resources of steam engines on long voyages.

A multitude of similar desiderata might be pointed out in the steam engine: meanwhile we content ourselves with these three. A good engine must be at once safe, light, and economical in its consumption of fuel.

Taking, then, these as the requirements, engineers and mechanicians have thought of very many ingenious processes by which they might be satisfied. The physical philosopher, considering the whole character and procedure of a *perfect* engine,—wasting no heat in its operations, not heating its containing vessels, losing none by their variations, nor dissipating it in smoke or waste steam; in fact, doing everything which an engine that operates by converting heat into power (a *thermo dynamic* engine) can do, tells mechanicians how far they can proceed. Having reached a certain point, they must stop. Contrivances of theirs may bring us nearer and nearer to that point, may make us almost arrive at it, but there is a limit beyond which these cannot lead us. (See Professor Thomson's paper on the *Mechanical Action of Heat.* Royal Society of Edinburgh. *Transactions*, vol. xx.)

Given the temperature of the body by which heat is communicated, and the temperature at which the used-up material is sent off, the physicist can tell what proportion of the whole value of heat is used in work, and what proportion must be lost in a perfect engine.

We can here allude only very briefly to the convertibility of heat and work—referring for a more extended account of it to the article HEAT. When we lift a body weighing one pound through one vertical foot from its position, we put forth a certain definite effort, or do so much work (in this case, what is called a unit of work). When we put our hand out so as to catch a body mov-

ing in any direction, we also put forth an effort, or do so much work. This effort of ours has been put forth to counteract the inertia of the body by which it would be forced to go constantly on in the same direction at the same rate. The body in its motion, therefore, is said to put forth effort to do work also, and that precisely equivalent to the effort required to stop it, or the work done in so doing. Now, according to the mechanical theory of heat, every heated body is one whose particles are in motion, more or less violent, among themselves. Each particle, therefore, is doing a certain amount of work; and, adding all the work of all the particles, we get a sum of work done by the whole heated body.

Now, when we have such a body, it may be possible, by certain contrivances, such as the Steam Engine, to reduce the particles of matter either to rest, or to a much slower motion, by transferring their motion to new masses of matter. Thus, when a bullet shot off, strikes upon any object not fixed steadfastly, the bullet retains part of its motion, and transfers part of it, to the new object. In this case, the *whole* measureable motion lost by the bullet, is not transferred to the moveable mass, but a considerable part of it goes instead, to *the production of internal heat motion*, in the mass of the bullet and of the body struck. Motion, therefore—*visible motion*—is here converted into *heat*. So, conversely, perceptible heat can be converted into visible motion; as in the steam engine the consumption of coal gives rise to the motion of the machine. An exact equivalence, capable of easy statement, holds between these heat motions, and ordinary motions. The amount of heat necessary to raise the temperature of one pound of water one Fahrenheit degree from 32°, is called the thermal unit, and this thermal unit is accompanied by thermal motions, which do as much work, as is done in raising 772 pounds of any matter one foot vertically from the ground.

The mechanical philosopher comes now to the engineer; and finding how much coal his engine requires, knowing the mechanical equivalent of a pound of coal—the amount of work done by the particles of its mass moving among themselves, when it is heated—he tells him what would be the total work obtainable from it, if this work could all be converted into a palpable mechanical form. Finding next the temperature to which he raises his steam, and the temperature at which the condenser is kept, he tells him how great a proportion of the whole equivalent of the fuel expended, will be realized by a perfect engine working at these temperatures.

The law is expressed as follows:—

$W = J.\ H. \dfrac{S - T}{\frac{1}{E} + S}$, where W is the actual work done; H the number of units of heat; and J the equivalent in units of work, of one unit of heat. S and T are the temperatures in centigrade degrees of the source of the steam issuing into the cylinder, and of the condenser respectively. Hence, $\dfrac{S - T}{S + \frac{1}{E}}$, a fraction smaller than 1, will give the proportion sought. Now, T, the temperature of the condenser, must be kept in given circumstances pretty nearly the same, and $\frac{1}{E}$ is constant ($\frac{1}{E} = 273{\cdot}224$ by experiment). T is constant, because the condenser, kept constantly plunged in cold water, which is changed as quickly as it becomes heated by the emitted steam, does not permit the temperature to rise much or rapidly. The only change, therefore, in the value of these proportions will arise from the change of value in S. Let us see how this will effect the results, for a perfect engine:—

$$\frac{S - T}{\frac{1}{E} + S} = 1 - \frac{T + \frac{1}{E}}{S + \frac{1}{E}}$$

Here everything is constant but the one quantity S. As it increases $\frac{1}{E} + S$ *increases*, and $\dfrac{T + \frac{1}{E}}{\frac{1}{E} + S}$ *decreases;* therefore, $1 - \dfrac{T + \frac{1}{E}}{\frac{1}{E} + S}$ *increases* with the increase of S. If S become very great indeed, sensibly the whole possible mechanical equivalent may be obtained. If it be very small, the realized proportion will become very small along with it.

From which, it appears that the increase of the temperature of the steam admitted into the cylinder secures an increase in that *duty* (as it is called) of a perfect engine, which converts heat into work.

We must, therefore, necessarily, when we wish economy of fuel, work with engines having the steam admitted into them at very high temperatures. But here we are met by a practical limit. If we use steam at high temperatures, we cannot get rid of its enormous expansive power; in consequence, probably, of the great amount of heat which it takes into its mass and retains as latent; which heat while increasing the expansion, does not raise the temperature. With high-pressure engines, we must construct, therefore, very strong boilers, and with such boilers we shall never be quite safe from innumerable accidents; while the expense will be vastly increased. We must have the pressures not so very high above atmospheric, as steam at even 150° centigrade (302° Fah.) would give us; while, at the same time, the temperature at this pressure shall be very much greater than 150°. With such a temperature, which is not by any

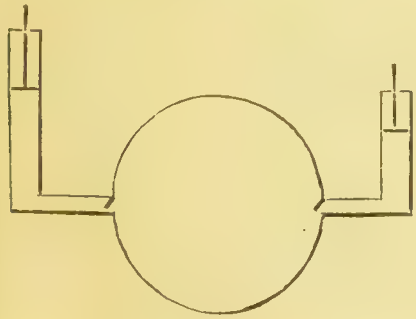
means low for a steam engine, we should have the proportion $\frac{S - T}{\frac{1}{E} + S} = \frac{1}{4}$ nearly, T being considered $= 50°$ centigrade, or 122° Fah. A temperature, at all events, not far below 50°, will certainly obtain for T; the condenser becoming always somewhat heated by the admission of the heated material from the cylinder. From Southern's experiments (*Philosophical Magazine*, 1847, part I.), we find that the pressure of steam at 302° Fah. would be above $4\frac{1}{2}$ atmospheric; and, this pressure from the inside, only a very carefully constructed boiler could usually sustain (the excess over the internal atmospheric pressure being above 50 lbs. per square inch); while the least derangement of construction would be productive of very dangerous explosions.

We have here then a *want*—that the temperature of the heated mass—the source—be raised as high as possible; and a *difficulty*,—that if we raise it to any height above 300° Fahrenheit degrees, we shall meet with practical difficulties of the most alarming kind. These are, moreover, inseparable from the use of steam.

This circumstance has urged the attention of mechanicians to *heated air* as a moving power. The advantage of employing it,—should mechanical difficulties of construction be surmounted,—will be at once evident from this fact, that the same pressure at which steam has a temperature about 300° Fah., gives a temperature about 1700° Fah. to heated air.

The most recent investigations on which reliance can be placed, are contained in a paper read by Mr. Joule of Manchester, before the Royal Society (*Philosophical Transactions of the Royal Society*, 1852, vol. i.), to which, for a fuller treatment of the subject, we refer. We proceed to detail the principal steps of his process, and the results at which he has arrived.

He considers the Air Engine, in its simplest possible form; supposing that all such practical difficulties as the loss of heat by radiation from the heated air vessels, &c., are got over. His theoretical Air Engine is an apparatus consisting of two separate cylinders, each communicating by valves with a receiver.



In one of these cylinders a volume of air is compressed, increasing, equally therefore,' in temperature and in pressure, since the apparatus loses no heat to the surrounding air. Suppose the process to have gone on for some time, the receiver being opened, its valve opening inwards, communicating with the condensing cylinder, being driven in at each successive descent, and then shut by the pressure of the air just admitted, when the piston of this condensing cylinder is drawn up. A valve in this piston, also pressed inwards, admits common air when it has been drawn up, and shuts again, when a new compression commences. Suppose, then, the receiver filled with air above the ordinary temperature and pressure (heat being all this time applied to the receiver). We shall have this confined air opening a valve which goes outward from the receiver, and entering beneath the piston of an expanding cylinder driving it up. Now, there is a certain amount of air driven in, compressing the air in the condensing cylinder. When an equal amount of air has been given off with the receiver, the valve opening into that cylinder shuts. This is managed by an arrangement of the weight of the valve. The air in the receiver, so expanding as it does when this valve is open, gets more room, and presses less upon any vessel enclosing it. Then as the valve is made somewhat heavy, it shuts by its own gravity—the moment that the air has lost the elastic force which counterbalanced this gravity and drove the valve open.

At this point there is no material advantage gained, upon the whole. The ascending piston might be made, through that part of its ascent which it has now performed, to do the same work nearly that was required to push down the descending one. The air, however, at this time, in the expanding cylinder, is still hotter, and presses more powerfully on the sides consequently, than common air. The piston is accordingly still pushed upwards, and may, in doing so, be made to work an engine. By another valve, when this operation has been carried far enough, the piston is allowed to resume its original place, and to be again pushed up by a new rush of air from the cylinder.

The process employed in Mr. Joule's experiments is this:—The receiver is filled with air at atmospheric density, which is heated to a moderately high temperature (390°·464 Fahr.) At this temperature the air in the receiver, being kept in constant volume, presses against the sides of the receiver with considerable force (25·95 lbs. per square inch). When the piston, which has to descend through 12 inches, has passed through a part of its stroke (4 inches), the pressure is the same as that of the air in the receiver. The air is, however, at a much lower temperature (107°·31 Fahr.), because it is not kept at constant volume, but compressed. The valve then opens into the receiver. During this part of the process, the air had opposed a continually increasing resistance to the downward motion.

During the remainder of the descent, if the receiver be pretty large compared with the cylinder, it will remain what it is at the end of this. The fact of the whole body of air contained in the cylinder and in the receiver being compressed into one space may be disregarded if it be so. The heat then applied to the air in the receiver increases its temperature and its volume, and drives open the valve, and makes the second piston ascend. Here, also, the fact of the air contained in the receiver expanding through part of the cylinder until the original work spent be regained, can be considered as producing no material change upon the pressure. When the valve closes at the moment it is completed, and the communication between the cylinder and the receiver is closed, the air in the *cylinder* alone expands, and is reduced to the atmospheric pressure.

In order to show how the calculation can be effected upon which depends the excellence of such an air engine, we will quote an example given by Mr. Joule. For the mathematical principles upon which his calculations are founded, we must refer to his paper.

He has a condensing cylinder 12 inches long, and with a sectional area of one square inch. The area of the section of the exhausting cylinder is the same. He commences with air of the density of the atmosphere in the receiver, having a temperature of 390°·464 Fahr. According to the gaseous laws, the pressure at this temperature will be (supposing that of atmospheric air to be 15 lbs. per square inch,) 25·95104 lbs. per square inch. The piston will require to be wrought through one-third of its stroke (4 inches) before the air reaches this pressure. At this point the temperature will be 107°·3094, and the work which will have to be done would be sufficient to drive 6·537154 lbs. through one foot of space. The atmosphere, however, has operated upon the square inch of area of the piston, with a force of 15 lbs. through 4 inches of space. There is, therefore, due to it force sufficient to drive 5 lbs. through 1 foot. Hence the effective work done by the engine, absorbed in this part of the process, is 1·537154. During the remainder of the stroke, there is a pressure of 25·95104 lbs., acting on a square inch through $\frac{8}{12}$ths of a foot, and of this pressure 15 lbs. acting similarly is balanced by the atmospheric pressure. We have, therefore, $\frac{8}{12}$ (25·95104—15) = 7·300693 foot lbs. as the amount of work done in this part of the process by the engine. In the expanding cylinder, again, as the process goes on, until as much air has been let out as came into the cylinder, we have, in that part of it where there is emission from the receiver, a passage of air through 12 inches, with a pressure of 25·95104 lbs. resisted by an atmospheric pressure of 15 lbs., and giving off thus 10·95104 ft. lbs. of work. The communication being now cut off, the air expands until it reaches the pressure 15 lbs. As it was required to compress the volume of air in the condensing cylinder to two-thirds of its original bulk, in order to increase the pressure from 15 to 25·95104, it will be necessary, inversely, to expand it to three halves of its bulk, in order to bring it from the latter pressure to the former. The air will accordingly expand after the communication has been cut off, through 6 inches, and the effective work will clearly be $\frac{3}{2}$ (1·537154) = 2·305731. Hence the work given off, in the whole course of the operations, will be 10·95104 + 2·305731 — 7·300693 — 1·537154 = 4·418924 ft. lbs.

Now, in order to find whether the engine be economical, we must consider what has been expended to attain this result. The only source of expenditure is, then, the heat required to raise the temperature (107°·3094 Fahr.) of the eight cubic inches of air, at the constant pressure already stated, up to 390°·464. We find, then, that the heat required to do so would raise 1 lb. of water 0°·043043 Fahr., and, therefore, dividing 4·418924 by this, we obtain the number of ft. lbs. of work obtained from every amount of heat capable of raising 1 lb. of water 1° Fahr. In this case we shall have 102·66276 ft. lbs.

But, the theoretical maximum, when all the heat employed is made full use of, and none of it wasted (as in this case the air is let off from the exhausting cylinder, at 237°·5 Fahr.), is 772 ft. lbs. for every amount of heat capable of raising 1 lb. of water from 0° to 1° Fahr. In a perfect steam engine (free, as we imagine this engine to be, from all loss of heat by radiation to the surrounding bodies, or conduction through the vessels composing it), where steam at 284° Fahr. is employed, which is about as high as it can safely be used at, 209 ft. lbs. of the 772 may be thus brought up, or converted into work.

If, however, we keep the temperature and pressure of the air in the receiver very much higher than what we have supposed, we shall obtain more favourable results. Thus, if we have a pressure of 70·60445 in the receiver, the piston would require to pass through 8 inches of its stroke in order that this may be reached. If the air be, further, of double the ordinary density, its temperature will be 706°·559, that of the air forced into the pump will be at the same pressure, and with temperature 311°·3732. The temperature of the air escaping into the atmosphere will be 277°·5, and the amount of work obtained will be 279·9628. None of these temperatures or pressures are such that they could not be borne by well-constructed vessels.

Considerable practical difficulties, however, stand in the way, but the results already attained warrant a confident hope that air engines may yet be brought into favourable competition with steam engines, and may perhaps supersede them.

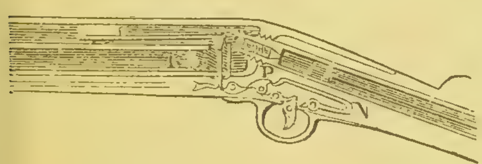
Mr. Joule suggests the following remarks to

those who may be desirous of constructing a good air engine:—

The receiver need not be much larger than either cylinder. In the practical engine, the exhausting valve opens just as the valve opens from the receiver to the condensing cylinder. Hence the volume is kept sensibly equal to that in the receiver originally, and the pressure, therefore, is sensibly the same. A coil of pipe might form the most convenient receiver, as having a constant heat most readily applied to it, so as to maintain the air within at a uniform temperature. If this be secured, the air engine would act regularly.

The ordinary methods, described more fully in the article on the STEAM ENGINE, would require to be adopted in order to transfer the work from the cylinder and piston rod to the weights to be moved, and in order to secure the opening of the two valves, as just described, simultaneously.

Air Gun. An instrument in which the elastic force of compressed air is made use of as the moving power. We give a representation of a section of the common air gun in that figure below. There



are two cavities in the gun. The one is closed at both ends, by a plate and a moveable piston, and opening into the other by a valve, which is moved by the trigger, and which is placed immediately behind the bullet, K, in the open barrel. The piston is so constructed that it can be opened and fresh air supplied, when the air first used has become exhausted. The piston is then pushed upwards, and the air in the barrel is condensed. If the cavity in the stock of the gun be very large compared with the passage beside the barrel, a very high degree of condensation may be obtained by the expenditure of all our strength. The trigger is then touched, and the air entering behind it expands violently, driving the bullet before it. The trigger is then left free, and recoils, closing the valve, and leaving the air somewhat less condensed than before. A considerable number of shots may be discharged, however, with the same air, if the condensation has been very great at first. The velocity which the air gun gives a bullet, depends of course upon the degree of condensation. When the air has been condensed into about $\frac{1}{40}$th of its bulk, the velocity will be about half that produced by gunpowder. This is, however, a very high degree of condensation. The air gun is too expensive for ordinary use. The least defect in the fitting of the condensing piston, or in the construction of the cylinders, would render it useless. In war, too, the force which one using it requires to expend, is a serious practical objection to its general adoption. It might, perhaps, be readily charged by small steam engines kept constantly working. The invention of it is ascribed to Ctesibius, who lived in Alexandria, B.C. 120. His instrument is described as having discharged arrows by the elastic force of condensed air. Marin, a native of Lisieux, in France, revived the invention in modern times, and presented one to the reigning sovereign, Henry IV. Instead of a simple piston, a condensing syringe is employed practically, which, with much less waste of space, produces greater condensation. See AIR PUMP.

Air Pump, is an instrument for extracting the air from a vessel. We shall explain along with it an instrument for condensing air into a vessel, depending upon the same principles, and called the *condensing syringe*, as the air pump is the *exhausting syringe*. Fig. 1 represents the former, and fig. 2 the latter, instrument. The

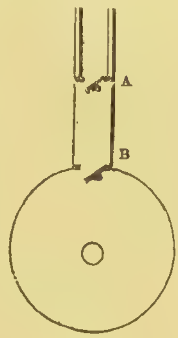


Fig. 1.

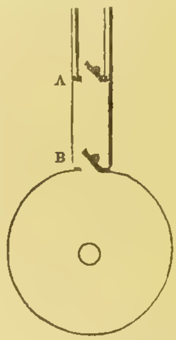
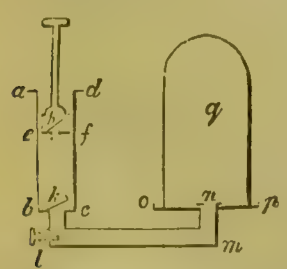


Fig. 2.

bulb, O, in either case, is the vessel, the air of which is to be wrought upon. The valves, B and A, open upward in fig. 2, and downward in fig. 1. Suppose, then, in fig. 2, the piston raised from the bottom to the top of the tube. As there has been no air in the tube, it rushes through B and fills the whole space. It loses, however, in pressure, and is consequently unable to lift the valve, A. After it has risen to the top, the valves, A and B, close. Now, push the piston downwards, the downward pressure upon B is increased, but it only opens upward, and the air in the vessel, therefore, is not disturbed during the downward motion of the piston, but remains at the same pressure as the whole air in the machine after the first operation. There is, however, powerful compression of the air in the tube produced in the descent, and when the pressure becomes sufficiently great, the valve, A, is lifted and the air escapes. Let us suppose the vessel to be 3 times the size of the tube, and examine what the result of these processes will now be. At the end of the first, the air has expanded to $\frac{4}{3}$ of its original bulk, and so $\frac{1}{4}$ of it is contained in the tube, and $\frac{3}{4}$ in the vessel. At the end of the second process this $\frac{1}{4}$ is expelled, and the $\frac{3}{4}$ remains. When another pair of similar processes were completed, we should have $\frac{3}{4}$ of $\frac{3}{4}$ remaining. After five pairs we should have $\frac{243}{1024}$, or about $\frac{1}{4}$ remaining: and by con-

tinuing the process we should succeed in reducing the air remaining within to any fraction, however small, of the original quantity. The rapidity of the process will depend, if the workmanship of the instrument be perfect, upon the tube bearing a large proportion to the vessel to be exhausted. One practical difficulty is thus remedied. When the fraction of the original quantity of air left in the tube becomes very small, its pressure has become reduced in the same proportion. In consequence, when we raise the piston from B to A, the air, having lost its elastic power, comes to be unable to lift the valve, B, and no further exhaustion would be possible. In fact, if there were left $\frac{1}{100}$th of the original, the pressure would be $\frac{1}{100}$th of 14·7 lbs. per square inch. Suppose the valve, then, to be 3 oz. weight, and 1 square inch in surface, there will be a downward force of 3 oz., besides a slight cohesive and frictional force, opposing an upward force of $\frac{14 \cdot 7}{100} \times 16$ oz. $= 2 \cdot 352$ oz., and the valve would consequently remain at rest. To obviate this, a string passes from the one end of the piston to this valve, so long as to become tightened just before the piston reaches the top, so as to open the valve, however small the upward elastic pressure of the air may be. The condensing syringe works thus:—Let the piston be pushed down. Then, as the air will rush outward, the valve, A, will be pushed up, and the whole content of the tube pushed into the vessel. In this state the pressure upon B, when it closes, upon the restoring of equilibrium will be, in the case we have supposed, $\frac{4}{3}$ the original pressure, and will, therefore, keep it easily closed against the air. When the piston rises, as there is no air between A and B, the tube will be refilled with common air. When we push down the piston again the same process will be repeated, but the valve, A, will not open until the piston has gone through $\frac{1}{4}$ of its stroke, because then the pressure will be just equal above it and below it. There will be then $\frac{1}{3}$ more pushed in, and the process may be continued as far as we choose. Latterly, the work of pushing the piston down will become very great, and so much the sooner the larger the proportion is which the contents of the tube bears to those of the cylinder. Here the condensation increases arithmetically, $1 + \frac{1}{3} + \frac{1}{3} + \frac{1}{3}$, &c., representing the results of each successive pair of strokes; in the case of the exhausting syringe, the progression is geometrical $(1 \times \frac{3}{4} \times \frac{3}{4} \times \frac{3}{4}$, &c). The methods by which the practical difficulties of the mechanism are overcome, and by which the instrument is made capable of more extended practical applications, need not be recounted here.



We engrave the common air pump. A tube communicates from the bottom of the plate with one or two exhausting syringes, *a* and *d*, the pistons of which are wrought by rack-work, moved by the turning of a handle. Sometimes a barometer is placed inside to serve as a gauge of the degree of exhaustion. Torricelli made the first vacuum, but Otto Guericke first constructed an air pump, which he exhibited publicly at Ratisbon (1654, A.D.). Boyle, Hauksbee, Gravesande, Smeaton, and others, have improved upon his model by adding rack-work, putting two syringes for one, &c. The principle has remained constantly the same

Alembert D', *Principle of.* To the illustrious Frenchman, whose name this principle bears, belongs the unquestioned honour of placing the science of Dynamics on a basis sufficiently broad for all possible requirements. Previous to his time, Dynamical problems were not resolvable by any general method; each class of problems requiring the aid of some special theorem or method applicable to that class alone;—in many cases, indeed, the method had to be found for the particular problem. D'Alembert conferred on Rational Mechanics the inestimable boon of a method applicable to every form of investigation; and, what was more, he practically reduced *Dynamical* problems to mere *Statical* ones. His principle has the further advantage of being easily explained. Suppose a number of bodies connected by a system of forces, it will always be easy to detect the positions that will be occupied by the bodies, at the end of the next moment of time, or the spaces which they will all pass through, during the next moment, supposing the system to continue in action. But it will also be easy to detect the spaces through which they would pass during the same moment, were the *system* or the *connection* suddenly broken up. Compare, then, these two sets of spaces, viz.: the spaces through which they *will* actually pass, and the spaces through which they would pass if *free*. By the theorem of the *composition of forces*, the former spaces may be resolved into the latter spaces, and a third set of spaces. But this third set indicates the motions destroyed because of the *connection* of the bodies, or because they act and react on each other, through a System. And, as action and reaction are always equal within a system; it follows that the sum of the quantities of motion annihilated must be equal to 0; in other words, that what one set of the bodies have gained, another set will have lost. So that we have an Equation of equilibrium, which, rightly treated, will unfold the characteristics of the system of Forces under consideration. La Grange at once recognized the signal value of this Principle of D'Alembert, and made it the basis of his methods in the *Mecanique Analytique.*

Algebra, taken in its full generality, is now the most potent arm of Mathematical Science. As with all great powers, the efficacy and extraordinary grasp of Algebra, were not suspected during its rude beginnings; nor does it seem improbable that its capabilities as a help in the pursuit

of Truth, are as yet but very partially known. As imported from the East by the Pisan Merchant Leonardo, about the year 1200, it was merely a contrivance to save trouble—a sort of short-hand writing, applied to the solution of Arithmetical problems: it has already passed far beyond that, having grown into an instrument by which we can disentangle the most complex relations of Quantity, Form and Force; and determine the effects of their most complex combinations: in future times, it may ascend into an Organ of Universal Logic, and enable us, with an equal power, to disentangle the more subtle processes of Thought. In illustration of the bearing of this latter remark, the student is referred to the recent works of Mr. Boole. Of a subject like Algebra, we can give little account in this Dictionary: it will be enough to advert, under three heads, to the leading phases under which, as an abstract science, it at present appears.

(1.) *Arithmetical Algebra.* This comprehends the whole of the original Algebra, as known to the Hindoos, the Arabs, or to Diophantus Vieta or Cardan. It is simply what Newton called it—*Arithmetica Universalis*; and its artifices are as follows:—to denote the ordinary Arithmetical operations, symbols of operation such as $+$, $-$, $\times$, $\div$, : $\surd$, &c., are employed: instead of known or fixed numbers, the Algebraist uses *letters*, such as a, b, c, &c.; and for quantities really determined, through their relations with others, but whose explicit value has not been actually worked out, he employs the letters x, y, z, &c. The reasoning on which the Arithmetical Algebraist adventures, is of course purely Arithmetical; that is to say, he can admit nothing either in operation or result, which cannot be predicated of pure numbers. His art, however, gives him the immeasurable advantage of reasoning and determining *generally*, instead of requiring to do so for every *special case*; general theorems concerning number can thus be formed, and laws laid down for the easy treatment of whole classes of problems. Included within arithmetical algebra, we have the solution of Numerical *Equations*, the doctrine of *Permutations* and *Combinations*, and of *Chances*, and the *Diophantine Analysis* in its largest extent:—*q. v.*

(2.) *Symbolical Algebra.*—A vast extension of the primary algebra; one whose methods and results were accepted, long before it was felt necessary to revise and reconstruct its logic and foundations. In this—the true modern algebra—the symbols of operation are considered no longer representatives of definite arithmetical processes; they are symbols of operation, defined merely by their relations to each other. For instance, $+$ and $-$ are merely two symbols of *inverse* operations; $\times$ and $\div$ are also each other's *reciprocals*, or *inverses;* while $+$ and $\times$ are connected by a general relation, suggested by the relation between addition and multiplication, and inclusive of it. Viewed from a point of view so abstract, all those symbols whose occurrence and employment in arithmetical algebras were sources of ever-recurring puzzle and vexation, (such as *negative quantities*, *numbers* less than *nothing*, and those curious *imaginary quantities*, as they were fitly termed), take on intelligible and important meanings, and become symbols of actual and rational processes. The student will learn much concerning the value of the change now referred to, even in this limited respect, from the second volume of Dr. Peacock's admirable treatise on algebra; nor can it require to be more than hinted, that, under cover of this general idea, other signs of relation—signs, adequate in number and significance, to all possible relations of things and thoughts—may become definite logical signs in an Universal Symbolical Algebra. As to the old symbols or representatives of *number*, these may of course be made representative of any definite thing or idea, known or knowable, through its fixed relations with others. Already algebraic science teems with hints of what its fabric will one day become,—not the least important, being those tendencies towards a pure *Calculus of Operations*, which one easily detects through all its more daring generalizations. The following divisions of what is commonly called algebra, cannot be treated logically, unless on the basis of this extension of the meaning of symbols:—the theory of *indices*, including so-called *imaginary expressions;* the general theory of *equations;* the *arithmetic* of *sines; exponential quantities; series* and *development* in general. The entire application of *Algebra* to *Geometry* reposes on an extension of the meaning of the ordinary symbols: something on this subject will be found under QUATERNIONS.

(3.) *Algebra, Double, Triple.*—Terms applied to those important extensions of symbolical algebra, which promise to enable it to cope more directly and easily with problems concerning space of two or three dimensions. For all that we can say on the subject see GEOMETRY SYMBOLICAL. The student is especially referred to a remarkable treatise on *Double Algebra*, by Professor De Morgan.

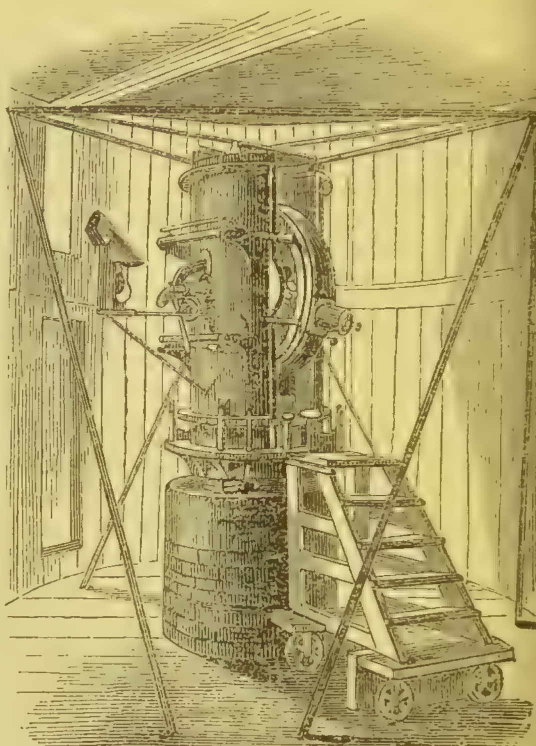
Algebraic Geometry. See GEOMETRY ANALYTICAL.

Algebra, the Modern. See POLYNOME.

Altitude in Astronomy is the arc of the great circle passing through the zenith, intercepted between the object and the horizon.

Altitude and Azimuth Instrument.—An instrument that has played an important part in practical astronomy, and which, although superseded to a large extent by the use of Meridional Instruments, has recently been restored to our great Observatories, under several forms, most recently and perhaps most effectually at Greenwich, by the counsel and under the especial care of Mr. Airy. The general character of an altitude and azimuth instrument is this:—*First*, there is a horizontal circle finely divided and

supposed to be planted perfectly horizontally, and so that the zero point of its divisions coincide with the meridian line, or with the exact south. If this circle is not perfectly horizontal, or if the zero point of its divisions does not coincide with the exact south point, the indications of the instrument, in so far as this portion of it is concerned, will be erroneous; but as careful testing can find out that exact amount of error in either particular, which remains after mechanical adjustment has done its utmost, the *correction* required to be applied to every individual observation may be deduced, and the observation thus rendered as good, as if the instrument had been perfect. From the centre of the horizontal circle an upright pillar arises, which ought to be perfectly perpendicular to the plane of that circle; and to the side of this pillar is attached a vertical circle, to which the telescope belongs, through whose optical axis the observer discovers the star he desires to *fix*. By the theory of the instrument, this vertical circle, in whatever way it is turned—as the pillar to which it is attached moves round—ought to be perpendicular to the horizontal circle; and the zero point of its division should coincide with the absolutely horizontal or the absolutely just zenith point. These requisitions never being exactly fulfilled by mechanism, corrections must be applied here also; so that the *accurate* use of the instrument is by no means an easy task. Something further on this subject will be found under CORRECTIONS; and further still under CIRCLE. The importance of the instrument consists in this:—observations can be made by means of it, much oftener than is possible by instruments which are confined to the *meridian;* a quality of great moment, in the case especially of comets, whose apparition is so evanescent, and which, in uncertain weather cannot be often seen on the meridian. Its inferiority to meridian instruments, is owing to its comparative complicacy; but this has been got over to a marvellous extent by the skill of our famous workmen, as well as by aid of the theory of corrections. The most celebrated Altitude and Azimuth instruments are the great ones by Ramsden; the Westbury Altitude and Azimuth instrument by Troughton; the great vertical Circle of Ertel; and last, and perhaps best of all, the instrument just alluded to, recently erected at Greenwich. Among the many reforms carried out so vigorously by Mr. Airy in the instrumental department of that great Institution, this is not the least: he has attempted in the first place to exhaust all that mechanism can do on behalf of the solidity—the firmness, of astronomical instruments and their adjustments. We shall require to advert to this subject again under CIRCLE; but probably the triumph of the Astronomer Royal has been nowhere more decided than in the case of the instrument now more especially referred to. A sketch of it is subjoined; the instructed eye will immediately seize the significance and relations of its different parts. Regarding some of its recent work, see LUNAR THEORY.



Altitudes Double, Method of; Altitudes Equal, Instruments of.—The *instrument of equal altitudes* now belongs to the archæology of astronomy, and need not detain us here. The *methods of equal and double altitudes*, with all their relations, will be found explained under LATITUDE, LONGITUDE, TIME.

Altitudes, Measurement of.—The measurement of celestial altitudes is accomplished by the altitude and azimuth instrument and by the meridian circle (*q. v.*). The measurement of terrestrial altitudes, by the common methods of trigonometry (see THEODOLITE), it is not necessary to notice in this place. It is otherwise, however, in regard to the *measurement of altitudes* by the *Barometer*, a process as nice in theory as convenient in practice. We shall attempt briefly to explain it. It is well known that the *Barometer* measures the *weight* of the superincumbent column of air, at the spot where an observation is made, or, allowing for temperature, it measures the *density* of the air at that place. Now it is very evident that the density of an elastic aerial envelope like our atmosphere, must diminish as one ascends within it; the atmospheric layer at the surface of the earth being pressed on by the whole superincumbent air, is necessarily denser than any higher bed which is not pressed on by the whole atmosphere, but only by the portion above it. Conceiving the atmosphere, then, in the main something of a

homogeneous mass, *i.e.* taking no account of its internal irregularities with regard to *heat*, &c., it is easily shown that the densities of these various beds diminish according to a certain definite ratio as we ascend; in fact, in relation to altitudes increasing in *arithmetical* ratio, the densities diminish in a *geometrical* ratio; or, what is the same thing, the several altitudes form a peculiar system of logarithms of which the reciprocals of the densities are the natural numbers, that is—

$$A - \lambda o\gamma \frac{1}{D} = - \lambda o\gamma D$$

$$a = \lambda o\gamma \frac{1}{d} = - \lambda o\gamma d.$$

Whence—

$$A - a = \lambda o\gamma d - \lambda o\gamma D = \lambda o\gamma \frac{d}{D}.$$

Or supposing $a = o$, and consequently that d is the density of air at the surface of the earth, we have for any altitude—

$$A = \lambda o\gamma \frac{d}{D}.$$

The next question is, what is this peculiar system of logarithms? Or—as any one system of logarithms can be converted into any other system by multiplying the former by a constant quantity—we have to fix a value, m, that will make the following equation true:—

$$A = m.\ log.\ \frac{d}{D}.$$

The term *log.*, now represents the logarithms of the ordinary tables. This m is detected by experiment. It involves our determining according to *what peculiar geometrical ratio* the density diminishes with height: and m is found to be very nearly 60,000: therefore—

$$A = 60{,}000 \times log.\ \frac{d}{D}\ \text{feet.}$$

Or since the *heights* of the barometer are proportional to the *densities* of the air, we have, naming these, h and H:—

$$A = 60{,}000 \times log.\ \frac{h}{H}\ \text{feet}$$

h being the height of the barometer on the surface of the earth, and H the same height at the top of the mountain, or other altitude, which it is desired to measure. Such is the simple basis of the theory at first laid down by Dr. Halley; but it is greatly complicated in practice, by the *corrections*, requiring to be applied to the foregoing easy formula. These corrections mainly arise, from the influence of variations in heat alike upon the barometer and the atmosphere; from variations in the force of gravity, depending on the height of the stations, and the latitude of the place; and as has been lately pointed out by Bessel, from the modifications produced by the existence of a humid atmosphere of peculiar habitudes, within the dry or permanently elastic one. When all conceivable corrections of this kind are applied, the formula becomes extremely complex—so complex indeed that it cannot be used unless with the aid of subsidiary tables—*hypsometric tables*, as they are called on the continent. Numerous collections of tables of this kind, with directions for use, exist in this country. We only specify those of Mr. Baily and Mr. Galbraith. A copious set were published by Oltmanns, and applied to Humboldt's invaluable observations among the Andes; but on the whole, the best we have recently seen are those by Delcros—taking into account every member of Laplace's minute and comprehensive theoretical formula—published in the *Annuaire Meteorologique*, for 1849, at Paris. None of these tables, however, include Bessel's correction regarding the humidity of the air; a correction whose importance he indicates in No. 356 of the *Astronomische Naehrichten.* In the *Annuaire* above mentioned, for 1852, a new set of tables are produced by M. Plantamour, having reference to *humidity*, and certainly they leave nothing to be desired. It is worth while to estimate, by a testing example on a great scale, the value of this new correction. Calculated on the ground of observation, by MM. Bravais and Martins, on their ascent of Mont Blanc, on 29th August, 1844, the height of that mountain appears by the tables of Delcros to be 4814·5 French metres; calculated by M. Plantamour's tables it is 4811·7 metres; the difference being 2·8 metres, or about 1 in 1600, *one-sixteenth per cent.* The question naturally occurs, whether these minute theoretical corrections, are not far within any possible accuracy of observation? For this subject see Barometer. In the meantime, for general observations with *ordinary* instruments, we commend the following simple rules. The heights of the mercury and the indications of the attached and detached thermometers being observed at both stations, as nearly as possible simultaneously, put in practice these rules:—(1.) Correct the length of the mercurial column at the upper station, adding to it the product of its multiplication into twice the difference between the degrees on the attached and detached thermometer—the decimal point being shifted four places to the left. (2.) Subtract the logarithm of this corrected length, from that of the lower column; multiply by six; and move the decimal point four places to the right: the result is the approximate elevation in English feet. (3.) Correct this approximation, by shifting the decimal point three places back, and multiplying by twice the sum of the degrees on the detached and attached thermometers; the product, being added, will give the true elevation.

Amphitrite. One of the small planets between Mars and Jupiter. See ASTEROIDS.

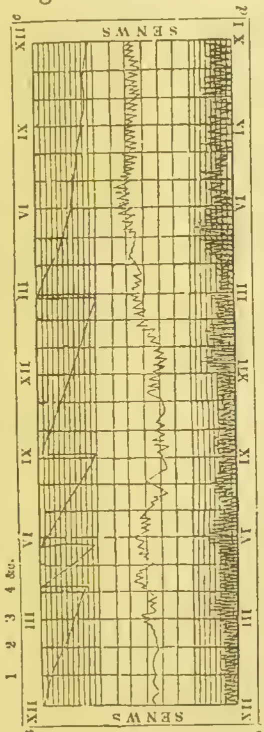
Analysis. Mathematicians understand by *Analysis*—in the widest and most common acceptance of the term—*Algebra*, and all branches of the calculus of Magnitudes by means of *general signs*, in which there is no trace of specialty, or anything that can indicate the particular nature of the Magnitudes. The rules of the Calculus once adjusted to a small number of the fundamental properties of such magnitudes, the Calculus becomes a language—a logical instrument—which works, so to speak, of itself, or without farther attention being necessary than what is required to see that the *rules* be thoroughly obeyed. For instance, *analytical geometry*, after expressing by a preliminary synthesis the characteristic properties of the objects considered, deduces all other properties by the pure force of the Calculus; the special object ceases to occupy the inquirer's thoughts, which are all bestowed on the effort to overcome the difficulties of the Calculus, should there happen to be any. And similarly, *analytical mechanics* is the method of translating into general language the fundamental conditions of equilibrium and movement, so that, after such translation, all else may be deduced by the simple application of the rules of the Calculus. The advantage of analytical methods consists in the generality and regularity of their procedure.

Analytical Geometry. See GEOMETRY ANALYTICAL.

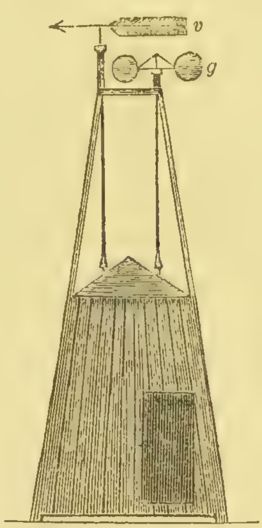
Anemometer; now a most important instrument in all meteorological Observatories. The object, as its name imports, is to measure the *wind*—its *direction*, and *force*. The only one in use, until recently, was Lind's, a small instrument, in which the wind blew into the mouth of a tube, always turned towards it by a vane, and depressed—in proportion to its force—a column of liquid therein contained. The defects of such an instrument are obvious. It could not be very sensitive; but above all it wants the essential power of registering its own indications. Mr. Adie of Edinburgh removed this last objection, and, to some extent, the former, in a statical anemometer on something of the same principle as Lind's. We shall here briefly describe the three modern instruments that seem best adapted to the important requisitions of meteorology:—(1.) The first is Dr. Whewell's. By means of a vane, a small windmill is kept turned to the face of the wind; and by its revolutions, it indicates the wind's velocity. The direction of the vane, and the working of this little wind wheel, thus mark the two important elements, viz., the direction of the wind, and its velocity; and the question remained, how these elements and the changes in them could be registered. The swift movements of the wind wheel were, in the first place, made, by an endless screw, to communicate a vertical motion to a pencil working on that screw (which screw was about $2\frac{1}{4}$ feet long) so that the pencil pressing on a sheet of paper marked the length of its progress during any interval of time elapsing between two observations. This was connected with the element of direction in an ingenious way. Through the centre of the instrument a fixed pillar is placed, of about four inches in diameter, and around this pillar a sheet of paper can be temporarily placed, ruled vertically according to the directions; or so that the pencil aforesaid strikes on its north line when the vane indicates north, or its west line when the vane indicates a west wind, and so with other directions. The pencil thus gradually travels down on the outside of this cylinder, marking, on the various direction-lines, the velocities of the wind blowing in these directions; and when it has reached the bottom of the paper, in proportion the instrument must be readjusted by the removal of the pencil to the top of the endless screw, and the application of a fresh sheet to the cylinder. The defect of Whewell's anemometer as a registering instrument is this:—it does not register the element of *time;* it simply gives the integral velocity of the wind in all the directions in which it has blown during the interval between different notices by the observer. Still, although thus limited, its registering is of great consequence; and, as our first self-registering instrument, this anemometer has contributed importantly to the evolution of various phenomena of the winds. (2.) Superior to Dr. Whewell's, at least in one paramount element, is Mr. Osler's anemometer; the instrument still chiefly in use, and which is always receiving modifications at the hands of its ingenious inventor. Mr. Osler, of course obtained the direction of the wind from a vane. The vane communicates, by a long rod, its own motion to a small pinion wheel within the observatory. The pinion, by means of a ratchet, converts this rotatory motion into a linear one, and the ratchet carries a pencil that strikes on a sheet of paper lying below it, and ruled according to the normal directions of N.W.S.E. This sheet of paper does not remain fixed under the pencil, but is gradually carried on by a clock, so that while the pencil oscillates to and fro, as the direction of the wind shifts, these oscillations are found written on the ruled sheet beneath it, in strict connection with the *time* at which the changes took place. The next requisition undertaken to be answered by Mr. Osler, was to register the *force* of the wind. Immediately below the vane, and attached to it,—of course in this way always facing the wind,—he places a brass plate, one square foot in diameter. To the back of this brass plate is attached a spring, of tested strength; a spring beaten back according to the force blowing on the plate. By means of a wire passing through the rod which communicates *direction* to the observing room,

ANE

this spring communicates with a counter-spring. As the upper spring is beaten back, the lower or counter spring is free to unfold; and as it unfolds, a pencil, placed above the sheet formerly spoken of, starts out and impresses a mark upon that sheet. whose length thus indicates the *force* of the wind. The effect of this registering will be better understood by the annexed copy of one of the sheets. The middle tracing is the line of *direction* for twenty-four hours, and the lower tracing is the indication of the corresponding *force*. Mr. Osler has added, by a very simple contrivance, a third pencil, which writes down the quantity of falling rain. This as recorded appears in the third or upper tracing of the annexed sheet.



The instrument we have described is quite a beautiful one, and extremely complete. It gives, we have seen, the direction of the wind, the force of the wind, and the quantity of rain; a separate pencil records each on a sheet which is carried onwards beneath the pencils regularly by a clock. The observer has therefore merely to supply a fresh sheet every twenty-four hours, and to make numerical reductions of these sheets as they are successively taken off. The main defect of Mr. Osler's anemometer is this:—it does not register with sufficient accuracy light aerial currents; nevertheless, its indications have already thrown much light on the more important relations of the winds. (3.) The last instrument requiring notice here is an anemometer by Dr. Robinson of Armagh, suggested to some extent by the characteristic machinery of Dr. Whewell's, but certainly a great improvement on its prototype. Dr. Robinson was led to the construction of his anemometer by a rigorous analysis of the imperfections of the previous ones. Mr. Osler's, it will be observed, registers the *pressure* and not the *velocity* of the wind. Now the *relative* variations of the pressure of the wind are found to be twice as great in any breeze as those of the *velocity*; so that a pressure-gauge is subject to the most uncertainty. It is laborious also to deduce velocity from pressure; and as velocity is the element we want, a pressure-gauge ought if possible to be avoided. Whewell's gives, or ought to give *velocity*; it does not give it accurately, owing to the imperfections inhering in a windmill vane. Sir William Snow Harris found that the spaces traversed by the recording pencil are not as the velocity but rather as its square. The external form of Dr. Robinson's anemometer is represented in the annexed sketch:—



The wind vane, *v*, indicates direction, as in all cases, but instead of the windmill vane in Dr. Whewell's instrument, a horizontal arm is placed at *g*, carrying at its extremities cups, whose convex sides are placed opposite to each other, so that the power of the wind to make it turn, resides in its effect on the concave side of the cup, over its effect on the convex side. These hollow hemispheres are in fact perfect motive agents, substituted for the imperfect windmill vanes; they are perfect, because the motive effect on them is uniform and unchangeable; whereas, the ordinary windmill arms change in their susceptibility with the angle according to which they lie,—indeed not one of them can without actual comparison be interchanged with another. After numerous experiments Dr. Robinson concluded himself warranted in laying it down as a general law, that in a horizontal windmill of this description the centre of the hemispheres move with *one-third of the wind's velocity*, except in so far as they are retarded by friction. A result of the most important kind, as it guides us for the first time *to a mode of* DIRECTLY *measuring the velocity of the wind.* The machinery by which Dr. Robinson proposes to carry out the registry of his results is fully described in vol. xxii., part 3, of the *Transactions of the Royal Irish Academy.* If the principle of his anemometer is unobjectionable, it must be confessed that there is an im-

perfection in its registries, or power of exhibiting that triple representation of time, velocity, and pressure which so eminently distinguishes Osler's. Meanwhile, meteorology is greatly indebted to the investigations that led its admirable author so far. As to the information concerning aerial currents communicated by anemometers, we refer to the article WINDS.

Aneroid Barometer. See BAROMETER.

Angle. The opening, or the measure of the opening, of lines or planes that meet. This opening, or degree of inclination, of the lines or planes has no relation to the absolute spaces between them; the magnitude of these depending on the lengths of the lines or sides, with which lengths the *angle* has nothing to do.—*Angles* can be compared with each other; one angle being either *equal* to another, or *greater* or *less* than it. Angles also bear to each other the relation of *proportion*. All which, follows from the fact that two angles may be equal to each other, and are so when—the angular point and one side of each coinciding—the other coincides also. Angles, as quantities, are distinguished from lines, as quantities, in this—they have a *natural unit* to which they can all be referred. Lines have no natural unit; there being nothing beyond convenience to induce us to measure a length by *inches*, *feet*, *yards*, or *miles*. But the RIGHT ANGLE is a constant and absolute angular unit; so that angles can always be expressed in pure numbers, representing the proportion they bear to the right angle. Important consequences from this fundamental distinction will be noticed hereafter. (See PARALLEL LINES).—The following are technical definitions of certain kinds of angles very commonly spoken of:—

1. Of plane angles, a *right angle* is that made by a straight line perpendicular to another; strictly defined as one of the two angles which one straight line standing on another makes with it when they are equal. An *obtuse* angle is *greater*, and an *acute* angle *less*, than a right angle.

2. Of solid angles, that contained between two planes (or, which is the same thing, two surfaces, for which planes can be substituted along the straight line of contact) is called a *dihedral* angle. It measures the space between the planes. The angle made by three adjacent planes is *trihedral;* and by many adjacent planes *polyhedral.* The *dihedral* angle reduces to a plane angle. The trihedral and polyhedral are the true solid angles. At the corner of any room a good example of a *trihedral* angle is presented.

3. In astronomy, the *angle of position*—formed by the arcs drawn through a star and the poles of the equator and ecliptic respectively—the arcs along which declination and latitude are respectively measured. *Hour angles*, the angles made by the arc through the poles and a star with the meridian. This changes from hour to hour with the diurnal motion. These are also called *horary* angles. *Angle of commutation*, the angle between two lines drawn to the centre of the sun from the earth and the place of any planet, when reduced to the ecliptic. *Angle of elongation*, formed by two lines from the earth, to the sun and a planet respectively. *Angle of longitude*, the angle formed by the meridian and the circle of longitude of a star, the circles cutting at the pole of the ecliptic. *Angle of parallax*, formed by the vertical and the circle of latitude.

4. In optics, the *visual or optical angle*, that formed by two rays from the centre of the eye to the extremities of an object. The image on the retina is proportional in magnitude to this image, and it is by it that the eye estimates *directly* the size of objects. *Angle of incidence*, the angle contained between the direction of a ray, falling on a surface, and the perpendicular to the surface, at the point where it falls on it. *Angle of reflexion*, the angle of this perpendicular and the reflected ray. *Angle of refraction*, the angle between its continuation and the refracted ray. *Angle of deviation*, the difference of the angles of incidence and of refraction. *Angle of polarization*, the angle which the reflected polarized ray makes with this perpendicular (the *normal* to the surface at the point).

These are the chief, though far from being the only angles referred to, in a technical way, in the different departments of science.

It has been pointed out above, that the right angle is the natural unit of the angle. However indispensable for ultimate reference, however, the right angle is too large for ordinary use. All angles except one (in the limited acceptation usual to the term) would thus be fractional parts of the unit angle. In consequence, the right angle is divided by us into 90 equal parts, each called a degree; each of these into 60 minutes; each of which is again subdivided into 60 seconds. The introduction of the system of decimal measures into France, caused a proposal to subdivide the right angle into 100 degrees, each into 100 minutes, and each of these into 100 seconds. In some books we find this method, but it has not been generally adopted.

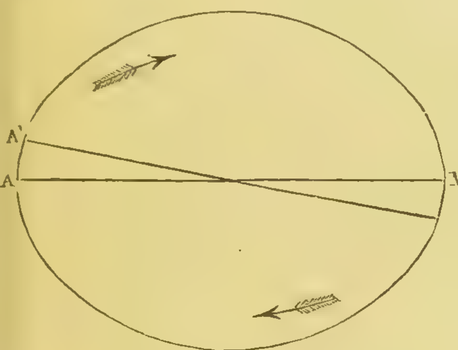
Anion. One of the terms introduced into electrical science by Mr. Faraday; its opposite is *Cation*. By anion is meant that part of the body under decomposition, by or through means of the electric current, which goes to the *anode* side or pole of the current; and by *cation*, the body that goes to the *cathode* side. See next article.

Annular. See ECLIPSE.

Anode. The technical term proposed by Faraday for the side or extremity of the decomposing body, at which the electric current, according to former phraseology, *enters*, in other words the *negative* side. It is the side at which oxygen, chlorine, acids, &c. are evolved. The opposite or positive side — the same author proposes to term the *cathode*—that side at which

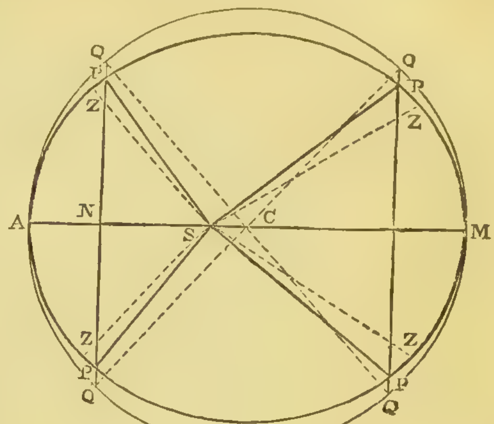
the combustible bodies, metals, alkalies, and bases are evolved. Further on this subject under ELECTRICITY.

Anomalistic Year. So called because it depends upon the position of the apogee, to which we refer the anomaly. (See ANOMALY). If the orbit of the earth were a perfect ellipse, the anomalistic year would be exactly equal to the tropical, or common year. (See TROPICAL YEAR). The axis, however, is, as it were, twisted forwards after the moving body in the direction of its motion, so that after it has completed a full revolution, there still remains the space between the old apogee, where it completes it, and the new apogee, which has moved in the direction of the



motion. The apogee moves, in fact, about 11″·8 in a year, and the moving body has to traverse 360° 0′ 11″·8 in order to complete a revolution of this sort. The anomalistic year is, therefore, longer than the common year, and is 365 days, 6 hours, 13 minutes, 45 seconds. The figure will illustrate this. After the moving body has gone round from A, the apogee, at the commencement of motion to A, back again, it has completed a tropical or common year. In that time, however, the axis has moved forward, and the apogee has taken a new position at A′. The time occupied in passing from A, round the whole circumference and on to A′, is called the anomalistic year.

Anomaly. An angle measuring apparent irregularities in the motions of the planets. They move in ellipses, having the sun in one focus. Let the ellipse A P P M represent the orbit of a planet, S being the focus, and P the position of the planet at any given moment. Then the anomaly—what is called the *true anomaly*—is measured by the angle A S P, A being the point of the perihelion, and P the position of the planet. Again, if the planet be supposed to describe its orbit, uniformly, passing over equal spaces in equal times, its position would be different from what it really is, because the law of its motion is different, except at the aphelion and the perihelion. Suppose that in the time which it takes to describe the arc A P, A would pass according to this law over A Z; then A S Z is what is called the *mean anomaly*. If, again, we describe a circle upon A M as diameter, and draw from P, Q P N perpendicular to A M, joining Q, the point where this line cuts the circle with C, the centre of the circle, the angle A C Q, is called the *eccentric anomaly*. When



we speak of the sun or moon, we conceive them both to describe elliptic orbits round the earth, because, although in fact, only the moon does so, yet we refer all motion naturally to the earth as the fixed point in the universe, and, because by considering the earth at rest, we introduce neither fundamental error, nor increase the complexity of calculation. In speaking of a planet's satellites, we conceive the planet itself at rest; and in treating of double stars, the one is supposed to revolve round the other. The mean and eccentric anomalies are thus, in all cases, imaginary angles, originated for facilitating the use of tables.

Antarctic. See ARCTIC.

Anthelia. *Crowns* in meteorology are circles, one or more, around the sun or moon, and concentric with these luminaries. *Anthelia* are phenomena of quite the same kind, but in the part of the sky opposite to the sun; in that particular they are analogous to *rainbows*. They are seen in this wise:—When the sun is just rising, and the observer is between the luminary and a cloud or fog, he observes around the shadow of his head, projected on the cloud or fog, an *aureola*, or glory, whose brightness gradually fades into distance. In the polar regions the phenomenon is always seen where a fog exists while the sun shines; and upon mountains it occurs when the shadow of the observer is projected on a cloud. Sometimes one, sometimes two, three, and even four bright coloured circles are seen in such cases, and they are so placed that the line passing from the centre of the sun through the eye of the observer, passes through their centre. The fourth circle is seldom seen complete; it is called the *circle of Ulloa*. There is an analogous phenomenon which explains *anthelia*:—when the sun rises behind a hill covered with trees or brushwood, the spectator in the shadow of the hill may observe all the small branches projected on the sky, not

opaque or black, but brilliant and silvery. These small branches act in the same way as the globules of vapour in the phenomena of *crowns* (*q. v.*) and *anthelia*. This action is termed *Diffraction*, *q. v.* To the same cause is referable the beautiful colour of spider's threads, and of the motes in the sunbeam.

Antichthones. People that dwell at two parts on the surface which are however equal in latitude. Their seasons will of course be reversed, the winters of the one being the summers of the other, and so throughout.

Antipodes (ἀντι, πους, feet opposite). A term applying to the inhabitants at two opposite extremities of a diameter of the globe. It is clear, then, since degrees of latitude are measured north and south of the equator, and the intersection of a diametral plane, with the surface (or the circumference of a circular section of the mass of the earth), divides it into two equal parts, that the antipodal places must have the same latitude, the one being, however, north latitude, and the other south latitude. Again, if longitudes be measured for half the circumference eastward and westward, it is manifest that these places must be always the one in east, the other in west longitude, unless they be on the zero meridian line, and that the sum of the number of degrees representing their longitudes will be 180°. If we reckon longitude all round the globe, as we commonly do now, the difference of the degrees of longitude will be 180°. Everything will naturally be just reversed in reference to the inhabitants of places, the antipodes of each other. The noon of the one is the midnight of the other; the longest day of the one, the shortest of the other; and they have summer in the one place and winter in the other. The average length of day in both is of course the same throughout the year. The opinion that there were antipodes was a fruitful subject of ridicule in ancient times; and in the middle ages it was deemed heresy to hold it. Columbus had, perhaps, as much difficulty in procuring any support in his expedition on this ground as on any other. "Tempora mutantur, et nos mutamur in illis."

Antiscii, or **Antœcii** (ἀντι, σκια, or ἀντι, οἰκεω). When any distinction is made between the two words, the former is the more general, referring to persons living in different hemispheres, and at similar, though not equal distances, from the equator. The Antœcii are those who live at equal distances from the equator (same number of degrees of latitude north and south), and under the same meridian. The sun, therefore, passes their meridian at the same moment. When it is summer with the one also, it is winter with the other, and *vice versa*.

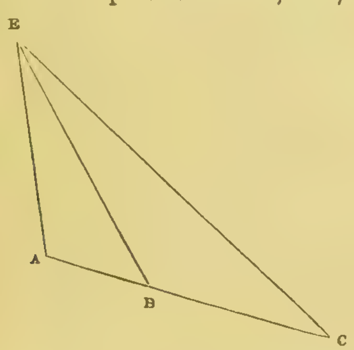
Aphelion. The greatest distance of a planetary body from the sun is called its aphelion (ἀπο, ἡλιος, from the sun). The aphelion of the earth and the apogee of the sun are the same. The aphelia of the planets change their position in successive years. The axis of the planetary orbit moves, as we have seen (Anomalistic Year), and the aphelion, which is just its one extremity, moves with it. It was usual to measure the anomaly from the aphelion, but the aphelia of the comets are not generally visible, and therefore, to secure uniformity of measurement, the perihelion has been adopted as the point of departure in all cases.

Apogee. The point of maximum distance from the earth of any heavenly body. It is only important with regard to the sun and the moon. The apogee of the former occurs at the same time as the aphelion of the earth. The sun is often considered as moving round the earth. The moon does move round the earth. The apogees in these two cases move themselves, but move regularly. The apogees of the other planets move very irregularly. The motion of the earth has in their case to be compounded with the motion of these bodies themselves. We are in apogee about July 1st. The progressive movement of the sun's apogee is very slow. The moon's apogee moves rapidly and completes a revolution in 3232 days, or nearly nine years. That is about 6′.41″ per day. The fact is very important in the theory of tides.

Apparent Magnitude and Figure of Bodies is different from their real magnitude and figure. A straight line for instance, drawn horizontally in the air, will appear to us circular. Sometimes, again, a straight line, when it all lies in the line from our eye to any one point of it, appears as a simple point. A surface, again, in which, if extended, the eye itself would be found, would appear to us as a line; and a solid body always appears to us a mere succession of surfaces, sometimes so seen that we believe that the body is solid, but if at a great distance not readily to be discerned from other surfaces. An angular or irregular body, again, appears always at any considerable distance more regular than in fact it is, because its little projections and irregularities, subtend very small angles to the eye, so small indeed, as easily to become insensible. So a long line of lamps seen from a distance appears only one continuous blaze, from the gradual diminution of the optical angles subtended by the distances between them. In astronomy, the *apparent magnitude* of an object is the angle which it subtends at the eye. It will therefore, abstracting refraction, which operates in all cases, diminish in inverse ratio to the distance. It will be evident also, even without mathematics, that it will be largest when the body is nearly perpendicular to the line of direction from the eye, and will vanish, becoming a point, if it were to appear in that line of direction. The heavenly bodies, however, being solid, this last consequence could never happen. Were they perfect spheres, we would not require to take into account the first cause of error. In measuring the apparent magnitude of stars, or of the sun and moon, we must recollect that their different points are subject to different influences from refraction, and to a very slight extent from

aberration and parallax, and that sometimes a body appears larger in one dimension, and proportionably smaller in another, than it really is.

Apparent Motion is the motion which a given body seems to us to have. Now, we refer all motion naturally to the eye. If, then, it be itself in motion, as when we travel in a steamboat or railway carriage, we consider a body actually in motion, conceiving the eye to be at rest. The consequence is that we combine the two motions of the eye and the body into one, which we attribute entirely to the body in question. Not that we always add or subtract the two motions, for this we only do if the lines of motion be parallel, and if the directions be opposite or the same; but we compound them together as we do two forces, and attribute the resultant to the body which is in reality moving only with the one velocity. Even, however, when the eye is at rest, there is a distinction between the real and apparent motions. Motion is, of course, measured by the spaces passed over, and the eye has no direct measure of these spaces, but by the angular distances of the optical lines. If, then, a body



move from A to C, the space from A to B will appear equal to that from B to C, supposing the angles A E B and B E C equal to one another; and if the body be moving uniformly it will take longer to describe B C than A B, and since these appear equal, will appear to move slower in that part of its motion.

Apparent Position. When we want to simplify some observed phenomenon—by taking into account the effect which some cause with which we are acquainted will have in modifying it, or when we want from a true observation, at a particular point, to discover what would have been found by observers at some station of reference—we call our original phenomenon *apparent*, and the resulting and reduced phenomenon *true*. The name in one application of it is perhaps improper, but it is warranted by use. Thus, when we observe a star's position, we subject our observed result to various modifications before recording it. There is, in the first place, refraction, by which the star is made to appear higher above the horizon, than it would otherwise appear. We require, therefore, to allow for this, and to place our star nearer the horizon, than our observation would lead us to believe. Then the aberration of light alters the direction of the ray which we see, from the true straight line from our eye to the star, and we must therefore take into account this cause of error. Nutation again changes the apparent position of the star, and we must rectify our observation for this also. By allowing duly for these, we will be able to arrive at the true position of the star from our point of view. But this observation would be only of use to ourselves, to compare it with subsequent observations from the same point. We therefore deduce from this the star's apparent position as seen from the centre of the earth, and so, as all observers reduce their observations in this way, the work of each is valuable to all. The *apparent place* of a star, then, is its place as observed at first, subject to all these causes of error, and its true place is when we have reduced our first observation, to obtain the direction of a straight line drawn from it to the centre of the globe. The apparent horizon is a good illustration also of the modifications introduced by astronomers, to render all observation valuable to all. The sensible or apparent horizon to a man at the surface of the earth is the circle, in which a plane, tangent to the surface of level water at his position, would cut the imaginary celestial sphere. If, again, he be a little elevated above the earth, the apparent horizon will be the circle, in which a cone touching the earth's sphere, and having his eye for the apex, would cut the celestial sphere. The real or rational horizon is the circle in which a plane parallel to the tangent plane at the surface, or immediately under an elevated position, would cut this sphere. The apparent conjunction of the planets, when the line from the eye passes through their centres, as distinguished from their real conjunction where the centres of the earth and of the true planets are in one straight line, is another illustration of the same necessity of correcting observations.

Apparent Time. See TIME.

Approximation is used in physical science to express a result which we are justified, according to the laws of probability, in taking as the result of a certain observation. Thus, let us observe the place of a star upon various occasions, and at various places, such, that it ought to be observed in the same position if no disturbing cause intervened. If we know the nature of these disturbing causes, we will observe so as to have them act in different ways, and, as far as we can guess, neutralize each other. Owing to disturbing causes, then, in external nature, and owing to the imperfections to which all quantitative observations are liable from the limitation of the observer's mental and physical powers, we shall have different results. The probabilities are, that these external and personal disturbing causes will act as often in increasing as in diminishing the result, and, in consequence, we approximate to the true result by taking all the

observations together. The most usual method is by merely adding all the numerical results obtained, and dividing by their number. This approximation, if grounded on a sufficient number of observations, may be safely relied upon. Approximation, mathematically considered, arises from a different cause, the inadequacy of any numerical system to represent every kind of quantity. Thus, in attempting to find the numerical value of the side of a square or cube, whose area or volume is given numerically, we are often obliged to use mere approximations to the truth. The same will be the result of attempts to find numerical values satisfying equations of the higher orders in most cases. Generally, however, in this approximation we are able to assign the *limits of error* with perfect mathematical certainty. This, in the other case, we are unable to do, although we can attain to mathematical certainty as to what result of a series of observations, we are bound, as rational beings capable of calculating chances, to employ.

Appulse. When the moon passes very near a star, there is said to be an appulse. The precise moment of its occurrence is very carefully observed. We find it of the utmost use, when we wish to determine very accurately the longitude of a place, and when we wish to correct the errors of tables, to know the exact moment of appulse.

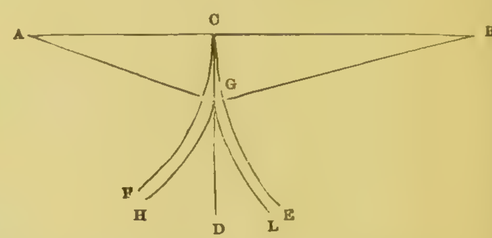
April. The fourth month of our year. The sun during this month is passing through Taurus. The word is derived from *aperio*, from the *opening up* of the treasures of the earth after the passage of winter.

Apsides. Those points of a body's path where its motion is at right angles, to the line to the centre around which its motion exists. They are the same points as the apogee and perigee, the aphelion and perihelion.

Aquila. A fine constellation in the northern hemisphere.

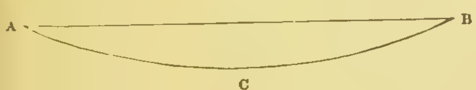
Ara. A constellation of the southern hemisphere, under the tail of Scorpio. It contains three stars of the third magnitude. The myth is, that it was the altar on which the gods swore fealty to Jupiter, before the war with the Titans.

Arch. The theory of the equilibrium of the arch, is one manifestly of great importance to the practical builder. Frequently cases occur in his practice where it is desirable that a solid road should be made over some impassable spot, without completely building up the included space. If he should attempt to throw a straight rod, or line of way over this space, its own weight frequently, and almost certainly the passing of any considerable mass in addition to that above it, would break down the structure entirely. Every part of the bridge between the solid piers on which it would rest, would be solicited by a downward force of gravity, due to its own weight. The part above the piers could not obey this solicitation, and so there would be a force endeavouring to turn the different parts of the bridge round these different points. Thus at any point in the middle, suppose C, in the figure, there would be a downward force due to the weight of the mass, C. Let us

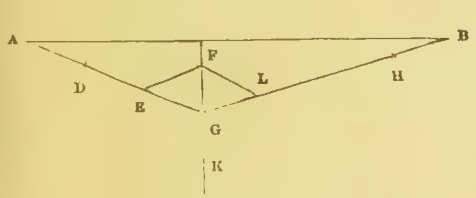


forget for the present that the whole of the line, A C B, is composed of parts quite as heavy as C, and examine the result there, if A C B were simply a straight rod without weight, which sustained the heavy mass, C, upon it. It is evident that the same result with certain modifications increasing its effect, would occur at all the points of A C B. Let us suppose now that the part B C is removed, and that only A C remains, with the mass, C, attached to it, A being a fixed point to which the stiff rod, A C, is attached. The mass, C, tends to fall straight downwards in the line, C D. But that it cannot do. It moves under one condition, that it shall always be at the same distance, A C, from the immovable point A, to which the inflexible rod, A C, attaches it. In fact it would just be as if we described a sphere round A, with A C for its radius. The circumference of the sphere includes every point in space at the same distance from A that C is, and so far as the connection of C through the inflexible rod with A is concerned, the ball, C, cannot move but along the circumference of this sphere. Then again, gravity always takes it straight downwards. It can only move then in a downward direction—right down vertically, along this sphere to which the rod fastens it. So, suppose that the plane of the paper is vertical to the horizon, the ball C, must describe whatever motion it can in that plane, and also in the sphere; therefore manifestly in the vertical circle, A C F. If we suppose A C similarly taken away, and B C a rod inflexibly fixed to the immovable point B, C could only move along C E. Now, if A C B were perfectly rigid, that is, if B C or A C were stiff enough absolutely to produce all the effects which are destroyed in their removals, the point would remain quite steadfast at C. But they are not so. B C tends to act a little as if it had been removed. It does not do the full complement of work in resisting the motion along C E that a quite inflexible rod B would do. Neither does A C in resisting the motion along C E. Seeking therefore to move in both directions, the body C moves in a sort of middle one and goes down, suppose to the point G. This line, C E, through which C falls, will quite evidently be longer or shorter as the beam is looser or stiffer, less or more flexible. If the

beam were quite inflexible it would be absolutely nothing. Take the new circumstances then. In falling to the point G, the mass has obeyed neither of the laws which bound it to the spheres, because it ought to obey both, which were contradictory. It has nevertheless fallen to G. The masses A C and B C then have become distended to A G and B G. They have in fact been each pulled out and consequently weakened. The cohesion of their particles has been so far diminished, and they are less able to bear new weights than before. This tendency to sink and to damage the strength of the mass, and ultimately to tear it possibly asunder, has been acting at all the points of A B, and not alone at C: we can fancy then that the mass, A C B, will take the lengthened out form represented in the figure, in consequence of its weight.



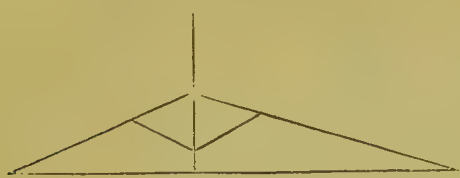
Again, when the straight rod, A B, has reached this form, A C B, is it any more secure? Unless somehow or other new forms have been brought into play by this curvilinear shape, it is evidently not one whit more—rather something less. Every part of it possesses the same weight as before, and that tends to carry each part downwards in a new circle, G H and G L, and the resistance which H G and G B can offer, the amount by which they are inferior to perfectly flexible rods is less than before, generally, with some bodies not necessarily, however. We must examine, then, this new case, because if we find that no new forces are introduced by the curvilinear figure, it will be clear that no straight roadway can bridge over a space, and also that no hanging chain, like A C B in the last figure, can do it. We find that they can in practice to a certain extent, and we must examine therefore where their source of power is—how they are able to resist this continual dragging downward inseparable from them in virtue of their weight. To take up again the case, therefore. Let A G B be the form of the line to which the weight, G, hangs as before.



Let us adopt a standard of length for the representation of force, and suppose, for example, that G K represents in length, as it does in direction, the force of gravity. This then acts upon the mass, A G B, and the ball, G, and will not permit rest in consequence of the imperfect inflexibility of A G and G B, unless some force contrary in direction and equal in magnitude to G K be somehow evolved, or some forces equivalent to such a one. Let the line G F (K G produced) represent this. Then, as we can only conceive of forces acting in matter, we can only obtain these required forces along the rod, A G and B G. According, therefore, to the proposition of the composition of forces (see FORCE), if we draw F E and F L parallel to B G and A G, forces which would be represented in magnitude and direction, according to the standard we have adopted by G L and G E, would be equivalent to G F. If then such forces can be, and are called out in the circumstances, there will be an equipoise to the force, G K, representing gravity and equilibrium so far as that goes. There would be equilibrium therefore thus so far as G is concerned. But as regards A G and B G, would there also be equilibrium with those forces which are thus called out in these rods alone acting? And is there not besides in fact an equilibrium as regards these rods? Why do these forces not send the pieces, A G and B G, away upwards through the air, tearing asunder the connection at G? Evidently because the rocks are there to prevent it. The points A and B are fixed points, and they prevent it. G A and G B are pushed against them by these forces and away, but they give back a force equal and opposite, and represented by A D (= G E), and B H (= G L) in direction and magnitude. They therefore counteract the forces, G E and G L, and there is equilibrium there also. There is therefore a pulling force exercised on A and B respectively, A D and B H, and there is again a tension equal and opposite to that, and keeping the rods in equilibrium. These latter pair of forces are, however, required for the counteraction of the weight G, and the result therefore is, that the three, G E, G D, G L, produce equilibriums at G, while the forces D A and B H, pulling A and B downwards, are transmitted from G, the origin of the action. If, therefore, the bars A G and B G are strong enough to resist the disintegration which the transmission of these tensions might produce, if such a force, first acting at G, and transmitted by its cohesion to the next particle, and by it to its neighbour, and so on till A and B, find no place in the mass of the rods where the cohesion is a smaller force than itself, and therefore incapable of resisting it, there will be equilibrium.

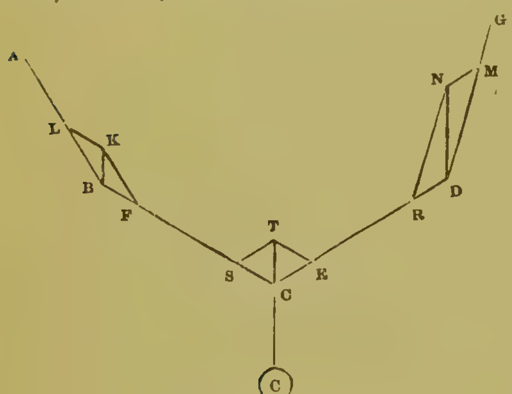
When once the equilibrium is established, it will evidently not at all matter how the relation which the whole arrangement holds as regards any external object, *e.g.* the horizon, be changed, if there be no change thereby produced in any of the acting forces. Let us suppose that the figure which we have described is turned downside up, and that it makes, in consequence, such an appearance as in the figure at the top of the opposite column. All the circum-

stances of import in the case will be quite reversed. In order that this reversal shall be



complete, however, it is evident that the force G K acts upwards, instead of downwards, and is, in fact, a prop now instead of a weight, when the equilibrium is established.

We shall take a more complex case, however, of the equilibrium of chains, which, as more readily intelligible to the learner, we thus take, as it rests on quite the same principles, as introductory to that of arches. Suppose that a weight, C, is attached to the point C, belonging to a string, A, B, C, D, G, which is incapable of stretching, or, in fact, of motion, and that we want to find the tensions that act along the string, by which the equilibrium is kept up, and the weights that must be hung at the other points, B and D, for this purpose.

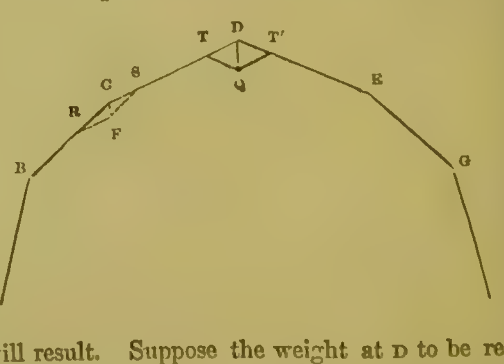


Let C T, the equal and opposite strain, be compounded of C S, C E. Then the tensions S C and C E, in the strings B C and C D are needful to preserve equilibrium at C. But not only there is equilibrium necessary. It must be also kept in B C and C D. There must, therefore, be equal and opposite tensions. There must be a force, B F = C S, and a force D R = C E. Let these, then, be measured off. Now, if weights be at all hung at B and D, the direction of their counterbalancing forces must be vertical, because they themselves must hang vertically. Draw B K and D N, therefore, vertically through B and D, and draw the lines F K and R N parallel to B A and D G. Complete now the parallelograms F L and R M. Then the force of gravity acting at B, *i.e.* the weight to be put at B, will be represented by the length B K, and that at D by D N, and the tensions acting on A and G, the fixed points, by B L and D M. Hence we have been able, given the figure which the polygon is to assume, to find the weights which must be suspended at all but one of the angular points of it, and the tensions by which its strings will be stretched.

The exact converse of this proposition depending, therefore, on the same methods of proof, would be — given the strain on the prop at C (the whole figure being inverted upward), required the strain on the props that must be placed at B and D, in order that there may be an equilibrium in the whole system.

It is evident that we might carry on this method of discovery, whatever might be the number of the points, B, C, D. If there be more of them, it would just be necessary to repeat the process the oftener, but on the same principles exactly. If there were so many that the whole space between A and E was so filled that it would be scarce possible to distinguish the difference from a curve, as in a chain of very small links, where each link is a straight line, yet, from their closeness the whole wears the appearance of a curve, there would be no difference in the principle. Finally, if we suppose the lines corresponding to the links (*i. e.* the sides of the polygonal line) to be infinitely small — smaller than anything which we can imagine — in fact, a curve, the same thing exactly would hold good. Given the form of the curve, and the weight which it was to bear at one point, it would be possible to tell how much must be borne at every other point. If, instead of having one of the weights thus given, such a condition connecting them were to be given as this: — Required the form of the curve which would be that of equilibrium when the weights would correspond to the lengths of the infinitely small polygonal sides, a special curve would be found out of the name of the *catenary* (*q. v.*) to satisfy these conditions.

The inverse of all these problems on chains or rods, where weights fall down, would be like problems on rods which props hold up, which props, therefore, sustain a certain amount of measureable pressure. Suppose, now, we consider such upright polygonal lines, where there are weights acting, as in the suspended ones, and where no props are brought into play. Let a given weight be placed at the point D; it is required to throw what weight will have to be put on at the points C, B, G, E, so that equilibrium



will result. Suppose the weight at D to be represented by D Q in direction and magnitude.

Then since D Q can be decomposed into forces D T and D T′, opposite and equal forces to these can neutralize. Suppose, therefore, a force in the direction T D, instead of D T, a force of equal amount, in an opposite direction, must act on the bar C D. Let it be S C. Then draw C P perpendicular as before, and S P parallel to C B, and complete the parallelogram P R C S. Then R C will represent in direction and magnitude the tension on B C and C P the weight required. So that equilibrium may be produced, the tension in C D meanwhile remaining equal to T D, one of the components of D Q. Carrying the same principle round to all the points, we can evidently get values for the weights to be placed at B, C, E, G. We shall seek then to gain some more accurate formular notion of the value of C P, which we can thus obtain by geometry.

It is a well-known trigonometrical proposition that the sides of a plane triangle are proportioned to the sines of the opposite angles. Our readers will find this demonstrated, if they do not already know it, in any work on Trigonometry, and in most on Geometry. Hence D Q : D T :: sin. D T Q : sin. D Q T. Now, sin. D T Q = sin. T D T′, because T D T′ and D T Q are supplements of one another. Also, since D Q T and Q D T′ are equal, sin. D Q T = sin. T′ D Q.

Hence the formula takes this form:—

D Q : D T :: sin. T D T′ : sin. Q D T′.

Now T D = C S.

Hence D Q : C S :: sin. T D T′ : sin. Q D T′.

Again, C S : C P :: sin. C P S : sin. C S P, or by the same reasoning as before,

C S : C P :: sin. B C P : sin. B C D.

Multiply the ratios; D Q : C P :: sin. B C P × sin. T D T′ : sin. Q D T × sin. B C D.

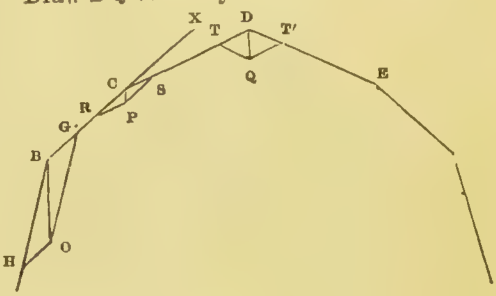
Or, D Q : C P :: sin. B C P × sin. C D E : sin. Q D E × sin. B C D.

Now, C P is the only unknown quantity of all those here given. D Q is known, as representing the weight; C D E and B C D are angles of the adjacent sides of the polygonal line, and the other angles, B C P and Q D E, represent the angles made with the successive sides by the vertical, which is a datum also given. It becomes possible thus, given the curve in the sort of arch that is to be described, and the weight that is to lie at one point of it, to find the weights that must lie upon the others.

This is one form in which the problem of the equilibrium of arches frequently comes before us. Given a certain arch or bridge of a curving form, and supposing a certain weight hanging on one part of the arch, what weight ought to be attached to the others to keep equilibrium? Or, as generally, a weight is attached to some and not to others, it is required to know what weights these are whose downward force the cohesion of the bridge materials must be able to resist, so that when a certain weight is laid on one part of the bridge, it may not break down at any part. This problem is, given the bridge, and the weight at a certain point, required to find the strain at all other points of the bridge. The problem, however, more usually presents itself in the inverse form when the bridge is to be built, and knowing certain data of the weights which it will have to resist, we seek to discover the form which we ought to give.

Thus, let D be given, and the weights represented by D Q, C P, B O, and also the direction of D C, and D E, it is required to find the figure.

Draw D Q vertically between C D and D E, and



complete the figure D T Q T′. Make C S equal to D T, and join S P. Draw C B parallel to that and P R parallel to C S. Then making B C equal to the given side, make B G = C R, and B O vertical equal to the given length. Join G O, and draw B A parallel to it, and O A to B G, and so on.

Following the same method, for the sake of discovering the unknown quantities here, that we knew before, we have—

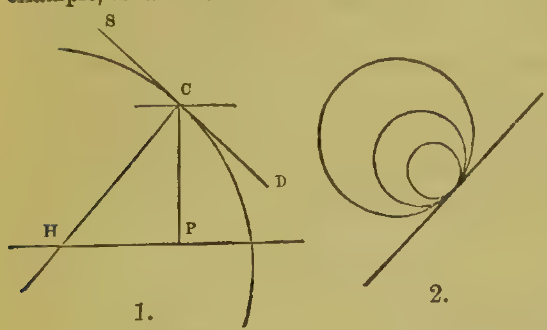
$$\mathrm{D\,T} = \frac{\mathrm{D\,Q.\ sin.\ Q\,D\,T}}{\mathrm{sin.\ C\,D\,E}}$$

This gives T D or C S. Then C S is known, C P is also known, and the angle S C P is the supplement of T D Q, and, therefore, is also known; so that thus, by trigonometrical principles, we can easily find S P or C R (*Euc.* i. 34), and C S P, which (*Euc.* i. 29) is the supplement of R C S.

These, then, are the two problems, inverse one to the other, with which the architect has to do. Given the bridge—what strains will be brought on it, all its parts, by the placing of a certain strain on one; and, secondly, given the utmost strain which each part will require to bear to discover a form of arch—one or two data as to its character at the crown of the arch (*e.g.* the angles T D Q and T′ D Q) being given—which shall give equilibrium under the pressures to which these strains are due.

In the usual case, the form which it is desirable to give the arch is that of some curve, or rather of some polygonal line, very nearly approaching a curve, and whose peculiarities can be best investigated on the principles of the differential or integral calculus, by supposing it a curve. In that case, the angle X C D, which is the supplement of B C D in the last figure, becomes the angle between the tangent to the curve at C, and the curve itself. This angle

between a tangent to a curve, and the curve itself, the angle of *contact*, as it is called, varies as the curvature of the curve varies. For example, if we describe two or three circles all



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touching one common tangent, it will be manifest enough (fig. 2) that the angle of contact is much greater in the smaller than in the larger.

Now, there is supposed to be a circle, which coincides the nearest possible with every curve at each point of its curvature. It is merely supposed, for the sake of assisting our notions of curvature, by referring them to the readily intelligible curve of the circle. However, these circles of curvature are supposed through the given point, and two points of the curve indefinitely near it, and, therefore, do closely coincide with it, and represent its curvature at the point. The radius of the circle is called the radius of *curvature.* Now, the angle of contact of which we have spoken, corresponding to X C D in the last figure, varies inversely as this radius of curvature does—that is, is reciprocally proportional to it. Also, the angles P C B, P C D, in that figure are equal to P C D, P C S, in this, because the tangent at a point is indefinitely near in direction to the very small sides of the polygonal figure substituted in idea for the curve, and its direction may, without error, be substituted for theirs. Now, P C D and P C S are supplements, and their sines are therefore equal.

Now, we saw already, in the proof above that where the same circumstances, known and unknown, were considered, with only a different set, known and unknown, from these in the last case, that

$$\text{D Q} : \text{C P} :: \sin. \text{B C P} \times \sin. \text{C D E} : \sin. \text{Q D E} \times \sin. \text{B C D}.$$

$$i.e.\ \text{D Q} : \text{C P} :: \frac{\sin. \text{C D E}}{\sin. \text{Q D E}} : \frac{\sin. \text{B C D}}{\sin. \text{B C P}}.$$

That is, each weight is directly proportional to the sine of the polygonal angle corresponding, and inversely to that of the angle of the polygonal side, adjacent with the vertical. The polygonal angle is the supplement of this angle of contact, and its sine is therefore equal to it. With two adjacent sides, the weight inversely proportional to the sine of this angle of the tangent, with the vertical, would give quite correct results; but in considering two remote sides, another element has to be introduced, which we eliminated from consideration, rightly in the case of the two adjacent sides, and the full formula would be—

$$\text{D Q} : \text{C P} :: \frac{\sin. \text{C D E}}{\sin. \text{B C D}} : \frac{\sin. \text{P C D}. \sin. \text{Q D E}}{\sin. \text{B C P}. \sin. \text{Q D E}}.$$

In the case of the two adjacent sides actually represented in that figure, sin. Q D C and sin. P C D are equal, and may, therefore, be struck out of the proportion. In the case of the curve, where, having lost sight of the sides from their indefinite minuteness, we ought no longer to consider any two as adjacent, we must not omit them. Remembering, then, that B C P and P C D are supplemental in the case of the curve and their sines equal we find, that the weight varies as the fraction

$$\frac{1}{\text{Radius of curvature} \times (\sin. \text{S C P})^2}.$$

This gives us the actual weight which may be put on at each spot. But it is desirable to know, instead of that, the actual thickness up to which we may lay on a uniform material, instead of as thus. Now, the actual weight of a certain length, near any given point, will not all lie pushing down with the same force, as if at the top, when on the inclined part of the curves. In fact, an equal mass of stone at the curves, and on the side of the curve, would clearly not hang on an equal length of the curve at all, but on a length, which would be inversely proportional to the sine of the angle S C P at the place. Hence it would be necessary to heap up the stone in this proportion, and the height C P might be represented by the formula

$$\frac{1}{(\text{Radius of curvature}) \times (\sin. \text{S C P})^3}.$$

Again, P C S = complement of S C H, and therefore sin. S C P = cos S C H, and the formula becomes

$$\frac{1}{\text{Radius of curvature} \times \cos.^3 \text{S C H}} = \frac{\text{Sec}^3 \text{S C H}}{\text{Radius of curvature}}.$$

The methods of finding the radii of curvature, upon which the theory of equilibrium of arches is thus seen to depend, rest upon principles belonging more peculiarly to the higher mathematics, and are technical; but we trust that, so far as we have gone, the reader, with a little trigonometry, or taking for granted the trigonometrical formulæ which we have required to state, will not have found very serious difficulties.

Taking this formula, then, for granted, we

shall compare the different parts of the arches, one with the other. The weakest place we should naturally expect to be the crown of the arch. All the masses on each side tend to pull it down, and it is least supported obliquely of any. This we actually discover generally in the arch. At the crown, the angle S C H becomes $= 0^0$, and its secant, therefore, equal to 1. The quantity, sec. 3 S C H, is then at its minimum value. We cannot compare, however, the other quantity included in the formula, until we know the special curves.

Again, comparing the strength of two arches, it is evident that it will be sufficient to compare the weights which each can sustain at this their weakest point. If a pressure be brought on the weakest part greater than it can sustain, then no matter how great the capabilities of other parts of the arch may be, it will fall. Comparing, then, the weights at the crown, we find sec. ^{3} S C H to be equal to 1 for both, and, therefore, the strengths are respectively in the inverse proportion of the radii of curvature. To take a practical instance of this, the radii of curvature to arches of the following curves, each with a span of 100 feet, and a rise of 40 feet, are given in the table:—

Segment of a circle	51·25
Parabola	30·125
Ellipse	62·5
Hyperbola	37·417
Catenary	36·9

Hence the parabola, having the smallest radius of curvature, can bear the greatest weight at the crown, and the ellipse, having the largest, the least. In accordance with this fact, it is found that there are very few elliptical arches of large span, in proportion to their height (in this instance the disproportion is not nearly so great as it frequently is), where the crown has not sunk a certain distance. Where the arch is so low, the circle that it resembles becomes very large—*i.e.* the radius of curvature is very large.

The formula, then, which we have found

$$\frac{\text{Sec.}^3\ SCH}{\text{Radius of curvature}},$$

expresses the weight of a superincumbent mass of uniform substance, as masonry, under the pressure of which all the parts of the curve will be in equilibrio.

It is easy, therefore, by mere application of this to a given curve, whose laws are known, to solve this problem: – Given the *intrados* of a bridge (*i.e.* the inner curve), to find the

height of the superincumbent wall, or *extrados*, at each point, so that under its pressure there may be equilibrium. We might go along, taking one given curve (see fig.) marking off at each point of its length, the values of the formula

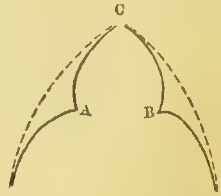
$$\frac{\text{Sec.}\ 3\ SCH}{\text{Radius of curvature}},$$

and so, joining our consecutive points thus obtained, coming to a polygonal line, while the closer our points were brought (as for a perfect solution they would need to be indefinitely close), this line would become more and more curved, would give nearer and nearer the complete curve of the extrados.

The inverse problem, to find the intrados when the extrados is given, is frequently also of very great use. It rests on the same principles, and we shall, therefore, not give any further account of it here. The one is required when we require a special form of arch for the transit of vessels, and when it is not of essential importance what sort of passage way there may be, so that there be one at all. The other is of chief importance, where the bridge is to be used only as a roadway.

There are various other matters of considerable importance in the theory of arches, which the very limited space at our disposal compels us to pass over. A number of the practical details connected with it are given in the article on BRIDGES, to which we must refer. For the theory of the Arch, the reader not acquainted very intimately with mathematics may refer, with great advantage, to Gwilt's treatise on the subject, to which the present article is much indebted; and, for the fuller investigation of it, the admirable article in the new edition of the *Encyclopædia Britannica* will supply ample means. See SKEW-ARCH.

Arch in Architecture. The arch, as a mode of passing from one pillar or upright support, in Architecture, to another, has had in practice various forms; and these are not uncharacteristic of Architectural styles prevalent at various epochs. The present Dictionary not including matters relating to the Fine Arts, we shall simply specify that the Romans put in practice the round arch and the vault. The *pointed* or *Gothic* arch, succeeded to the circular one; out of this grew the *foliated* arch, or arch of the form below, in which the foliations A and B were originally meant to give the arch greater power to resist a vertical pressure applied at C. Various modifications, in the way of ornament, grew out of this simple case of *foliation;* but into these we shall not enter.



There is further the *ogee* arch, or arch of double curvature, as below. This arch has many ornamental modifications. See ARCHITECTURE, DOME, &c.

Archimedes, Principle of. We owe to the immortal Syracusan the establishment of the principle which may be termed the foundation or corner-stone of rational statics, viz., the principle of *the equilibrium of the lever.* The principle itself is now well known; it is, that a lever, loaded with two weights, on opposite sides of the fulcrum, is in equilibrio when the weights are inversely proportional to the length of the arms at whose extremities they respectively hang; to which may be added, as a supplement, that the pressure on the fulcrum of the lever is then exactly the sum of the two weights. The demonstration given by Archimedes is a very simple and fine one. Claiming it as an axiom in mechanics—either as self-evident, or a result of universal experience—that the lever will be in equilibrio when two equal weights hang at the ends of equal arms, he first takes the case of *commensurable* unequal weights; and, by the artifice of dividing the two into different numbers of equal weights, and transporting these by equal distances to one side and the other of the lever, he easily arrives at the general conclusion. The theorem is then extended to the case of *incommensurable* weights, by a species of reasoning familiar to all students of Euclid. Various authors have criticised the process of Archimedes, with a view to simplify it, or to remove fancied defects. It cannot be said that much good has followed, at least if we except the efforts of Stevin and Galileo. There are some fine remarks on the whole subject in the preliminary chapter of the *Mecanique Analytique.*

Archimedes' Screw. A machine for raising water, consisting of a pipe rolled round a solid cylinder. Sometimes we have a spiral groove cut in a solid cylinder, and a water-tight cover put over the cylinder. This arrangement makes the tube semicircular. It is less liable in this way, however, to accident, since the screw is not exposed. The cylinder is placed in water, so that the angle which the axis makes with the vertical be greater than the angle at which the thread of the screw is inclined to the right section of the cylinder. Let the reader take a pencil, or any similar cylinder, and mark a spiral round it with his pen. Place it then in the position we have specified, and let him suppose the figure before him to represent the Archimedian screw. Suppose it then just touching in water, the open end of the screw being just covered. It is clear that the water entering the screw at the open end, A, will *descend* the tube by its weight, until it reach the point where the screw again ascends, and, by the principles of hydrostatics, it will ascend so far above this as to be on a level with the outside water. Turn the pencil then on its axis. Then all the water in the descending portion of the screw will press downwards, and it will occupy a complete semi-circumference of the screw. All the water in the ascending portion, however, will require to be pressed up, but as it does not occupy so much space, in consequence of the screw having an upward tendency, it will not press against the other pressure, so as to prevent any effect from it. On the contrary, it will be pushed upward, and more water will enter at the open end. If the screw keep constantly turning, this process will keep constantly going on, and that in every turn of the thread. The water constantly will be sent rapidly through the tube upwards to the end of it, and can there be made available for any special purpose. The turning of the axis, in fact, just serves to repeat the action of gravity upon the water over and over again, and by this machine it is made to raise water effectually by pulling it downwards. We mentioned that the angle of the axis of the screw to the vertical must be greater than that of the thread of the screw to the right section of it (perpendicular to the axis). It will be readily seen that the angle of this section to the horizontal is the same as that of the axis to the vertical, and that if it were less than the angle of thread to this section, the screw would rise throughout its whole course, and none of the water would flow into it at all. The screw may, of course, be used horizontally, but no mechanical advantage is obtained by the carriage of water from one horizontal position to another. It is sometimes, however, found convenient. In the explanation which we have given, we said that the screw is just dipped, and no more, into the water. In practice, a good part of the screw is sometimes immersed. The explanation is the same as before, and we have only used the limitation in order to render the matter as clear as possible.

Architecture:—in its full generality, the *Science of constructing buildings suitable for specified purposes.* But the natural and laudable disposition of men to ornament buildings, erected with pains and labour, has converted Architecture from a portion of statical science into a chief department of *Æsthetics.* Nay, it is that portion of æsthetics to whose memorials we may the most surely appeal, in illustration of the taste, the genius, the manners even, of past Ages. But into this mode of considering architecture, we can in no wise enter here. Suffice it to say, that its three grandest monuments are unquestionably the Hall of Karnac, the Parthenon, and that Choir of the Cathedral of Cologne. These stand out purely and nobly, indestructible monuments of Architectural and Æsthetic power. If selection should be asked, there are few in this northern region, and with our northern ideas, who would not first exclude from competition, even the exquisite Parthenon. Between the Hypostyle Hall and the Cathedral, any one's decision might well waver.—There are several doctrines—which should be ever held *cardinal* in Architecture—so closely connected with *Mechanical Science,* that we take leave to embody them here, as

of established maxims. First. Buildings being for use, *use* ought, as their *first*, to be their *final* aim. Secondly. *Ornament* ought to be superinduced on *utility*; never can it transgress this limit, without our getting into false taste. From these two maxims, many others follow. Let the modern architect adjudge by the *first* his rational admiration of some early Cathedral structures, in which there is not a Buttress without its mechanical import, or an Aisle without its interior employment and convenience; and then turn to his efforts to reproduce as a *modern church*, a turretted and pinnacled grotesque, in which the parts no more form a mechanical whole than do the parts of a confectioner's bride's cake; and what is worse, in which the interior—defaced besides by galleries choking up the arches—has not the remotest reference to the requisitions of modern Protestant worship. And let him equally adjudge, by the second maxim—unwarned as he has been by the failure alike of the *flamboyant* and the *perpendicular styles*, in which our pure Gothic had its termination,—in what manner he can explain *grotesques*, so utterly without meaning, now reproduced in sheerest and dullest imitation, but to which no devout—no living human Soul—can now attach one intelligible idea! In our modern vulgar, and lifeless, and absurd reproductions, not one living conception is embalmed. The great Cathedrals, of old times, are mostly incomplete: for instance, they have only *one* instead of *two* superb and symmetrical towers. It has become now a symbol of "correct and antiquarian fashion" to build even small churches with *one*, side and unsymmetrical tower!—Let us return to *reason*, and *thus* imitate those best of ancient times. An Architecture worthy of modern genius, fit for modern uses, helped by all modern appliances, and ornamented in accordance with the noblest of modern aspirations, remains to be created. Would indeed, that the Age could obtain one thoroughly good and free Stone Mason of unfettered genius!

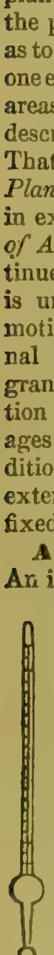
Arctic, (derived from the Greek, Αρκτος, a bear—from the proximity of the North Pole to the constellation — the bear), is used frequently as equivalent to Northern. The phrase *Arctic zone* is accurately applied to a circle having the North Pole for centre, and having a radius equal to 23° 28′ nearly. The *Antarctic zone* is a similar section of the earth's surface, having its centre at the South Pole. The *Arctic regions* is a phrase employed with reference to particular spots both of the earth and sky, and refers in every case to the regions adjacent to the North Pole. The poles are the extremities of an imaginary line round which the earth performs her diurnal revolution (v. Pole). The North Pole of the heavens is obtained by prolonging this line in the direction of the celestial sphere, and is the point where this prolonged direction meets the sphere. Of course it is not so much a point as a line of direction, since the sphere referred to is imaginary.

Arcturus. A well-known star in constellation Bootes.

Areas, *Principle of*; or *Principle of the Conservation of.* A principle in *Rational Mechanics*, of so general an application, that it can scarcely be called a secondary one. The first and simplest form in which this important principle can be presented, is the *Second Law* of the immortal Kepler. Having found that the planet *Mars* describes around the *Sun* an ellipse, in one focus of which the *Sun* is placed, he discerned soon after, that the law of the velocities of *Mars* could be expressed as follows:—The *radius vector* (*i.e.* the line joining the centre of the sun and the centre of the planet) sweeps over *equal areas* in *equal times.* So soon as the great German extended the fact of elliptic motion to *all* the planets, he recognized the law of the velocities to be *general* also; so that it could be stated that all the planets move around the sun with such velocities, that in *each* case the *radius vector* describes *equal areas* in *equal times.* Such the earliest discovery through *facts*, of the great principle of the *conservation of areas.* Let us attempt to describe it now in its full generality. Kepler's law extended to the case of one planet or orb encircling another; but it belongs to the most complex system, whose orbs are controlled by a mutual attraction. And in the following wise. Kepler's law, in reference to the planetary movements, expresses close approximations only; it would not be absolutely correct unless in the case of planetary orbs moving *independent of each other* around a central point. If the planets affect each other, *perturbations* ensue. But the principle of areas, rightly understood, holds good through all such perturbations. Take, for instance, a system of three bodies, such as the *Sun*, *Earth*, and *Moon.* If the orbits of the earth and moon were in the same plane, Kepler's law would be this:—The sums of the two areas described in equal times, viz. of the area described by the radius vector of the sun and earth, and the area described in the same time by the radius vector of the earth and moon, will always be equal. But the orbits are *not in the same plane;* hence a great complicacy, not however affecting the principle of the conservation of areas. Because of the difference of *plane*, resort must be had to the device of *projection; i.e.* the areas in question must be projected on *some one plane.* Suppose one's self looking at these motions through a transparent sheet of glass, placed in any way, the motion of the two bodies would appear on that transparent sheet as if they were in one plane, and the areas described by their *radii vectores*, would, as seen on that sheet, have definite measures; it is *these last, or projected amounts*, which, in accordance with the *principle* of the *conservation of areas*, are proportional to the *times of description.* Apply this scheme or mode of thought, to our entire planetary system; *i.e.*, look at the whole of it

through a transparent sheet, placed in any way, then the areas, as seen described by the *whole*, whatever their relations, will be strictly proportional to the times of description. The great Laplace even, saw the stretch of this principle but imperfectly. He conceived it rigorously true, if the major motions only were taken into account. It is certainly not so. Poinsot has recently insisted that not only the motions of *revolution* of satellites, but motions of *rotation* alike of planet and satellite, must be held in view, ere identity can be predicated between spaces described in equal times, within a system internally balanced. Thus understood, the principle of the conversion of areas must, in every such system, be rigorously true. But without going to such rigour, one important and comparatively practical conclusion may be drawn. Reference has been made to the *projection* of such areas on a *plane*. However that plane is placed, the foregoing theorem is true. But although the proportionality of *areas* to the *times* of description, holds true with regard to *any* plane; the amount in size of the areas projected on the plane will manifestly depend on the *position* of the plane. Now, with regard to the solar system, as to any other system, there is at every given time one especial plane, on which the amount of the total areas described will be greater than their amount described or projected or on any other plane. That plane has been termed the *Great Equator Plane of the System;* and it is the highest result in expression, of the principle of the *Conservation of Areas*, that this *great equator plane* will continue *invariable in position* so long as the system is undisturbed from without, and carries on its motions, through the interdependency of its internal parts. If the position, then, of our own grand equator plane were determined, that position would be a fixed point by which after ages might decide whether and how far our condition has been affected by communion with external systems, or by our sympathies with the fixed stars.

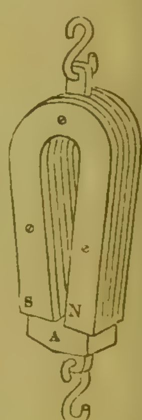
Areometer. (ἀραιός, light; μέτρον, a measure). An instrument serving to measure the densities of liquids. The principle of the areometer is simply this, that any solid body sinks farther in a light liquid than in a heavy one. It is usually a small glass bulb loaded at the bottom with quicksilver, or small shot, so as to set it upright, and having a scale at the top to mark the depth to which it sinks in any liquid. In most cases, the numbers on the scale are arbitrary, and refer to arbitrary indices of the strength or weakness of a fluid (*e. g.* over proof and under proof) employed by merchants in testing the value of their purchases. Where the liquid should be lighter the purer it is, the degrees proceed from the bottom to the top (as in alcohol), and where its value is proportioned to its density, they go downwards from top to bottom. As the density varies with the temperature, a thermometer is frequently placed alongside the graduated scale, and this is sometimes ingeniously constructed so as to serve as a sort of corrective against errors of observation on the scale of density. We give a figure of the instrument.



Aries. An equatorial constellation; the first of the signs of the ecliptic.

Argo. One of the great southern constellations. The star Canopus, of the first magnitude, is its most prominent star.

Armature. A French term partly adopted in English. There is no English one accurately corresponding. It is the name of the piece of iron attached usually to a magnet to prevent the dissipation of the magnetic fluid. The ordinary horse-shoe magnet is of the form represented in the figure, the poles N and S being opposite. The piece of iron, A, becoming by contact a magnet also, completes a magnetic circuit, each part of which acts and reacts on the other, as the coatings of the Leyden phial in the case of electricity do. For an ordinary bar magnet, it is usual to fix its magnetization by placing alongside of it another of the same form, but with its poles in directions opposed to those of the other, and to place two pieces of soft iron, one at each end, in contact.



Armillary Sphere. An instrument consisting of several brass rings, all circles of the same sphere, in the position which important circles of the celestial sphere have. Suppose that we have a celestial globe, along which are marked the following circles in their proper positions:—the *horizon*, the *meridian*, the *equator*, the *ecliptic* or circle of the *zodiac*, and the two *colures (q. v.)* (these all being *great* circles of the sphere, dividing it, that is, in two), and the two *tropics*, and the two circles which limit the arctic and antarctic zones (which four are *small* circles of the sphere), and let us suppose that rigid circular rims are made just to go round those lines, and the sphere then destroyed while these rims are connected with one another to remain steadfast in their relative positions, and mounted on a stand as the sphere was, we should obtain what is called the *armillary* sphere. The reader will readily understand how, supposing the centre of the sphere to be the position of a spectator at the earth, he will come to comprehend the *apparent* motions of the heavenly bodies and the significance of the references constantly made in describing the apparent places, to these elementary circles.

Artillery, a general name given to all arms used in war, which are too heavy to be carried by the individual combatants. These arms of course vary in characteristics according to their purposes. Some are employed for the attack or

defence of fortified places, and others in the open field or during the course of an open field fight. The purposes of defence, in case of a besieged fortress, necessarily admit the use of heavy fixed pieces of ordnance; and it was in such circumstances alone that Perkin's steam-gun, so notorious several years ago, was ever deemed to be applicable. Probably one of the finest developments of ordnance during a siege, that has yet taken place, occurred in course of the recent siege and defence of Sebastopol. The artillery officer and engineer cannot fail to derive large instruction from the detailed account of this celebrated event, now on the eve of publication under the auspices of the French Minister of War. Artillery ordnance varies also according as its destination is for use on land, or on board ship.—A formal article on Artillery would be altogether out of place in a Cyclopædia like this; we shall dismiss the subject therefore with a brief notice of its connection with the progress of the mechanical arts, and its dependence on the condition of these arts. It were of course needless to advert to the enormous or rather the fundamental change that passed over every requisition of Artillery on the invention of gunpowder. The force of the projectile was given to it previously, by aid of the elasticity of bent cords or of a solid spring: on that invention, a small portion of powder created an explosive and propellent force, of the power of some thousand atmospheres. Since then there has been but one limit to the efficacy of that propulsion, viz., our mechanical ability to bestow on the tubes holding the powder and projectile, as much strength as would resist the lateral action of the explosive force, and at the same time consist with our power to transport the piece of ordnance easily. Resistance to this lateral force, and lightness,—these are the prime requisites of a modern cannon. Especially is it so in parks of artillery meant for use in the open field, whether these be the lighter sort of *flying artillery*, or pieces and batteries intended for comparatively stationary action. Of course *trueness* of direction is demanded of cannon as well as of the hand rifle: unfortunately this has not as yet been manifested in action to anything like the degree of perfection attained by the smaller rifle. This however is imperatively demanded by the growing perfection of that small rifle. Field artillery in one of its important applications is meant to open the way to the action of infantry: it ought therefore to be effective at a distance which will insure safety to the gunner, from antagonistic riflemen.—We are now at the opening of a great European war. It is said that France has greatly increased its artillery arm, and that the Emperor has selected a new and favourite gun. The arm with which Sir William Armstrong has strengthened alike the field and sea artillery of Britain, is understood to be an unusual acquisition. It carries home through five miles, and is so light that two or three boys may manage it.—Especial reference is made to article RIFLE in our Dictionary; and also to the more extended article by the same author in the *Encyclopædia Britannica*, new edition.

Ascension, Right, Oblique. Ascension is an astronomical term used for the purpose of referring a star to its exact position in the heavens, and so rendering possible the identification of the star. The north and south poles of the heavens are regarded as fixed points, and the equator is consequently a fixed circle of the celestial sphere. Suppose, now, that a meridian passes through a given star, that is, a great circle through the star and the poles, it will cut the equator at a given point. The distance of this point, in degrees of the circumference from one of the points where the equator is cut by the ecliptic (the vernal equinox), is called the *right ascension* of the star. The right ascension is sometimes given in time instead of in hours. The whole heavens appear, in consequence of the diurnal motion, to revolve once in 24 hours. The space between the star and the point of the visible heavens, where the equinox is at any moment, will be passed over at the rate of $\frac{24}{360}$ of an hour for each degree of angular measurement. To obtain the ascension given in degrees to that in hours, we reckon 4 minutes for a degree, with corresponding fractions for the lesser angles. We simply reverse the process in finding the ascension in degrees from that in hours. The phrase ascension is employed for this reason. The astronomical day is reckoned from the moment at which the first point of Aries (the point of the equator corresponding to the vernal equinox) passes the meridian. All the stars which have the same right ascension will seem to rise upwards from the horizon (if visible, and if not, may be imagined to do so), and to cross the meridian of the place at the same moment. The right ascension, then, as we have described it, measures the interval between the crossing of the meridian by the first point of Aries and by the star. A reference to figure No. 1 will at once

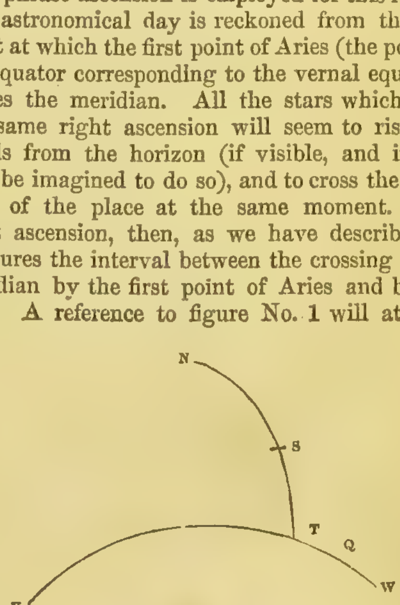


Fig. 1.

show that Q T would be the right ascension of the star, S, upon the meridian, N S T. The other name *oblique ascension* is used to express the difference in the times of the rising of the first point of Aries and a given star. If we consider that while some stars upon the same meridian, and having, therefore, the same right ascension,

almost never set, while others almost never rise, it will be evident that there cannot be the same difference between the times in which the first point of Aries, and all the points having the same right ascension, cross the horizon, and as there is the same difference between the times in which they cross the meridian, the oblique and right ascensions must be represented by different numbers. The distance between that point of the equator which rises with the star, and the first point of Aries, is called the oblique ascension. To a person at the equator, however, whose horizon is a great circle passing through the poles, the oblique and right ascensions are identical. We call *ascensional difference* the excess of the right ascension over the oblique, or *vice versa*. The figure (No. 2) will serve

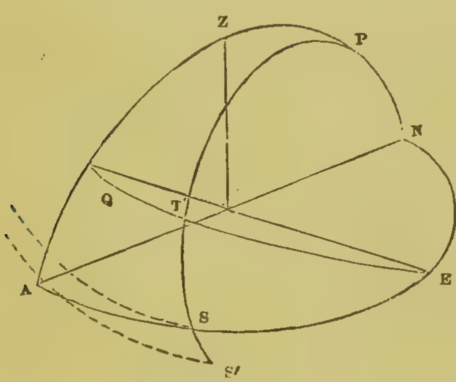


Fig. 2.

to explain this, though the reader's safest way is to consider carefully for himself the various circles described, and all the circumstances mentioned. Without this, plane figures rather mislead than instruct. The star, S, will cut any circle which passes through P, and the other pole (as the meridian does), at the same moment that T does so, and Q T, the right ascension, therefore, measures the interval between the passing of Q, and all stars upon the meridian, P T S. But a star at T, for instance, will have risen above the horizon, N E S, long before S, and one at S', for example, after it. The oblique ascension, therefore, is measured by the space between Q and E, the point of the equator, which rises along with S, and as Q T is the right ascension, T E is the ascensional difference. The oblique ascension and ascentional difference are transformed from degrees to time just as before.

Aspect. An astronomical term, almost disused. It is of interest only because it may be met with in old astronomical works of considerable value. The terms conjunction and opposition express the only two aspects of the planets (*i.e.* relative positions as seen from the earth), which are still named. At conjunction (*q. v.*) two planets have the same longitude; at opposition they differ by 180° in longitude; and in these 180° are the *trine* aspect, 120° apart; the *quartile*, 90° apart; the *sextile* 60° apart. The old characters used to indicate them are the following:—

Conjunction	☌
Sextile	✶
Quartile	□
Trine	△
Opposition	☍

These terms, obsolete in astronomy, are still found in abundance in astrological works—not all ancient yet.

Asteroids, or small planets, a group of bodies singularly insignificant in size, that revolve around the sun in planetary orbits, between Mars and Jupiter. Their relation to the solar system in general, being explained under SOLAR SYSTEM, we shall confine ourselves here to the statement of all that appears peculiar to the group, and characteristic of its position. The existence of a body or bodies, between Mars and Jupiter, seemed to be indicated by a remarkable *hiatus* between these planets (see BODE'S LAW), — the celebrated Olbers, of Bremen, ventured, about the beginning of the century, to assert that the application of telescopes to the search for planets occupying that place, would certainly be richly rewarded. In consequence of this suggestion, *Ceres* was discovered by Piazzi of Palermo, on the first day of this century; Olbers himself soon after detected *Pallas* and *Juno*, and Harding of Goettingen discovered *Juno*. This group of the four small planets, as it was termed, appearing to have verified the prediction and filled up the *hiatus*, curiosity and research were alike suspended, until stirred anew, in 1845, by the accidental discovery of *Astræa*, a fifth member of the group, by Hencke of Driessen. Many observers, in possession of suitable instruments, now rushed into the field; but none with so much perseverance and discernment than Mr. Hind, of the Observatory of Regent's Park, London. Up to the close of 1853, *eleven* new planets had been added to the group by this indefatigable observer, and while we write he continues his earnest and successful research. No fewer than *fifty-six* asteroids are already detected; but the probability is, that they count by *hundreds*, and that they form a stream or zone of small bodies occupying the place, and in so far performing the function, in the system, of the large planet which, according to Bode's law of the distances, might have been expected in the locality. They are essentially a *group*, to be viewed not singly but as a group. As the following table will show, they are nearly at the same distance from the sun; the periods of their revolutions very closely correspond; and in physical characteristics, for instance in *size* (some of them are not larger in surface than the kingdom of France); they are distinguished in the same way from all the chief planets.

ASTEROIDS.—TABLE I.

Name of Asteroid.	Mean distance from the Sun, that of the Earth being 1.	Time of a sidereal revolution. Days.	Eccentricity of Orbit.	Inclination of Orbit.		
Ariadne,	2·1990	1191·11	0·1575	3°	28′	0″
Flora,	2·20139	1193·01	0·156704	5	53	8
Harmonia,	2·26584	1245·78	0 04608	4	15	48
Melpomene,	2 29554	1270·35	0·217051	10	9	4
Victoria,	2·33468	1302·98	0·218255	8	23	6
Euterpe,	2·34642	1312·82	0·173861	1	35	33
Vesta,	2·36056	1324·71	0·090181	7	8	17
Urania,	2·36559	1328·95	0·126397	2	5	57
Nemausa,	2·3779	1339·34	0·0628	10	15	0
Metis,	2·38541	1345·68	0·123889	5	35	56
Iris,	2·38673	1346·8	0·230755	5	27	56
Daphne,	2·4003	1358·34	0·2025	15	48	0
Phocœa,	2·40067	1358·62	0·253126	21	36	5
Massalia,	2·40906	1365·74	0·14368	0	41	10
Hebe,	2·42519	1370·48	0·201802	14	46	20
Isis,	2·4339	1386·91	0·2229	8	35	0
Lutetia,	2·43523	1388·06	0·161634	3	5	22
Fortuna,	2 44304	1394·75	0·158426	1	32	28
Parthenope,	2·45185	1402·29	0·099508	4	37	1
Hæstia,	2·4569	1406·62	0·1226	2	18	0
Thetis,	2·4733	1420·73	0·127802	5	35	38
Amphitrite,	2·55447	1491·25	0·0725	6	8	0
Astræa,	2·57651	1510·58	0·189992	5	19	35
Egeria,	2·57686	1510 92	0·087084	16	32	14
Pomona,	2·58298	1516·28	0·095624	4	42	18
Irene,	2·58526	1518·29	0·168713	0	6	44
Calypso,	2·6129	1542·7	0·1803	5	4	0
Thalia,	2·62588	1554 21	0·235395	10	13	59
Fides,	2·64215	1568·7	0·174893	3	7	11
Eunomia,	2 64369	1570·05	0·187899	11	44	0
Virginia,	2·651	1576·56	0·2872	2	48	0
Proserpine,	2·65604	1581·07	0·087522	3	35	39
Juno,	2·66861	1592·3	0·256535	13	3	27
Nysa,	2 6769	1599 7	0·4534	3	53	0
Circe,	2·6883	1609·98	0·108252	5	26	33
Eugenia,	2·6968	1617·64	0·0914	6	35	0
Leda,	2·73986	1656·34	0·15557	6	58	32
Atalanta,	2·74989	1665·63	0 298171	18	42	9
Ceres,	2·76577	1680·05	0·079180	10	36	27
Pallas,	2·76953	1683·48	0·239045	34	42	37
Lætitia,	2·77105	1684·87	0·110813	10	21	0
Bellona,	2·77518	1688·65	0·154507	9	22	31
Polyhymnia,	2 86550	1771·74	0·336806	1	56	56
Aglaia,	2 8894	1793·99	0·1402	5	6	0
Calliope,	2·90950	1812 72	0·10196	13	45	28
Psyche,	2·92287	1825 21	0 134634	3	4	9
Leucothea,	2·97366	1873·02	0·21650	8	15	18
Pales,	3·0861	1980·23	0·2377	3	8	0
Doris,	3 1068	2000 22	0·0771	6	30	0
Europa,	3·1352	2027 65	0·1433	7	12	0
Hygeia,	3·14937	2041·43	0·104558	3	47	9
Themis,	3 15130	2043·34	0 117 03	0	49	3
Euphrosyne,	3·15616	2048·03	0·216012	26	25	12

The Elements of *Pandora*, *Alexandra*, and the new planet, not yet determined.

ASTEROIDS.—TABLE II.

Names.	Longitude of Perihelion.			Longitude of Ascending Node.			Discoverers.	Places of Discovery.
Ariadne,	277°	12′	0″	264°	45′	0″	Pogson,	Oxford.
Flora,	32	54	28	110	17	49	Hind,	London.
Harmonia,	2	2	16	93	32	28	Goldschmidt,	Paris.
Melpomene,	15	14	7	150	0	53	Hind,	London.
Victoria,	301	52	59	235	29	28	Hind,	London.
Euterpe,	87	48	30	93	42	1	Hind,	London.
Vesta,	250	46	10	103	23	39	Olbert,	Bremen.
Urania,	30	49	41	308	12	0	Hind,*	London.
Nemausa,	190	13	0	175	38	0	Laurent,	Nismes.
Metis,	71	29	21	68	31	24	Graham,	Markree.
Iris,	41	23	12	259	44	39	Hind,	London.
Daphne,	230	22	0	180	6	0	Goldschmidt,	Paris.
Phocœa,	302	37	22	214	3	40	Chacornac,	Marseilles.
Massalia,	98	16	30	206	36	24	De Gasparis,	Naples.
Hebe,	14	57	47	138	34	25	Hencke,	Driesen.
Isis,	317	58	0	84	28	0	Pogson,	Oxford.
Lutetia,	326	43	52	80	28	28	Goldschmidt,	Paris.
Fortuna,	30	47	53	211	26	33	Hind,	London.
Parthenope,	316	6	5	125	2	51	De Gasparis,	Naples.
Hæstia,	344	57	0	181	32	0	Pogson,	Oxford.
Thetis,	259	24	49	125	26	12	Luther,	Bilk.
Amphitrite,	56	13	42	356	22	51	Martin,	London.
Astræa,	134	35	36	141	24	49	Hencke,	Driesen.
Egeria,	119	45	7	43	17	34	De Gasparis,	Naples.
Pomona,	196	8	40	220	48	26	Goldschmidt,	Paris.
Irene,	178	51	11	86	49	1	Hind,	London.
Calypso,	94	39	0	143	30	0	Luther,	Bilk.
Thalia,	123	11	57	67	55	2	Hind,	London.
Fides,	66	4	28	8	9	37	Luther,	Bilk.
Eunomia,	27	51	46	293	54	51	De Gasparis,	Naples.
Virginia,	10	29	0	173	30	0	Fergusson,	Washington.
Proserpine,	235	16	30	45	53	20	Luther,	Bilk.
Juno,	54	8	17	170	59	32	Harding,	Lilienthal.
Nysa,	118	49	0	127	6	0	Goldschmidt,	Paris.
Circe,	149	58	35	184	47	11	Chacornac,	Paris.
Eugenia,	208	17	0	148	20	0	Goldschmidt,	Paris.
Leda,	100	40	28	296	27	47	Chacornac,	Paris.
Atalanta,	42	23	48	359	9	29	Goldschmidt,	Paris.
Ceres,	149	33	41	80	48	22	Piazzi,	Palermo.
Pallas,	122	3	50	172	38	20	Olbert,	Bremen.
Lætitia,	2	7	12	157	19	39	Chacornac,	Paris.
Bell na,	122	24	48	144	43	5	Luther,	Bilk.
Polyhymnia,	340	53	55	9	16	5	Chacornac,	Paris.
Aglaia,	306	1	0	4	25	0	Luther,	Bilk.
Calliope,	56	34	13	66	36	22	Hind,	London.
Psyche,	12	38	59	150	31	20	De Gasparis,	Naples.
Lencothea,	198	17	0	356	24	38	Luther,	Bilk.
Pales,	32	49	0	290	27	0	Goldschmidt,	Paris.
Doris,	77	12	0	185	14	0	Goldschmidt,	Paris.
Europa,	98	27	0	129	23	0	Goldschmidt,	Paris.
Hygeia,	227	47	59	287	38	34	De Gasparis,	Naples.
Themis,	138	2	48	36	12	39	De Gasparis,	Naples.
Euphrosyne,	93	51	7	31	25	23	Fergusson,	Washington.

Pandora discovered by Searle, of Albany.
Alexandra and the new planet by Goldschmidt.

Perhaps the first thing that strikes one, on glancing over this long and confused list of names, is the absolute need of a new and simpler mode of distinguishing the several Asteroids. This has been partially adopted, and must come into universal use. It is proposed that each Asteroid be indicated by a number marking its rank in the order of discovery, that number being enclosed within a small circle, thus:—

Ceres, . . (1).
Urania, . . (30).
&c.

This is evidently the only principle of numbering that could be permanent: any arrangement reposing on the distances of the Asteroids from the sun being ever liable to be broken up on the discovery of some new member of the group: the permanency even of this assignment, indeed, was recently threatened by a singular occurrence. On the 9th of September, 1857, Goldschmidt thought he had re-discerned Daphne, and published his observation under that idea. In turns out, however, that the body he saw was not Daphne, but must have been a new planet: (the "new planet" referred to in these tables). By the foregoing rule, therefore, its sign ought to be (47), and all signs subsequent to that would require to be changed. But such a difficulty is not likely to recur, and it may be got over in the present instance by numbering the planet when it shall be again seen, and shall receive some fantastic name. The following list gives the order of discovery, and of course the new conventional signs:—Ceres, Pallas, Juno, Vesta, Astræa, Hebe, Iris, Flora, Metis, Hygeia, Parthenope, Victoria, Egeria, Irene, Eunomia, Psyche, Thetis, Melpomene, Fortuna, Massalia, Lutetia, Calliope, Thalia, Themis, Phocea, Proserpine, Euterpe, Bellona, Amphitrite, Urania, Euphrosyne, Pomona, Polyhymnia, Circe, Leucothea, Atalanta, Fides, Leda, Lætetia, Harmonia, Daphne, Isis, Ariadne, Nysa, Eugenia, Hæstia, Aglaia, Doris, Pales, Virginia, Nemansa, Europa, Calypso, Pandora, and Alexandra.

Passing, however, from external details to consideration of the interesting character of this system, we find on comparing the orbit of Euphrosyne with that of Ariadne, that the *mean* breadth of the zone, or ring within which the Asteroids lie, is ·95716 of the mean distance of the Earth from the Sun, or about ninety-one millions of miles. But in consequence of the great eccentricity of several of the orbits, some of these curious bodies can adventure much farther than that into space. The planet Nysa, for instance, has an eccentricity so great that its orbit much more resembles that of a comet. It recedes farther from the sun than any of the others, and, with the exception of Hæstia, it approaches him the nearest. Of them all Hæstia comes nearest the earth; after it, in this respect, Nysa ranks. Euphrosyne and others have also very eccentric orbits. The inclination of the orbit of Nysa, too—viz.: 3° 53′—is not very distant from that of the orbits of Ariadne, Harmonia, Lætetia, Fides, Proserpine, Psyche, Pales, and Hygeia, so that it may, or rather must, frequently approach closely to some of these bodies. The inclinations, indeed, of Nysa and Hygeia differ by only six minutes, so that they are almost in the same plane. The longitudes of their ascending nodes, however, differ at present by 160°. It is scarcely necessary to call attention to the contrast presented by the inclination of the orbits of several of these Asteroids to anything that prevails in the other portions of our solar system. The greatest inclination known among the larger planets is that of Mercury, being 7° 0′ 5·9″; the inclination of that of Pallas is, on the other hand, 34° 42′ 37″; that of Euphrosyne, 26° 25′ 12″; of Phocœa, 21° 36′ 5″; of Atalanta, 18° 42′ 9″; of Egeria, 16° 32′ 14″; &c., &c. But their deviations do not involve any deviation as to motion from what the law of gravitation would induce us to expect. Although the ellipses are eccentric, and the inclination large, every one of these minute orbs obeys the three cardinal laws of Kepler, and so constitutes no exception to the grand harmonies of the solar system.

In reference to another feature of the position of these orbits, a speculation worthy of notice was recently started by Edward Cooper, Esq., of Markree Castle—a speculation further justified since the addition of many new Asteroids to the list. Mr. Cooper first called attention to the singular fact, in his work on Comets, and he took into his account the larger planets also. With reference to the Asteroids alone, it is this:—Of the fifty-three whose elements are given above, the perihelia of no fewer than forty lie within the semicircle from 0° to 180°, or on its verges; and within the same space are found the longitudes of the ascending nodes of forty-three. The quadrant from 0° to 90°, contains the perihelia of twenty-seven. There can be no doubt that there is a singular speciality here: the laws of probability are utterly against the casuality or indifference of such a distribution. Mr. Cooper is far from inclined to push conjecture too far: he modestly concludes thus:—"After all it may be accidental, as consistently with the laws of planetary motion, such a congregation of perihelia or nodes may occur at periods exceedingly remote."

Notwithstanding the uncertainty in which we are, as to the possible number of these planetoids, some definite light reaches us, from physical astronomy, on the question as to their total *mass*. In the case of a single planet of the ordinary size, between Mars and Jupiter, not only would its perturbing influence on the nearer orbs be felt, but that influence could be exactly calculated, and traced in all its relations, in the irregularities impressed by it, on their orbits and motions. These small masses must exercise a similar influence; although, from their diffusion through the

sky—the one, on this account, often compensating the action of the other—it is much more difficult, and might even appear impracticable, to deal with the problem. It is, however, only what are termed the *periodical irregularities* (see PERTURBATIONS) that would be compensated in this way. The *secular* variations, on the contrary, do not depend on the relative positions of the disturbed and disturbing bodies. There is, for instance, a secular motion of the perihelion belonging to this latter class. The perihelion alike of Mars and the Earth must be subjected to a perturbation depending on the total mass of these planetoids, their mean distances from the Sun, and the eccentricity of the orbit of the disturbed planet. Owing to the larger eccentricity of Mars, as well as the position of its perihelion, in reference to the mean direction of the perihelia of the discovered Asteroids (which lie almost all in the same semi-circumference of the heavens), that planet is much better fitted than the Earth to be the instrument of such an investigation. Looking at the subject in this point of view, M. Leverrier has lately shown that if the total mass of these planets reached equality with the mass of the Earth, the perihelion of Mars would have been disturbed by them to the extent of *eleven seconds* in a *century.* Now, although the orbit of Mars, and its history during the recent century, cannot be said to be determined with an ultimate rigour, it is safe to conclude that there is not an undiscovered error of this sort, to *a fourth* of the previous amount. From which it would appear *that the sum total of the matter constituting the small planets lying between the mean distances of* 2·2 *and* 3·19 *from the Sun, cannot greatly, if at all, overpass one-fourth of the mass of the Earth.* This restricted mass, however, is very considerable, being upwards of twenty times greater than the mass of our Moon; and it doubtless includes within it, multitudes of Asteroids not yet discerned by the telescope.

The contemplation of this very curious group gives rise to singular reflections. How odd the motions of masses of small orbs, with paths so near, that often they must pass within sight, in the celestial spaces—like ships within hail at sea! But whence came they? What means this extraordinary exception to that general law which has constituted the solar system for the most part an orderly arrangement of large orbs moving through spheres far apart from each other, and thus in all things independent? Reflecting on the fact that they occupy the precise place which, in conformity with Bode's Law of the distances, ought to have been filled by a large planet, Olbers threw out the conjecture that these Asteroids may be the fragments of a planet once existing there, but which, in some mighty convulsion, had burst asunder. Nor, perhaps, if one considers the inherence in all planets known to us, of the same disrupting powers which originate volcanoes and mountain masses in the earth, can the conjecture be rejected *à priori* as entirely fanciful. But a fatal dynamical objection remains. If the group are fragments arising from the bursting of one body, they would all, in the course of these revolutions, necessarily tend to return to the period when the primary explosion took place. Laplace, on the other hand, regarded them as indications that a primary or large planet had never been formed there, but that the ring of primary nebulous matter, out of which he imagined every planet to have sprung, had rather resolved itself, in this place, into a multitude of small knots or aggregations.—It cannot escape observation, however, that this group of Asteroids, strange though it may seem, when we compare its individuals with planets like Jupiter, or even the Earth, may not, after all, occupy an isolated place, even within our own system. Those showers of meteors that sometimes illumine the heavens, as well as those sparse but startling shooting stars, are probably masses of bodies not unlike the Asteroids, only a step lower down in the progress from large globes to mere dust. And then the *zodiacal light*, is not that still nearer to dust—akin, it may be, to the cometic matter—a thing *rare* in space? It may, indeed, turn out that our leading planets of the solar system, are only the more visible parts; and that when we know our scheme better, its simplicity will no longer be recognized.

Astræa. One of the small planets between Mars and Jupiter. See ASTEROIDS.

Astrolabe. The name given by the ancients to the instrument by which they measured the angular distances of celestial bodies. It had various forms; but the chief part of it was a graduated circle, around whose centre, as its point, an arm moved carrying *sights.* Rude though the astrolabe was, we must consider it the germ out of which our present Astronomical Circle sprung. See CIRCLE.

Astrology. The *superstition* of the ancient astronomies. The notion on which it rested was environed indeed with something of plausibility, in times when our small world was esteemed the main or central orb of the universe, and the stars gemming the Infinite only lights created for our use. That notion, in its highest form, may best be expressed as follows:—Besides performing their functions as lights, these stars are instruments by which the Creator governs terrestrial events, and their potency may be discerned through their *combinations* and *position* in the sky. It is needless to state, that the postulate, being wrong, every deduction must be erroneous also. The intelligent require no refutation of the postulate; nevertheless, there is an impressive lesson in the fact, that astrological almanacs are still favourites even with the masses of the people of England. Under our modern civilization, there are very *masses*, over whose heads the discoveries and acquirements of science

clean pass. These multitudes live within a sphere of their own, wherein prevail peculiar philosophies as well as peculiar religions. The extraordinary evolution of *Mormonism*, having made patent this astounding fact,—whether is it wise to fulminate against the intellectual and social heresies of Mormonism, or, to look deeper, at that ignorance and debasement—accompanied, nevertheless, with the capacities and aspirations of a higher humanity—which are its source?

Astronomical Signs. See SIGNS, ASTRONOMICAL.

Astronomy. The science whose object it is to discern facts and laws concerning the distribution, motions, and nature of the Heavenly Bodies. Its origin is in far antiquity: the shepherd Chaldeans connected, very early, the presence of certain constellations in the midnight sky, with the return of the seasons. We shall shortly distinguish here, the chief steps of its progress. Of these, the first, was that important one which led to the separation of the planets—as wandering orbs—from the fixed stars. This was accomplished in remote times; and in Greece theories arose, with a view to take account of the motions of the planets. These theories were perfected by the Alexandrian astronomers. Resting on the notion that the apparent motions of the planets are *real* motions, or, what is the same thing, that the earth is at rest, and in the centre of all the motions of the universe, the Greeks and Alexandrians reared a complex but most ingenious system, whose authority lasted until the times of Copernicus. Neither in these early days were the apparent motions of the system of the fixed stars overlooked: it is one claim of Hipparchus to immortal honour, that he discovered that great apparent motion of the celestial vault, which we designate as the *precession of the equinoxes*. (Compare PRECESSION). As we descend in astronomical history, not only does the distinction between planets and fixed stars become broader, but inroads are being made into each sphere, more and more successful as years pass on. It will suffice, in this general article, that we refer to *heads* under which, in this Dictionary, minute information, occupying as great space as we can devote to it, will be found.

(1.) *The Solar System.*—The discoveries of Copernicus revealed the cardinal fact, that the earth is merely one planet, rolling with others, and the sun. Several questions immediately arose, viz. *first*, What is the character of the orbits through which these planets move? A question answered by the observations of Tycho Brahe, and the Laws of Kepler. (Compare KEPLER'S LAWS). *Next* came the inquiry, according to what mechanical or dynamical laws do these bodies move? what new *forces* do their motions reveal? A question answered by our immortal countryman, Sir Isaac Newton, and his great successors. (Compare ATTRACTION, FORCE, GRAVITATION, LUNAR THEORY). The *physical characteristics* of the several constituents of the solar system next attract attention. All that has been ascertained on this subject is exposed under SOLAR SYSTEM, and under the special heads of the names of the different planets. To which must be added ASTEROIDS, COMETS, METEORS.

(2.) *The Fixed Stars.*—That great and unfathomable company of orbs which we name the *fixed stars*—orbs of the class of our sun, and round each of which systems of planets probably revolve—interest us in various ways. First, as to their *distance* and *distribution*, see CONSTELLATION, PARALLAX, NEBULÆ. Secondly, as to their physical aspects, compare TWINKLING, FRAUENHOFER'S LINES. Thirdly, as to their *apparent motion*, see PRECESSION, NUTATION, and TRANSLATION OF THE SUN. Fourthly, as to their *real motion*, see STARS MULTIPLE, and STARS, PROPER MOTIONS OF.

(3.) The third important division of astronomy may be termed the *Instrumental*. It refers to the manner by which discovery of facts is now successfully conducted. Information on this head will be found principally under CIRCLE, EQUATORIAL, TELESCOPE, TRANSIT INSTRUMENT. Minor points are referred to under ALTITUDE and AZIMUTH INSTRUMENT, COLLIMATION, ERRORS, &c.

(4.) *Practical Astronomy*, in its narrow sense, will be discussed, in so far as we can discuss it in this Dictionary, under CHRONOMETER, LATITUDE, LONGITUDE, NAVIGATION, SEXTANT, TIME, &c. &c.

The best modern work on astronomy is that by Delambre—the *Histoire* and the *Traité*, in nine quarto volumes. Also, Sir John Herschel's *Outlines*.

Asymptote. A straight line which a curve continually approaches without ever being able to meet it, is called an asymptote. The possibility of this, not seen often at first, will be readily demonstrated in this way. Suppose the curve to be at first distant by $\frac{1}{2}$ foot from the line, and suppose that in the next foot it goes $\frac{1}{3}$ of a foot nearer it, in the next, $\frac{1}{9}$ of a foot, in the next, $\frac{1}{27}$, and so on. If putting all these sums together they will never, however far we carry them, come to make up one-half foot, it is manifest that the curve will never meet the line. The sum of the geometrical series $\frac{1}{3} + \frac{1}{9} + \frac{1}{27}$, &c., when carried on to infinity, is equal to $\frac{1}{2}$, but only then. If we take any finite number we stop short of this, and the curve has not at that point been yet cut by the line. As this holds with any finite number of terms, the curve never reaches the line, but constantly approaches. Each advance is accompanied by an approach, but each approach becomes smaller and smaller. Two curves which have both the same asymptote, and which do not cut one another, are called also asymptotes, by an

extension of the term. It is only in practice applied to a straight line. There are curves like the circle which evidently cannot have symptotes. There are others, like the parabola, infinitely extended, which have not. The hyperbola is the commonest instance of a curve with an asymptote. The term is derived from the Greek, and signifies *not falling together*.

Atmometer, (ἀτμὸς, vapour; μέτρον, a measure). An instrument intended to measure the quantity of water evaporated in given circumstances. The simplest atmometer is obtained by exposing a vessel filled with water to the atmosphere, in circumstances as little liable to disturbance during the process as possible. The water is weighed before and after evaporation, and the difference of weights shows us the quantity of water evaporated. A multitude of circumstances, however, such as the dryness of the soil, its nature, the influence of plants upon it, &c., cause the evaporation to proceed more or less rapidly at the surface of the earth, and no atmometer has yet been constructed which serves very well, even at heights above the surface. Belloni, Leslie, Anderson, and others, have described various instruments, in none of which, however, can we place great confidence.

Atmosphere:—the gaseous envelope of any celestial Orb. The term is restricted here, however, to the envelope of the Earth. The discussion of the structure, extent, and habits of our Atmosphere would spread out over almost the whole domain of meteorology: we shall occupy the present essay with considerations of the main portion only, of this important division of physical research,—referring for details to specific articles.

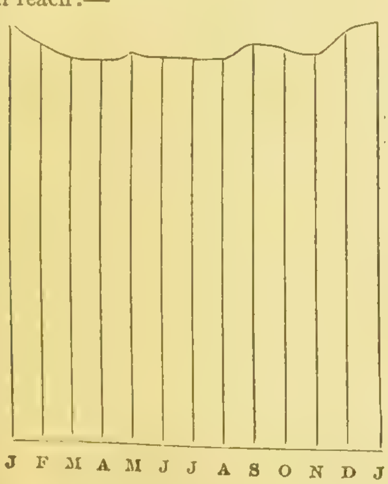
(1.) *Atmosphere, Constitution of.*—Our atmosphere is the recipient of all the vapours and gases exhaled and evolved in the laboratory of the earth; but it may be separated into two portions, closely intermingled, although widely distinct in their character and action, viz.:—I. The smallest, but not the least important section of our compound atmosphere, is its VAPOROUS portion,—consisting mainly of an envelope of AQUEOUS VAPOUR, interspersed through the other section of it. This important constituent varies in weight from ·0033 to ·0166 of the entire weight of the atmosphere; but, although it is thus comparatively trifling in quantity, it is an essential element of our aerial envelope, giving rise to many of its leading phenomena and changes, as will be seen below as well as under such articles as RAIN, CLOUD, MIST, HYGROMETRY, &c.—II. The PERMANENTLY ELASTIC atmosphere; or that large portion of it whose elements are not *condensible* under ordinary variations of temperature and pressure. The three leading permanently elastic gases found in this portion of the atmosphere are *Oxygen*, *Nitrogen*, and *Carbonic Acid Gas;* although there are everywhere distinct traces of *Nitric Acid Gas*, *Ammonia*, *Iodine*, and pure *Hydrogen*. The two elastic fluids first mentioned make up the mass of the atmosphere, being found in the proportion of 20·9 volumes of *Oxygen* to 79·1 of *Nitrogen* or *Azot;* while *Carbonic Acid Gas*, the element next in importance, is present only in the low and variable proportion of from ·01 to ·005. The proportions of these permanently elastic elements, do not in general perceptibly vary with localities; nevertheless, the recent delicate eudiometrical researches of Regnault warn us that this proposition must not be taken as of the last exactitude. The quantity of the important element *oxygen* seems to alter from 20 to 21 *per cent.*—variations referrible to *locality* partly, but also to *circumstance*. For instance, on the invasion of cholera on the Ganges, near Calcutta, in March, 1849, the proportion of oxygen had descended to 20·39. It is horrible even to look at analysis of the air in close and filthy places in our great cities, in the immediate proximity of many crowded dwelling-houses. The following is such an analysis from a confined wynd in Paris. No doubt multitudes of similar ones might be produced from corresponding districts in this country:—

Oxygen,	13·79
Azot,	81·24
Carb. Acid,	2·01
Sulph. Hyd.,	2·99

To breathe air so mephitic is to expose one's self to certain death.—We shall proceed to explain, in the order best suited to clearness, the habitudes of this our *Composite* Atmosphere.

(2.) *Atmosphere, the Composite;—Weight, and general Representation of; Variations in the Weight of, according to Locality, Season, and Hour of the Day.*—The determination of the prime physical fact regarding our atmosphere—viz., its weight—we owe to an invaluable instrument, the BAROMETER. The action and management of this instrument are explained under BAROMETER; suffice it here, that the *corrected* height of the mercurial column represents the height of an envelope of mercury, at the temperature of 32°, which would equal in *weight*, the entire aerial envelope of the earth. In so far as this element goes, our actual atmosphere might be supplanted by a liquid mercurial ocean of the average depth of 29·97 inches: and it will sometimes assist our readers' ability to conceive the changes befalling the atmosphere as it is, if he accustom himself to realize this substitution.—It appears, from observation, that the height of this mercurial atmosphere is not the same in all latitudes, nor in any locality at all seasons, or at all hours of the day. Some of the facts touching on this subject are explicable by theory, others not so as yet.—I. Reckoning from the same level—viz., the sea shore,—the heights of our supposititious atmosphere are found to vary with the *latitude*, according to an empirical law, of whose physical significance we cannot at present give sure account. At the equator and its neighbour-

hood the average height is 29·84 inches. At latitude 10° the pressure or height begins to increase visibly; augmenting on towards latitude 30° or 40°, where it seems to reach its maximum, being there 30·08 inches. Beyond this zone the mean height of the mercurial envelope diminishes, descending in the arctic regions to 29·76 inches. According to some observations, it would appear that the pressure once more increases, on nearer approach to the pole. The details on which these conclusions rest have been collected and presented in an instructive table by MM. Schow and Poggendorf. As to the theory of such variations, nothing satisfactory can at present be hazarded. The latitude of maximum pressure certainly corresponds nearly with the polar limit of the TRADE WINDS (*q. v.*) This subject is further complicated by a suspicion, if not an established fact, that the mean pressure of the atmosphere varies with the longitude also. The first and great *quæsitum*, evidently, is the construction of a full system of what are called *Isobaric lines*, or lines of equal pressure, over all the surface of the globe. —II. The weight or pressure of the entire atmosphere, varies also over every locality with the season of the year. The amount of the variation changes with the locality, and, in so far as observation has yet gone, lies between ·173 of an inch of the height of mercury, and ·074. There is a distinct connection everywhere with the seasons, although this is at first sight somewhat complex. On an average, the pressure is less in summer than in winter. But a *double period* is observable. Beginning with its maximum in winter, the pressure diminishes until the equinox; it then increases in summer, never however reaching the winter mean. A second minimum occurs in autumn, after which the curve gradually ascends until it attains the winter or absolute maximum. The following, for instance, is the curve for Berlin, drawn from a meteorological table extending over a greater length of time than any other within reach:—



The general cause of these oscillations is apparent enough. If the temperature of a column of air be augmented, the height of that column necessarily increases; so that the heated column may be conceived to overflow at its top, and therefore, as a whole, to become *lighter*. On this account the pressure should everywhere be less in summer than in winter; as we have said it is, in the main. Why these anomalies — several maxima and minima — exist, or why the curve of pressure does not exactly follow the curve of temperature, will appear in our next section; wherein we shall consider, *apart*, the habitudes of the aqueous and of the permanently elastic atmospheres. — III. The weight of the atmosphere over every locality varies also, according to a regular law, according to the hour of the day. Although plainly connected with *temperature*, this variation, like the former one, also presents anomalies. In the course of every twenty-four hours, the Barometer presents us with two maxima and two minima; recognizable in every set of observations that have been continued long enough to enable us to get rid of anomalies, or of the effect of irregular fluctuations. The hours at which these *crises* take place—or, as they are called, the *tropical hours*—vary somewhat with the place of observation; generally speaking, however, they may be taken to be as follows, in the northern hemisphere:—

Minimum of morning,....	3 h. 45 min. A.M.
Maximum —	9 h. 37 min. A.M.
Minimum of evening, ...	4 h. 5 min. P.M.
Maximum —	10 h. 11 min. P.M.

These tropical hours vary also in each place with the season; during winter they are nearer midday on the whole, by about two hours, arriving later in the morning and sooner in the evening. —Another feature of this diurnal oscillation is remarkable. Its amplitude or amount—which is best represented by the difference between the mean of the two minima and the mean of the two maxima—varies with the latitude of the place, the season of the year, and the height of the place above the level of the sea. It is greatest at the equator, diminishing as we approach the polar circle, where it becomes almost insensible. In *winter*, in all localities, it reaches its yearly *minimum*, rising to a *maximum* in summer. With the *height of the place*, again, these variations change in a complex way; the hours of maxima and minima becoming reversed. But as such changes will be better understood when referred to their causes, we shall postpone especial notice of them until next section.—It is of the last importance to the study of all the relations of the atmosphere, that the *mean daily height* of the barometer be determined at every place of observation; and, as the foregoing variations seem to render this a little difficult, the mode of accomplishing it may be specified here. If the height in question be measured every hour, or *very frequently* and at regular intervals, during the twenty-four hours,—the sum of the readings,

divided by the number of the times of observation, will give the daily mean. That same mean, however, may be obtained approximately by taking observations of the morning maximum and afternoon minimum. It is better, however, to take either of the following three sets, first at 6 A.M., 2 P.M., and 10 P.M.; or, secondly, 7 A.M., 2 P.M., and 9 P.M. The barometer attains its own mean height between mid-day and one o'clock—the moment varying according to the season.—IV. The weight of the atmosphere over any locality is likewise subject to other and apparently irregular fluctuations. These depend on *motions* within our aerial envelope itself, and will be treated in a subsequent section, as well as under RAIN, WINDS.

(3.) *Atmosphere, Causes of the Changes of Weight of. Influence of the Habitudes of its Constituents.*—We shall not attempt at present to explain the causes of those *permanent* irregularities in the weight of the atmosphere, which depend simply on the *latitude*, and apparently also the *longitude*, of the place. But the fluctuations adverted to in II. and III. paragraphs of the foregoing section can readily be accounted for. In the case of a permanently elastic atmosphere, a proposition, already hinted at, would be absolutely true:—*When the barometer falls in a country, it is because the temperature of the country is higher than that of the neighbouring countries, whether because it is heated directly, or because these countries are cooled; on the contrary, the rise of the barometer proves that this country becomes colder than those which surround it.* A proposition flowing out of one general and simple consideration, viz.:—In an atmosphere *in equilibrio* as to temperature, all its columns, from base to extreme surface, would at every locality be of the same height and weight. If one column be heated, it will, as has been said, expand and *flow over the others*, or, it will *lose in weight.* On the contrary, if a column is cooled, it will contract in volume; and the neighbouring columns will flow into it, and *augment its weight.* Taking this for our ground then, we would expect, that as a country becomes heated, whether during the course of the seasons, or according to the hours of the day, the barometer should fall there, and *vice versa;* in other words, the *maximum* of the barometer should occur everywhere, on an average, at *mid-winter*, and its *minimum* at *mid-summer;* while two opposite crises should occur in its diurnal course, at the hours of greatest daily cold and greatest daily heat. Now, although in the facts already mentioned there is a general correspondence with these laws, we detect apparent anomalies likewise. For instance, why have we that irregularity in the curve of the barometer during the season? whence the apparent *minima* in spring and autumn, and that comparative ascent in summer? And although, as day proceeds, the mercury falls between the periods of mean temperature—viz., 9 or 10 in the morning, and 9 or 10 in the evening—and an hour close on the hour of *maximum daily heat;* whence the singular *minimum* of an hour of the night corresponding so closely with the hour of *greatest cold?* By consideration of the different habitudes of the two *constituents* of our compound atmosphere, these apparent anomalies are easily explained. As already stated, we have a *vaporous* mass mixed with our permanently elastic envelope, comparatively small in amount certainly, but quite efficient, through its extreme sensitiveness to changes of heat, to impress *anomalies.* Now the weight of the vaporous atmosphere must evidently increase with the heat of the *season* and the *hour of the day*, and *vice versa.* Its relations to the barometer and thermometer are therefore the *inverse* of that of the permanently elastic envelope; and *out of this contradiction* the enumerated anomalies spring. Fortunately the two atmospheres can be separated. By the instruments of hygrometry (see HYGROMETRY, PSYCHROMETRY), the weight of the vaporous atmosphere is measured *by itself;* and by subtracting this weight from the total weight of the air, we obtain the weight of the dry mass of air surrounding us. Separating, then, these two counteracting influences, and attributing to each, its effect on the barometer, we reach the following conclusions:—I. When the effects due to the vaporous atmosphere are deducted, and we have the weight of the dry or permanently elastic envelope alone, the minima of spring and autumn disappear, and the heights of the barometer are inversely as the heights of the thermometer; the one *maximum* of the year occurring in the *depth of winter*, and the one *minimum* at the *hottest period of summer.* The declension of the weight of the composite atmosphere in spring and autumn is due to this:—In spring the pressure of the dry air rapidly diminishes, while there is no compensating or corresponding increase in the amount of vapour. This last increases rapidly towards the height of summer, compensating for and counteracting, in so far, the continued diminution of the weight of the dry air; and in autumn the aqueous vapour rapidly precipitates, while the weight of the dry air augments only slowly. Exactly, too, in proportion as the seasons differ from each other at any place—*i. e.*, according to the difference between mean winter and mean summer temperature—is the difference between the winter *maximum* and summer *minimum.*—II. The diurnal oscillations of the barometer or of the weight of the atmosphere, although somewhat difficult to explain in all places, evidently originate in this same complex character of our envelope. M. Dove, of Berlin, first proposed a separation of the effects of the two atmospheres, as the key to these irregularities; and the following are his general results:—Take away the effect or weight of the vaporous atmosphere, and the oscillations are reduced to one single curve; viz., from a *maximum* some time after midnight, or the hour of greatest

cold. to a *minimum* at the hour of greatest heat; the *crises* in the morning and evening disappear. The midnight maximum of dry air becomes the minimum of the composite atmosphere, because of the remarkable abstraction of, or condensation of vapour, necessarily occurring at this diurnal period. There is a morning maximum again, because of the rapid ascent of vapour as the temperature of the day augments. About three or four in the afternoon, the great descent of the weight of the dry air, counteracts the augmented weight of vapour, and constitutes a *minimum;* while the diminution of vapour towards evening, over compensated by the increase of weight of the dry air, occasions an evening *maximum.* The theory now given, may be carried out so far as to account for the varying *amplitudes* of the diurnal oscillations, according to latitude; so that these apparently difficult movements of the barometer, are really nothing more than one regular feature of the *climate* of a place.

(4.) *Atmosphere, the Habitudes of a Vertical Column of.*—It is now necessary to consider our aerial envelope in reference to another of its dimensions, viz., its height or depth, and the phenomena belonging to its various strata—arranged according to their altitudes. As explained in a previous article (ALTITUDES, MEASUREMENT OF), the densities of the various strata of air, or what is the same thing, the weight of the masses incumbent on them, decrease in geometrical progression as their height above the surface of the earth increase in arithmetical progression; which theoretical principle, coupled with facts deduced from observation, enables us to construct as approximately accurate, the following table:—

Height in Miles.	No. of Times Rarer.
0	1
$3\frac{1}{2}$	2
7	4
14	16
21	64
28	256
35	1024
42	4096
49	16384
56	65536
&c.	&c.

It is evident that at such a rate of diminution of density the atmosphere may virtually be said to have a physical limit not far removed from us: did it extend as high above us, as London is distant from the city in which we write (Glasgow), one cubic inch of the air we breathe, would be expanded so vastly that it would fill a sphere equal in diameter to the orbit of Saturn! But the outer boundary of the atmosphere is far less remote; one can scarcely conceive it extending higher than from 100 to 150 miles. There are two very important considerations intimately related with this rapid declension of the density of a vertical column of air.—I. The *temperature* of the atmosphere *diminishes as it grows rarer*, or as we ascend in it. The exact relation of this decrease of temperature and decrease of density is perhaps not accurately established; and it is plain that it must be seriously affected by the diverse winds prevailing in diverse parts of the earth, and other circumstances. Nevertheless, it may be stated that in our northern climates the diminution is not far from 1° for 270 feet of perpendicular ascent. Or more accurately, a general formula may be given in the following terms. Taking 85° as the mean temperature of the surface of the earth at the equator, and 58° the mean temperature at latitude 45°, we have for any other latitude ϕ, the mean temperature t, from this equation—

$$t = 58^\circ + (85^\circ - 58^\circ) \cdot \cos 2\phi$$

or

$$t = 58^\circ + 27^\circ \cdot \cos 2\phi$$

and if the place is h feet above the surface of the ground, or n times 270 feet,

$$t = 58^\circ + 27 \cos 2\phi - n$$

On ascending into the atmosphere, at every latitude, a point or altitude will always be reached at which it rather freezes than thaws; a point named the point or limit of perpetual congelation. This point is found with a general accuracy by the equation

$$32^\circ = 58^\circ + 27 \cos 2\phi - n$$

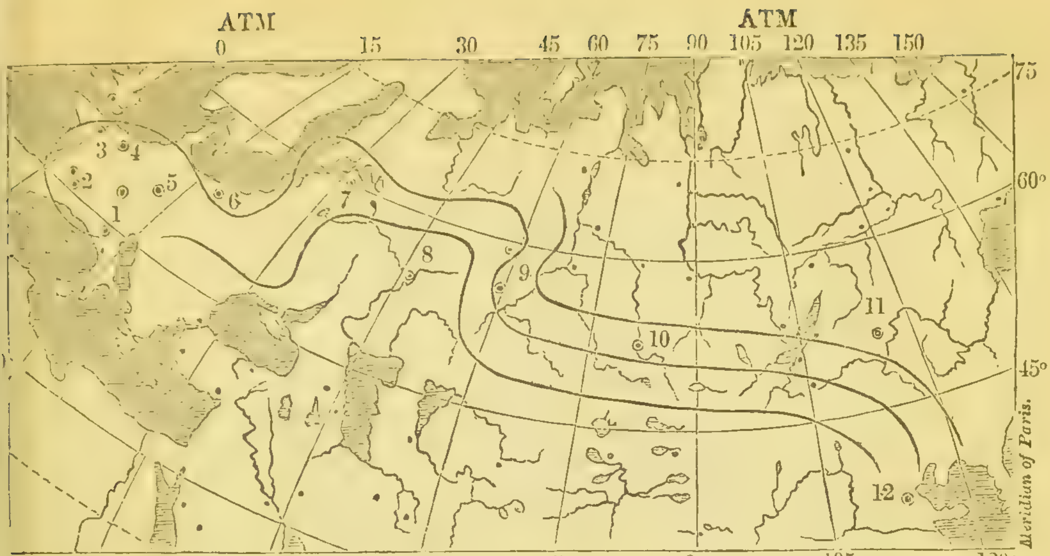
or

$$n = 26 + 27 \cos 2\phi$$

a formula which would give 14000 feet as the height of perpetual congelation at the equator. The actual height is greater than this; but the formula tolerably well represents the average elevation of the line at the various latitudes. No general formula, however, is applicable, unless the limit of accuracy be regarded as very wide, inasmuch as the height of perpetual snow in any country whatever, depends during any course of years, on elements having little connection with the latitude, such as the temperature of the plains or plateaux above which the snows fall, the degree of the heat and the duration of the summers, the quantity of snow falling during winter, the prevailing direction of the winds, the more or less *continental* character of the country, the dryness and transparency of the atmosphere, the escarpments of the mountain summits, and the masses of neighbouring snows. In the immediate proximity of this line of congelation, lying closely below it, is the region of the *glaciers*, those immense masses of moving snow and ice which have performed so important a part in modifying the present surface of our globe. II. This regular diminution of temperature, as we ascend in the atmosphere, induces very important effects on the *vaporous portion* of our envelope.

It is easy to see, in the first place, that since this vaporous portion, however small its relative quantity or weight, plays the remarkable part assigned to it above, in the production of the barometric oscillations at the surface of the earth, we should here expect a key to changes in the law of these oscillations, according to the elevations at which we observe them. The vaporous atmosphere diminishes in weight as we ascend, much more rapidly than the dry air, and its influence accordingly becomes less and less felt. But of much more importance is this,—*the vaporous atmosphere is constrained to adjust itself to the temperatures of the dry atmosphere;* and as these temperatures are not what its different strata would naturally assume, it must be regarded as placed by its associate in a *condition of restraint.* Out of this *restraint* almost all the phenomena of *hydrometeors* may be said to spring; and these again react on the dry atmosphere and produce in it remarkable changes. The topics now referred to, will be fully discussed elsewhere; especially in the articles on HYGROMETRY, HYDROMETEORS, and WINDS *(q. v.)*

(5.) *Atmosphere; Irregular Oscillations of, Weight of, Motions of.*—Besides the periodic oscillations now mentioned, the weight of the atmosphere over every locality is subject to changes, recurring indeed, but whose periods are not fixed,—originating in more complex causes. It cannot have failed to be observed, that, through effect of what has been explained, our aerial envelope must be in a condition of unceasing motion. That inequality of heat which belongs to the surface of our globe (CLIMATE) necessarily affects variously the columns of air superincumbent on the surface; these cease to balance each other in a state of rest; and interchanges of currents, with a view to the re-establishment of equilibrium, are the consequence. To facilitate an accurate apprehension of the character and effect of such motions, it is needful to distinguish carefully the two quite different ways in which any fluid,—whether liquid or permanently elastic,—can be agitated. These two ways are as follows: *first*, by effect of a transmission of its particles from one place to another at a greater or less rapid rate, as, for instance, in the case of *currents* of water or rivers, or aerial *winds* or *gales;* and *secondly*, by the mere transmission of *motion* or of *oscillations*, as in the case of the propagation of vibrations along an outstretched string. Of this latter sort are the tidal waves of the ocean, which, every observer knows, do not *transport a floating body on their surface*, that body remaining comparatively fixed, or only rising from the trough to the crest of the waves, and *vice versa*, as the waves themselves roll by; and of this sort also are those great atmospheric oscillations now termed atmospheric *waves*. The phenomena and influences of these two descriptions of motion are manifold; we shall give a brief account of them separately.—I. *Atmospheric waves.*—Every close observer of barometric changes must have noticed the remarkable fact, that, irrespective of diurnal and seasonal changes, there appears everywhere, a flow more or less regular, of what may be termed barometric *maxima and minima*, unaccompanied by winds, or at all events having no visible relation to winds: first, there occurs a minimum of pressure, then the pressure gradually rises until it attains a maximum, from which it again returns to a new minimum. These successions are evidently no repetition of one phenomenon, because the distance of time between the two minima, and the difference of pressure between the maximum and minimum, are not the same on the recurrence of the change; they are rather a series of individual phenomena, much of the same kind. It is observed too, that such changes at one place are attended by corresponding changes at many other places distant as well as near, so that the entire occurrence may be represented by the supposition, that a great atmospheric wave is being propagated across these localities,—the period of *maximum* pressure being the moment of the transit of the *crest* of the wave, while the preceding and succeeding *minima* indicate the passage of its trough. The distance from *minimum* to *minimum* would, under this view, indicate the *breadth* of the oscillation or wave, and the difference between the *maximum* and *minimum* pressure, its *amplitude*. The character and progress of these waves have not been closely studied until recently, and our knowledge of them is still very imperfect. By the labours, however, chiefly of Mr. Birt, who has wrought most effectively under the encouragement of the *British Association*, certain important results have been established. The Atmosphere it appears is always agitated by such waves; and there appear to be many systems of them,—one system often meeting with another, and occasioning curious and complicated *knots* and lines of pressure. The extent of a main wave is very great: of which the chart at the top of next page is highly illustrative. It traces the progress of a minimum for three successive days, over that vast stretch of country from *Paris* to *Pekin*, and indicates the velocity of its march. The bendings of these curves are very curious, being caused chiefly by the different velocities with which the waves are propagated over different regions. The lap round the Alps is especially noticeable. Mr. Birt has gone into much greater detail than we can follow here; he has even sketched the shape or section of several important waves in his various reports.—The various causes of these oscillations cannot of course be accurately ascertained, until the facts concerning them are known over the greater part of the earth. But something—although not the whole—is apparently due to disturbances occurring in the polar regions, through the sinking of the upper equinoctial currents of air (see WINDS), and the departure southward of the colder polar currents. If this interchange took



1. Parma. 2. Geneva. 3. Paris. 4. Brussels. 5. Prague. 6. Warsaw. 7. St. Petersburg. 8. Kazan. 9. Catherinerburg. 10. Barnaoul. 11. Nertchinsk. 12. Pekin.

Ordinary Propagation of Atmospheric Waves, with lines for June, 1841.

place exactly at the pole, we should have a regular propagation of waves towards the south, extending around the whole earth; but as it does not do so (see TEMPERATURE), there must arise a series of separate waves, stretching out towards the south along *sectors*, whose angles are more or less acute or obtuse, and generally interfering with each other. The following general conclusions, however, may be safely accepted as summing up our present knowledge of the phenomena and progress of these waves. *First*, The atmosphere is constantly traversed by several different systems of waves. These interfere or interlace, and so produce over every locality on the earth, a special condition of atmospheric pressure. *Second*, Amidst all the variations of such movements, there may be always traced a predominating system of waves belonging to and characteristic of the same climate, or large portion of the globe. *Third*, All atmospheric waves, in Europe and Asia, are propagated from north to south, although not with the same velocity; they move more rapidly in central Europe and in Asia, than they do in Russia and the countries around the Ural (see *Chart*). *Fourth*, Atmospheric waves are propagated with greater facility over the surface of oceans, than across continents. In general the asperities of the globe, especially chains of lofty mountains, diminish their velocity and also impair their intensity or amplitude. *Fifth*, This inequality of velocity on continents, the neighbourhood of the sea, and near mountains, explains the inflexions or sinuosities of the line in the foregoing chart, as well as of all other lines of progress yet known. *Sixth*, The velocity of propagation, as already said, is variable. As an average velocity we may state from eighteen to thirty miles an hour; at the Ural they do not move more rapidly than six miles an hour. *Seventh*, The direction and progress of atmospheric waves have no relation to the prevailing *winds*. It is with them exactly as with waves of *sound*, which are transmitted in all directions without regard to the winds, although these last sometimes modify both their intensity and velocity.—It scarcely requires to be remarked how importantly the progress and concurrence of these great oscillations must modify atmospheric pressure over every locality. The complete mapping of them is a task very far from being accomplished; nor until after that—though constant observation at vastly more numerous stations—shall have been achieved, would it be reasonable to expect a satisfactory theory of their origin and habitudes. One cannot avoid reflecting, that were the outer shell of our atmosphere self-luminous, these waves and their interlacings would present to a spectator from without, features far from dissimilar to that singularly mottled aspect of the apparent surface of the Sun.—II. The second class of motions characterizing the mass of the atmosphere, is much more noticeable, through the higher and more palpable nature of its effects. *Winds*, properly so called, are true *currents*—violent displacements of the aerial particles—just as the water of a river does not oscillate but flow onwards. The mainspring of all such *translations* has been already indicated, viz:—*if one vertical column of air be heated or cooled, a current is necessarily thereby determined towards the heated column or from the cooled one.* The physical relations and character of different regions of the earth render this inequality of heating an essential part of its constitution: for instance, the equinoctial columns of the atmosphere must always be warmer than the polar; the air over continents must be warmer in summer and colder in winter than the air over con-

tiguous oceans; the air resting over a desert of sand, is always hotter than that which overhangs a region of forest, &c. &c. *Winds* thus arise necessarily; they must exist always; and whatever their complication, they form, as we have said, an essential characteristic of our atmosphere. The subject which has now come before us being one of the most complex and extensive connected with the physical geography of the earth, cannot rightly be discussed as part of another article. We shall give a synoptic view of our knowledge concerning it, under the appropriate head—WINDS, to which general head we refer all aerial movements of translation, whether these are local or cosmical, gentle or so violent that they amount to *hurricanes* and *typhoons*,—generically to *storms.* It may be remarked in this place—in reference to much already adverted to—that each wind has, in every separate locality, its peculiar but determinate barometric influence, so that the variability of winds has to be subjoined to those many causes above specified, of variations of Atmospheric pressure. Compare the article just mentioned.

(6.) *Atmosphere, Heat of.*—Some leading facts regarding the heat of the atmosphere have been already spoken of. For the rest compare TEMPERATURE.

(7.) *Atmosphere, Optical Phenomena of.*—The Atmosphere acts upon the Ray passing through and into it, in various ways:—It *absorbs*, and therefore *reflects* it partially; it *refracts* it, and thereby evolves very interesting phenomena; it acts by *diffraction* and *interference* in producing curious appearances; and it also *polarizes* the solar beams. We shall do little more in this article than refer to the different heads under which the various aspects and operations just spoken of, are explained in sufficient detail.—I. The *colour* of the air is mainly *blue;* a fact apparent in the blue tint assumed by distant objects—as mountains—seen through it. The explanation usually given is, of course, this:—That the particles of the atmosphere *reflect* blue rays chiefly, and *absorb* or transmit the other rays. But the whole subject of the optical relations of gases, is at present vague and incomplete. It seems likely that a bundle or sheaf of solar rays, passing through large thicknesses of air, gradually loses the blue rays by *diffusion;* hence, perhaps, the circumstance that celestial objects near the horizon appear of the tint complementary to the blue, viz., the *yellow.* This same fact is indicated by the spectrum received from the sun at different altitudes. As the sun's altitude declines, the violet part of the spectrum gradually shrinks in comparative space; at last it disappears altogether, and the spectrum yields only red, orange, yellow, green, and a small portion of blue. The optical properties of aqueous vapour give additional interest to these phenomena. The yellow tints of the sun and moon, seen through clouds, are owing to these properties; hence the common saying, that a red setting sun is a sign of great humidity. It is, of course, not possible to separate the effects of the humid atmosphere from those of the dry one, or to say exactly what is due to the one and what to the other,—they being always intermingled. The only general conclusion we can reach is that, as with the sea, our aerial envelope belongs to that class of bodies which have two colours—one through effect of reflection, and the other from light transmitted.—Instruments have been devised for measuring the tints of the sky. see CYANOMETER, DIAPHONOMETER. See, also, STARS, *Accidental Colours of;* and DAWN and TWILIGHT.—II. The atmosphere plays an important part, through its power to *refract* light. Its functions in this respect are fully discussed under REFRACTION ASTRONOMICAL; MIRAGE; RAINBOW; HALO; PARHELION.—III. To *diffraction* and *interference* we owe the phenomena of CROWNS and ANTHELIA (compare these articles); and under the head POLARIZATION is placed a full explanation of that curious subject, the *polarization of the Atmosphere.*—These references will guide the student to as accurate a knowledge of the several subjects as our limits permit us to offer him.

(8.) *Atmosphere, Electricity of.*—It is well known, and will be explained in detail under appropriate articles in this Dictionary, that when the particles of a body experience any derangement, or undergo any change whatsoever, whether as to their natural position of equilibrium, in their grouping, or in their chemical constitution, there supervenes an immediate separation of a part of what are called the two electricities; or, to speak more rigorously, a certain development of the electrical polar forces. This phenomenon is invariably produced whenever the particles are shaken, or separated by friction, percussion, heat, light, chemical action, &c. Now this same separation of the electricities, or development of the polar forces, appears an essential element of relations between the earth and atmosphere; which two portions of the terrestrial spheroid are, through the influence of causes far from well understood, always in opposite electric states—the latter manifesting the positive force, and the former the negative force.—I. Premising that the instruments used in detecting the existence and variations of atmospheric electricity will be described under ELECTROSCOPE, ELECTROMETER, and MULTIPLIER, we shall first lay down, in due order, the general laws which the facts already accumulated seem to indicate on this curious subject. 1. As already stated, the electricity of the earth is *negative;* that of the atmosphere *positive:* extending to a slight distance from the surface of the earth (a distance varying according to the nature of the soil, but which in the *open country* is from three to five feet), there is a *neutral stratum*, above which the positive polar force manifests itself, increasing in intensity as we ascend. To ascertain the law of this increase were of great importance; and the object has

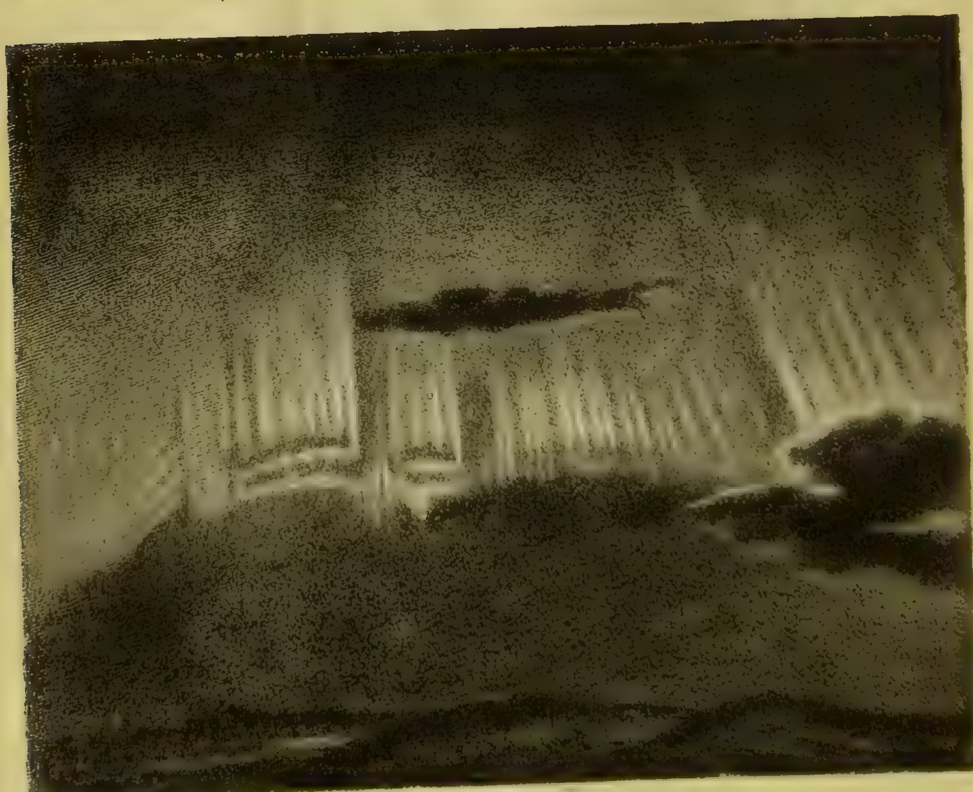
been sought to be accomplished by aid of *kites*, *captive balloons*, and *arrows* shot to various heights. The practical difficulties of the investigation, however, are very great, nor have any results more precise than the following been yet obtained:—*First*, If an isolated body near the surface of the soil, and previously in communication with the soil, be brought into contact with the electroscope, the gold leaves do not separate or give signs of electrical tension; but if the sky be clear, and the apparatus is carried to a height, the leaves diverge, and indicate positive electricity. *Secondly*, The tension thus indicated increases with the height. *Third*, The action between two bodies showing electric tension, takes place with increased facility, if one or both of them be easily vaporized. There is, therefore, more powerful action between the atmosphere and surfaces of water, than between it and dry surfaces. Vaporization is increased by this action; and vapours in a *negative* state are thus diffused through the air, and retain that state so long as their molecules remain in the vesicular state. (See Clouds). *Fourth*, When the air is dry no electric indications can be obtained, unless through means of very long rods; when it is slightly humid, on the other hand, continuous currents appear, although the rod of the instrument be comparatively short.—2. During serene weather the tension of atmospheric electricity undergoes regular variations, according to the hour of the day and the season. We owe most important knowledge on this subject to Schubler of Stuttgard, Arago of Paris, Quetelet of Brussels, and lately to the meteorological Observatory at Kew. It appears, *first*, that we have everywhere two daily *maxima* and two daily *minima* of tension. At Stuttgard, for instance, there is a *minimum* at *four* in the morning, a *maximum* at *eight*, a second *minimum* at *five* in the afternoon, and a second *maximum* between *eight* and *nine* at night. And, *secondly*, that the *greatest* intensity is in *winter*, and the *least* in *summer;* or, rather, that in serene weather the intensity of atmospheric electricity is in proportion to the increase of *cold*. The cause of these variations is probably the following:—Towards midnight the electricity of the atmosphere ought to be feeble, because the *humidity* of the evening and the first hours of the night must, through its conducting power, have transmitted to the earth a portion of what had previously accumulated in the air; when the sun has risen with its heat, the vapours *rise*, instead of *falling*; so, all discharge in this way ceases; as the sun's heat increases, and ascends to its maximum, the air gets quite dry, becomes a bad conductor, and thus affects the instruments only slightly; finally, as the sun descends, vapour is precipitated, and for a time transmits electricity very abundantly from above—so that we have daily two apparent maxima and two minima.—In the same way the annual variations can be explained. During the heat of summer the air is dry, and a bad conductor; while in winter the humid air is a good conductor.—It would seem, therefore, that these maxima and minima rather relate to the conductive power of the inferior strata of the atmosphere, than to the absolute tension of atmospheric electricity itself.—II. The foregoing results, imperfect as they are, do not hold except when the sky is clear. The formation of *clouds* originates other electric phenomena, of great complicacy, and sometimes of remarkable splendour. The student will consult Cloud, Thunder-storm, Lightning. — III. Certain imposing *meteoric phenomena* are likewise due to the electric state of the atmosphere. See Water-spout and Hail-storm.—IV. As to the origin of atmospheric electricity, a few words must suffice, nor are these proposed in confident solution of the difficulties of this obscure subject. The inquiry as to the origin of any development of the electric forces is tantamount to this:—What changes are proceeding among molecules of the bodies in connection with which the development takes place? The most evident and extensive change of molecular condition, connected with the relations of the earth and atmosphere, is *evaporation*. It is established by decisive experiments, that during the process of the pure and simple evaporation of water, at the surface of the earth, no palpable electricity is disengaged; the negative electricity of the foam and vapour produced at *jets-d'eau* and cascades being referable to quite another cause: but there is a certain development of the polar forces, during evaporation from *saline masses*. Something of the phenomena in question may also be attributed to those innumerable chemical reactions that occur within organized bodies; although as these reactions take place in very different directions, and the gases that escape are continually touching the surfaces and interior parts of these bodies, it is certain that quantities of developed electricity, must be *immediately recomposed*. Only one other resource remains, viz., *thermo-electricity*. It is well known that the unequal distribution of heat in a heterogeneous metal suffices to separate the electricities; the portions which are most heated taking a negative electricity, and those which are least heated, positive electricity. It is not improbable that we ought to consider the earth and atmosphere under this relation. The higher parts of the atmosphere, because of their rapidly diminishing heat, ought to become more and more positive, while the earth should show an increasing negative intensity downwards to its centre. This explains the fact too, of the greater separation and display of electric forces in the tropic regions; for there the foregoing contrasts are the greatest. The intensity of these phenomena naturally diminishes as we pass towards the temperate zones; and it may be that the polar regions, where the aerial gradation of heat is comparatively inconsiderable, serve as a point of re-union to the polar forces disengaged elsewhere; and it

may be, in so far, on this account, that they are the theatre of the brilliant displays known as the Aurora.

Atmospheres of the Planets. See Appendix.

Attraction. Among the various motions visible in the material universe, there is a large and important class, resembling, in every way, what would take place if one portion of matter *drew* other portions towards itself; for instance, as with the magnet and a piece of iron: to this class of phenomena the name or term *Attraction* has been assigned. The phenomena in question may be divided into *direct* and *indirect*, or, perhaps, *simple* and *composite*. The *simple* instances of attraction in the universe are countless. Foremost of all, we have the case of *falling bodies*, first scientifically treated by Galileo, which originated the idea that the earth, and perhaps *every portion of it*, have a significance amid surrounding motions, as if they drew all other matter towards them. This latter extension of the phenomena of terrestrial attraction, was completed by the demonstration by Maskelyne, of the effect of the mountain Schehallien on the plummit, and by those curious experiments by Cavendish (since repeated by Mr. Baily and others), which demonstrate that balls, of perfectly *unmagnetic* metal, visibly attract each other. To this same class must be referred direct magnetic attraction, electric attraction, &c. The indirect instances of attraction have the following characteristic. Attraction is not seen in them *purely:* it is inferred as one of the simple or primal constituents of a *composite* phenomenon. For instance, in case of a trajectory or projectile, the attraction of the earth enters as an element; for when we disengage, from the motion of the projectile the effect of the *horizontal* impulse given it, we find, alike in its ascent and descent, the precise phenomena of falling bodies. Again, the planets do not fall to the sun, as a stone does to the earth; but if we disengage from their curvilinear motion the effects of a primal impulse onward, we also detect as a *residuum*, this tendency to fall towards the sun, *as if* they were drawn towards him. In the same way all chemical phenomena are conceived to indicate attractions indirectly; so that it has come to be held as an assumption, which may be accounted general in physical inquiries, or, in other words, a *physical axiom*, that all matter tends to attract or draw towards it other matter, unless in the case of opposite *polarities* (see Polar Forces). Further speculative views on this subject are given under the article Boscovich. There are two very distinct and disparate lines of thought and inquiry regarding these phenomena of *attraction*. 1. The investigation of the *laws* or the *order* of those motions to which we apply the epithet *attraction*. In regard to *falling bodies*, this was at once begun and accomplished by Gallileo. Newton finally disposed of the important question in reference to that veiled attraction towards the sun, which is indicated by *planetary orbit*, and *velocities:* others have wrought, and are working, in the same field, as to electric, magnetic, and chemical, and other physical attractions. 2. It is quite otherwise with inquiries as to the *physical cause* of the phenomenon named *attraction;* of this, physical science can never know anything. It may discern relations connecting the various attractions, and thus, it may be, ultimately assimilate them, or discover some simple law that comprehends all their diversities; but the material world can never reveal anything but *sequences*, and the order of *sequences;* so that the inquiry as to the *efficient cause* of such phenomena is beyond reach of physical inquiry. The frequent misconceptions as to the physical significance of the word *attraction*, and the mass of useless speculation thereon built, would have been avoided, had attention been paid to the important truth now indicated; nor can we altogether exempt from censure those speculations by Laplace, which seemed to this prince of mathematicians but very indifferent metaphysician, to intimate almost an *à priori* necessity for the *law of gravitation* of Newton. See *Systeme du Monde.*

Atwood's Machine. Perhaps no questions in mechanics are more interesting than those concerning the fall of bodies. They were, however, for a long time the subject of but very slight and inefficient experiments. Bodies fall in so short a time through so considerable a space, that it was found impossible to get to elevations fitted in other ways for the purposes of experiment, sufficient to let us observe them easily. Besides, the resistance of the air, though at the commencement of a body's fall very slight, becomes yet considerable as its velocity increases. The machine of Atwood proposes to reduce the velocity of falling bodies, and so to enable us to observe their laws, by giving us time for experimenting, and by rendering the resistance of the air comparatively insignificant. It accomplishes this object thus:—A string, to which two equal weights are attached at the two ends, passes over a pulley, and remains in equilibrium. A very small weight (small compared with either of the equal weights) is then added at one end, and the string at that end commences to descend. It is evident that the force which gravity exerts upon the descending equal weight is exactly balanced by that upon the ascending. The only force acting, therefore, upon the weights is the force of gravity pulling the smaller weight down. This force is used to produce an equal motion upon the three weights together; and we have thus, calling g the force of gravity operating on these three weights, if left free, a force $= \frac{g\,n}{2\,m + n}$, where m represents either of the equal, and n



e smaller weight. This force will be equal to

$$\frac{1}{1+\frac{2m}{n}},$$ where $\frac{2m}{n}$ may be made as large

we choose, by sufficiently increasing m and ninishing n, and where, consequently, the oportion of the force acting upon the mass, to e natural force of gravity upon it, may be as nall as we choose. The velocity will be proportionally small, and the times of describing ensurable spaces will be easily measured also. 7 the help of this instrument we may establish, perimentally, the following laws:—That the aces described by a body acted upon by a constant force are proportional to the squares of the nes, and that the velocities acquired by the body e proportional to the times. This last law will observed, if, after finding to what points the dy reaches in the successive seconds, rings be aced, which shall permit the larger body to ss through, and which will detain the smaller. ie motion will continue with the velocity impressed at the moment of this change, and without crease or sensible diminution. Practically, a ock beating seconds is attached to the machine, d a very delicate machinery is employed in ndering the friction of the cord upon the pulleys slight as possible. This does not, however, terfere in the least with the principle of the apparatus.

August. The eighth month of our year. It as originally called *Sextilis*, or the sixth month, om the position that it held in the year of omulus. It received its present name in honour the victories of the emperor Augustus, 8 B.C.

Auriga. A northern constellation.

Aurora Borealis, or, rather, the POLAR URORA,—that which appears in the Arctic reons being named *Borealis*, while the Aurora the southern pole is the *Australis*. This ngular and most beautiful phenomenon consists *mellow lights variously coloured*, which dart, certain seasons, from all parts of the horizon polar countries; and it is usually announced id accompanied by great magnetic perturbaons, indicated by startings of the needle. In ir regions of the earth, the Aurora appears first usually like a dingy fog, in and somehat above the northern horizon, and generally ither brighter towards the west. By and by iis dim mass takes on the form of a circular egment, resting, at each end, on the horizon; ie higher part of it being surrounded by a hite light, sometimes resolving itself into one r two distinct luminous arcs. Then begin these 'ell-known beams and shoots of diverse colours, riginating in the obscure segment, which they reak up into bright patches, as if they threw ie whole of it into a sort of palpitation. When ie Aurora is extensive, these beams, although onstantly shifting, converge towards the zenith, where a centre, or superb auroral crown is formed. As the phenomenon diminishes in intensity, the jets continue, but the crown shifts, and is seen sometimes on one side of the sky, sometimes on the other: at length these movements cease; the light withdraws itself nearer and nearer to the western horizon; the obscure segment, as it too diminishes, becomes luminous; at length, every trace of it disappears. The most complete account given of the northern Aurora, as seen in its own latitudes, is that by by the French scientific Commission, which, in 1838-39, spent a full winter at Bossekop, in West Finmark, N. lat. 70°. We shall describe the main features of the phenomenon, on the authority of M. Lottin, one of the members of the commission—referring for details to the great work published at Paris, at the expense of Government. In the evening, between four and eight o'clock, the light mist which prevails almost constantly to the north of Bossekop, to the height of about 4° or 6°, becomes coloured over all its upper rim, or rather appears fringed by the light of the Aurora existing behind it. This bright border soon grows more regular, and merges into an indistinct arc of pale yellow, whose extremities rest upon the earth: an arc which rises gradually up into the sky, its keystone always nearly coinciding with the magnetic meridian. Soon afterwards dark streaks separate the luminous matter of this arc; the well-known rays are then formed; these stretch out, and draw themselves in, now slowly, now almost instantaneously; they dart, shiver, and dance,—augmenting and diminishing suddenly in splendour. The feet, or roots of the rays are always especially bright, and continue during all the display to form a shining arc more or less regular. Their length is very various; but all converge towards that point in the heavens indicated by the prolongation of the south pole of the *free* magnetic needle. At times they are quite prolonged, *en masse*, to this point of union; forming there the fragment of an immense luminous cupola. The *arch* in the meanwhile is itself mounting towards the zenith, and is generally shivering, as if with undulations; these undulations or waves of light, passing for the most part from west to east. Sometimes, although rarely, retrograde undulations may succeed; the waves, after passing from one side of the heaven to the other, retracing their steps, and flowing back to their point of departure. This alternation of waves in the luminous arc, is sometimes surpassingly grand. At one time, the alternating movement has the appearance as if a brilliant curtain were above us, its folds agitated by the winds; and, again, the edges of the arc having separated from the actual horizon, their folds become very complex, inwrapping each other, and presenting to the astonished spectator a variety of the most graceful curves. The remarkable appearances now described, are presented in our engraving. The

brilliancy of the rays or shooting sheets, is, during these extraordinary changes, subject to sudden augmentations of intensity; so that it comes to surpass that of stars of the first magnitude: they dart out with extreme rapidity, and the curves spoken of, form and reform, as quickly as the twistings of a serpent. Then the rays become coloured; the base red, the middle green; while the rest of them presents a clear yellow. These colours preserve, without exception, their respective positions, and are always of admirable clearness; the red is like blood; the green of a pale emerald. Soon, however, the phenomenon shows signs of having exhausted its vigour. The splendour diminishes, and the colours disappear; the arch, meanwhile reforming, continuing its ascensional progress and approaching the zenith, and the rays which dart from it becoming, therefore through mere effect of perspective, shorter and shorter. At length the summit of the arch reaches the *magnetic zenith*—(the point indicated by the prolongation of the *free* magnet). The base of the rays is thenceforward alone seen; if they continue coloured, they seem like a large red band, through which the green of their upper parts can be distinguished, and if the undulating motion formerly spoken of, has not ceased, their feet continue to form a long sinuous and undulating zone. In the interval occupied by these changes, other or secondary arcs appear; and while they preserve their distance, they exhibit a succession or regular series of aspects, such as has been described. Sometimes, however, they approach and mingle with each other; so that it is easy to conceive that the appearances produced are literally indescribable. The celestial vault becomes an immense and magnificent dazzling cupola, overhanging a world covered with snow, which again is environed and set within an ocean black as pitch.—So much for the general phenomena presented by the Northern Aurora. In an elaborate memoir published by the French Commission, M. Bravais has subjected all important details to a close scrutiny: he has spoken minutely of the *Obscure Segment;* of the *Arch*, the *Rays*, and the *Crown;* of *Auroral Sheets;* the motion of *Palpitation;* of the *intensity* and *colour* of the *Auroral Light;* and of its *distance*. The latter is represented as between 60 and 120 vertical miles—miles upwards from the earth's surface: the determination is very uncertain; as, indeed, is sufficiently shown by its indeterminateness. No doubt, however, the auroral light is *thick; i.e.*, there is a large interval of space between its inner and outer surface. The chief value of M. Bravais's determination, will be found in what we have now to say regarding conjectures as to the nature and cause of the phenomenon. These are designated expressly as *conjectures*.—It is needless to dwell on the earlier theories. That such men as Halley, Mairan, and Dalton, chose to put forth explanations concerning a phenomenon they had really never seen, or whose phases—not the most ordinary—had received any determinate or numerical *fixing*, only shows that the exercise of the generalizing faculties is comparatively pleasant, and that the force of them, in most good minds, quite overpowers the checks which ever and anon are applied by persons having more of the *observing* instinct.—M. Biot is an authority of a different order; he knew something, although not everything, of the totality of the phenomenon;—we do not think, however, that he made much of it. Holding by the theory of its *electric origin*—a doctrine suggested by its evident connection with terrestrial magnetism—he considers its elements probably composed of metallic particles of an extreme tenuity, that serve as conductors between the various atmospheric beds, which are known to be very unequally charged with electricity. Suppose then, two similar columns suspended vertically in the atmosphere, the electricity of the beds of air lying from the top to the bottom of these columns, will find these conductors more or less perfect; and if the tendency of the electricity to get into equilibrium surpasses the resistance opposed by the imperfect conductive powers of the said columns, a long vertical discharge will follow, sparkling from one metallic molecule to another; variously to the spectator, because he looks through a *thickness* of beds, and so producing a shifting auroral phenomenon. Is it possible to imagine a grave philosopher more grossly mistaking his function than M. Biot has here done? We acknowledge his great services; but, in this instance, he is only playing at toys. A more rational speculation by far, is that of M. Kaemtz. He attributes auroral phenomena to an electrical *induction* manifested in the atmosphere, and produced by changes in the magnetic intensity of the globe. Neither will this notion, albeit rather a favourite one, account even for the known facts.—It is utterly vain to search at present for a theory of the Aurora. What is known is this,—the direction of the auroral *jets* or *rays* and the position of the *crown*, have a connection with the magnetic meridian; and the aurora produces great magnetic perturbation.—Further lights as to this singular subject may be found by the student under our various notices of ELECTRICITY and MAGNETISM. As to theory or explanation, we must even *observe* and *wait*.

Autumn. The third season of the year; so called from the *increase* of the fruits and plants coming to full maturity. It commences on the 23d of September, or sometimes the 22d, the day of the autumnal equinox, just when the sun is about to enter the constellation Libra. It closes on the 21st or 22d of December, when the sun enters Capricorn. It lasts, in all, 89 days, $16\frac{5}{12}$ hours. During its continuance, as the sun is continually descending in the signs, the days grow shorter and shorter, and the nights lengthen.

Axe or **Axis.** It is difficult now to define this technical term, its use has become so ex-

sive, and its meanings so varied. Perhaps following will be found to comprehend most its legitimate significations:—If any line lds a symmetrical position with respect to any stem, motion, or phenomenon, or if it be such its relations to that system, motion, or phenomenon, that it is convenient to distinguish it a specific name; that line is termed an *axe* *axis* of the said system, motion, or other phenomenon. Very many applications of the term s, justifiable by no closer definition, will be ind throughout this Dictionary.

Axes, the Principal. A term indicating a y important phenomenon and general theorem that part of rational mechanics which relates to *rotation* of solid bodies, or rigid systems of points. iefly, the subject may be explained as follows:—ny solid body, of whatever figure, is struck in y place not exactly opposite its centre of gray, it will begin to *rotate*, as well as to move wards. It rotates at first around an axis, med the axis of spontaneous rotation, and position of which depends on the kind of w given. But this axis is not generally the ed one. Unless that happens to be a *principal* s, it shifts; and very soon the body comes to ate around an axis which it retains, or which s not shift: this is called a *principal axis.* w, every rigid system has at least *three such* es, or *right angles to each other*, and these called the *three principal axes.* Some bodies rigid systems have many such—in a sphere, instance, every diameter is a principal axis; t, as already said, every rigid system has at st three. The position of these axes can be ermined in every case by analytical processes, thout much either of skill or trouble. The *incipal axes* at one time played a considerable rt in mechanical theory, and they are still of nsiderable importance. They enjoy one prorty, which, as it is often referred to, may not fitly be explained in this place. There is a hnical phrase connected with rotatory motion, name *moment of inertia.* The meaning of it ll be understood from what follows. Suppose solid body, or a rigid system, in the act of ating about an axis, with a definite angular ocity; and while it so rotates, let us fancy a rticle, m, at rest suddenly attached to it, at a tance of r from the axis. It is evident that angular velocity will now be diminished, cause the body has got the new particle m to g along with it. Now, it is diminished to following extent:—The angular velocity in first instance is expressed by a formula in a *ctional form*, and to find the new angular locity we require to add the quantity $m \times r^2$ to *denominator of that fraction.* The quantity r^2 is called therefore the *moment*, or the effective ount of the *inertia* of the added particle—the asure of the force with which it has dragged ck the system to which it had become attached. ich being the measure of the moment of inertia of a single particle, it is easy enough, by analytical methods, to determine the moment of inertia of any number of particles, or of any body or rigid system, with respect to any axis around which rotation takes place. Now, the *principal axes* enjoy this property,—with regard to one of them, the moment of inertia of the rotatory mass is greater than with regard to any other axis passing through the point at which they intersect; and with regard to another of them, the same moment of inertia is less than with regard to any other axis. In other words, we have the condition at once of a *maximum* and a *minimum* value of the *moments of inertia.* The geometer who first very clearly investigated this curious subject was Euler.

Axiom. *Logic*, *deductive* or *inductive*, has no power save this,—it can elicit new matter out of premises, or combine and generalize separate facts or determinations. A *deductive science*, then, must accept or presuppose all those premises, out of which, whether by analyzing their contents, or by combining them variously, its whole possible fabric must be reared; and an *inductive science* must equally accept, as its foundation, certain general truths or laws, and certain general principles or laws of combination, on the ground of which, and by means of which, its further discoveries are to be interpreted and co-ordinated. Those fundamental *elements* or *acceptances* are AXIOMS. The more general the science is, the more general are its axioms; but every *special* science, logically considered, must have its *special* axioms. The sources of axioms, or the roots from which they spread, are threefold. 1st. There are laws of the mind, in obedience to which alone the logical faculty can contemplate either the external world or the mind itself. The laws which have regard to the latter are often in metaphysical language termed *intuitions;* but the word is faulty, for the laws in question are very various. The laws under which we contemplate the external universe, on the other hand, are, although various, not very complex; they are included under what Kant has termed the *laws* of our *sensibility*, and the *categories* of the *understanding.* 2d. Propositions that result from universal experience in the elementary physical sciences, such as the sciences of *motion* and *force*, must be accepted by them as axioms. But the certainty of this latter class of axioms is less than that of the former. The laws of the observing faculties are necessarily universal; but the results, or so-called results, of universal observation, must, at the best, rest on but secondary evidence. 3d. Arranging the sciences according to a hierarchy, or in order of their complexity, the elements or axioms of the higher class must always rest upon the demonstrations of the lower. This third class of axioms, accordingly, has only a third or still inferior degree of certainty. For practical illustrations of the doctrines here expressed, compare

Geometry; Motion, Laws of; Parallel Lines.

Azimuth. The azimuth of a body is an arc measured on the horizon, intercepted between the meridian or circle through the zenith of the place and the poles, and a circle through the zenith, the nadir, and the given body. The altitude of the body is measured along this circle, upwards, from the nearest point where it meets the horizon. It is evident, that when we have given the altitude and azimuth of a star at any given moment, we shall be able to point out its exact position in the sky.

Azimuth Compass. See Compass.

B

Balance. The balance is employed as a measure of the quantities of matter contained in substances. If all bodies were constituted similarly—if their density were the same, and their constituent atoms alike in nature as well as shape —we should have a sufficient measure of the quantity of matter which a body contains in the volume which it fills. Bodies are not, however, thus similarly constituted, and, when we wish to compare their masses, we require to look for another standard. We find this in the principle that equal forces, or the same force, will produce effects upon bodies proportional to their mass. Thus, if two equal bodies (equal in mass) be solicited by equal forces, their motions will be, in every respect, similar. If two equal bodies, under the action of certain forces, have acquired, at the end of any given time, the one a velocity sufficient to carry it through 10 feet in the next second, and the other a velocity capable of carrying it through 20 feet in the same time, the last force is double of the first. If, again, the body in the first place have been three times in mass that in the second case, the latter force will be only $\frac{2}{3}$ of the former. In the case of the balance we have equal forces acting upon equal masses. The two motions which would result are, as it were, however, set over one against the other, and there is obtained, therefore, no motion. The two bodies to be compared are set, the one at the one end, the other at the other end of a bar, which rests at its point of bisection upon supports. The principle of the lever teaches us, that, if the bar be truly bisected, it will turn, upon its supports, in a vertical plane to the side of the greater weight. We see the same principle illustrated every day, by children riding on logs of wood, for example, where, if the arms of the log, from the point of support upon which it turns to the place where each boy sits, be equal, it immediately inclines towards the heaviest. The least inclination, therefore, of the bar from the true horizontal, indicates to us the inequality, either of our masses, or of our lever arms. If we are confident of the perfect equality of the latter, we may be certain that the former are unequal.

This is the general principle of the common balance. As, however, perhaps no instrument is more important in physical investigations, we will make a few remarks upon the methods which have been employed to secure a perfect balance.

There is one method employed to secure a true result, from even an imperfect balance, which is very ingenious and very useful. We refer to the method of *double weighing*. It is almost impossible to make the arms of a balance exactly the same in length and thickness. There is always some little difference, which, in extreme cases, might come to cause errors perfectly sensible were it not for this method. Even the influence of temperature upon a body like the bar of iron usually employed for the *beam*, necessarily not quite homogeneous throughout, causes considerable deviations. The method of double weighing is as follows:—We first bring the given body to exact equilibrium, by weights placed in the opposite scale. Then take it out of the scale; and restore the equilibrium thus disturbed by placing shot, or some weights, the exact amount of which you know, in the scale, instead of the original body. The weight of this quantity of shot will be the true weight of the body. It is easy to see how this method of measurement gets rid of all such causes of disturbance as inequality of the arms. We have simply two bodies successively placed in the same circumstances, and producing successively the same effect upon a third body, which has remained throughout the process in the same circumstances. As this effect depends (being the action of the *force* of gravity upon the bodies) upon the mass of the bodies, we must conclude their masses to be exactly equal.

The two conditions which it is most important that a balance should fulfil are *sensibility* and *stability*. The former requires that a very small difference of weight shall cause a deviation from the horizontal line. Thus, Ramsden made a balance for the Royal Society, so sensible, that one-millionth part of a pound was capable of turning it. Balances are now quite commonly constructed capable of detecting differences in weight of a thousandth of a grain, or one seven-millionth of a pound. The latter requires that the balance, when disturbed, shall rapidly return to rest. We may see at once that there is something of opposite in these two conditions; the latter being such that, when it obtains, it may destroy a deviation from the horizontal before it has become noticeable—that is, such that, even with a small inequality of weight, the balance may apparently be in perfect equilibrium. In fact, they may be shown theoretically, not indeed to be quite in-

mpatible, but to be so to a considerable extent. In the mercantile balance, stability, therefore, being there important for the economy of time, is the quality chiefly desiderated. In the philosophical balance, again, where economy of time is of much less importance than perfect accuracy, sensibility is principally sought. The condition of stability is the following:—That the centre of gravity of the beam and scales should be *below* the point of suspension. The stability is greater the farther the centre of gravity is below the point of suspension. In that case it will be clear that any disturbance of the balance, while it does move its centre of gravity, will move it through less angular space, when we take the point of suspension as the centre of angles, and that, in consequence, the vibrations will more speedily cease. It is more important, however, for us to know the conditions of the *sensibility* of the balance. One evident condition will be, that there be as little friction about the points of support as possible. Could it be that there should be no friction, the very smallest difference of weight would move the balance from its place; for nothing, in that case, would oppose the tendency of the preponderating weight, until the deviation of the centre of gravity of the instrument from the vertical position, under the point of suspension, became great enough that its tendency to its original position under this point should come into play. In consequence, the beam rests upon what is called a knife-edge, supported upon highly polished agate planes. Similarly, the little ring, by which we suspend the weights and scales, should have an inner surface of agate plate, or, at all events, of highly polished steel; and the lower edge of the hole through which the ring passes should be a knife-edge. In order to ensure the highly polished steel of these knife-edges from losing its sharpness too speedily, the beam is lifted up by a sort of fork, from the ordinary point of support, when the balance is not in use; this being screwed up, by a screw at the bottom of the balance, so as to catch its arms on either side. To prevent, further, the accumulation of dust, or the gathering of moisture, upon the balance, and the consequent rusting and increase of weight of parts of it, it is usually kept in a glass case, within which a salt, such as chloride of calcium, which absorbs moisture strongly, is contained. It is important further to notice that, as steel and iron are very apt to acquire magnetic properties, especially if kept where magnets are kept, as in philosophical instrument rooms, frequently, the balance should be constructed, as much as possible, of brass.

There are some points to be attended to in the plan of construction of our balances, besides these mere mechanical details, which it is important to notice. Thus,

1. All other things remaining equal, the sensibility of a balance will be increased as the length of its arms increases. The reason for this will readily be seen. The preponderating force, when there is one, acts with a longer leverage. (See LEVER). It is farther from the point of support, and although, therefore, itself less, it may produce an equal effect with a greater force acting with shorter leverage.

2. If we add to the weight of the beam we diminish the sensibility of the balance. The reason for this, also, will be readily apparent. The weight of the beam presses downwards upon the point of support. Now, the heavier this pressure is, the greater will the friction on that point be (see FRICTION), and that in the exact proportion of the pressure. According to this same principle, it is worthy of notice that the sensibility will increase according as the loads in the scales diminish. The two loads act together at the middle of the beam, and produce also a pressure of the knife-edge upon the agate plane. The greater this pressure is, the greater will be the force of friction which must be overcome before a slight difference in weight will produce a deviation from the horizontal position of the beam. In ordinary philosophical balances, however, we have not often need to consider this latter application of the principle. The weights tested are usually very slight, and bear, in consequence, a very small proportion to the weight of the beam and scales, so that the frictional force is increased by them in an indefinitely small ratio. If, however, we measure considerable weights, it is necessary to keep this in mind when we decide on the sensibility of the balance.

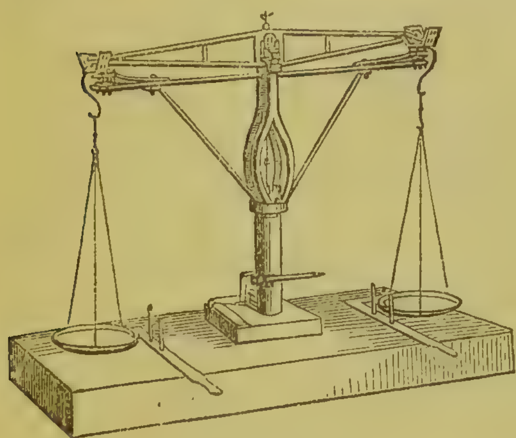
3. A third law is, that the sensibility is increased by diminishing the distance of the centre of gravity of the beam from the point of suspension. We saw already how this diminished the stability of the balance, and it will be clear that, so far as this goes, the stability of the balance will be exactly in the inverse proportion of its sensibility. The same principle will require the centre of suspension to be very near the point which bisects the line joining the points at which the scales are suspended. It is at this point that the weight of the scales and the load acts, when the beam is truly bisected by the point of suspension, and this point ought to be near the centre of suspension, for the same reasons as the centre of gravity of the beam. The coincidence of either of these points with the point of suspension would tend to produce unstable equilibrium.

A needle is usually attached to the centre of the beam, so as to be exactly perpendicular to the line of the points of suspension; when this hangs in a vertical direction, the balance is in its true position of equilibrium. There is frequently a graduated scale attached, by which we may read off the angle of deflection. We are thus enabled, also, to tell when equilibrium will be restored, before it has taken place, by the equality of two successive deviations of the needle from the vertical.

In order, further, to give us a power over the

sensibility or stability of the balance, according as we require it for different purposes, a small *nut* is attached to a screw at the top of the beam, which we can screw down—so lowering the centre of gravity of the beam—or screw up—so raising it—so as to have the distance of the point of suspension from the centre of gravity of the beam, under certain limits, what we desire it to be. It is evident that the possession of such a power is a desideratum, when a balance may be required either for very delicate philosophical investigations, or for more ordinary purposes.

We subjoin a figure of the ordinary balance.



Bent Lever Balance depends upon nearly the same lever principle as the common steel yard. It is an elegant instrument, but this we believe to be the best that can be said of it. It consists of a bent lever, fastened at the centre on a pivot, having at one end a scale suspended, while at the other the lever ends in an index needle, just before which is a heavy weight. This weight keeps the lever nearly vertical when there is no weight in the scale. There is a graduated scale annexed to the balance, along which the needle runs when the lever moves. When a weight is put into the scale, the tendency is to raise the lever nearer to a horizontal position—and the needle indicates the extent of this. We can easily comprehend then, how the instrument can be graduated just as the common steel yard is. It has not its defect, that the standard weight employed may be lost, and not easily recovered. The weight here is part of the lever arm itself.

Hydrostatic Balance.—This instrument depends upon the hydrostatical principle, that bodies immersed in a fluid are subject to a force, resisting gravity, and equal in amount to the gravitation of quantities of the fluid contained under volumes equal to those of the bodies. In consequence, they lose just so much of their weight when they are weighed while in the fluid, as the weight of an equal volume of the fluid itself amounts to (HYDROSTATICS). Taking advantage of this principle, the hydrostatic balance is chiefly employed for the measurement of specific weights, or specific gravities of solids and liquids, and of densities where the body weighed is homogeneous. It consists, in its simplest form, of an ordinary balance, to the bottom of one of the pans of which a hook is permanently attached—so as not however to disturb the equilibrium—the pan itself being lighter than the other by the weight of the hook. A given body is then—if it be a solid—weighed as usual, and its exact weight recorded. A fine silk thread is then attached to the hook at the bottom of the pan, and if great accuracy be required, the weight of this, if at all sensible, will be also noted down, and used to correct the result. The body is then attached to the thread, and dropped into the fluid. If the weights which formerly produced equilibrium have been retained in the scale, they will now draw the body until its lower edge just touches the water—where the force of cohesion of the body to the water may or may not hold it, as the case may be. If, however, this effect is to be prevented, we must take weights from this other scale, so as to keep the body wholly in the water. Note the amount of weights necessary for this. Then as we have had first the weight of the body—then the same weight diminished by that of an equal bulk of water, the difference between the former amount in the scales, and the new amount will give the weight of an equal bulk of water—that is, it is equal to the weight of those shot or ounces, or whatever it may have been, which we took from the other scale. We obtain thus the weights of equal bulks of water and of the given body, and dividing the latter by the former we have the specific gravity required.

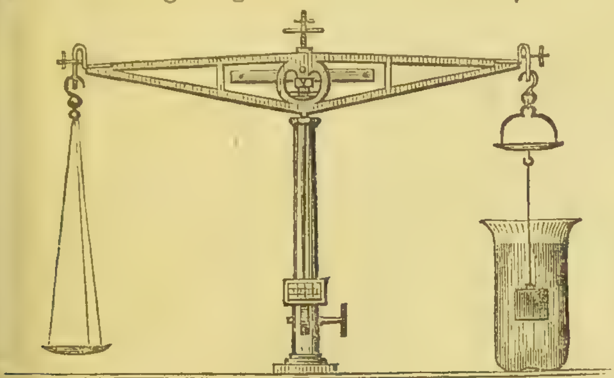
Theoretically we would require to take into account in balancing, that our weights, and the things weighed may displace different quantities of air, and so, in this case we would require to remember that the weight that we obtain in the two cases is that of a body weighed successively in air and in water. We must remember, however, also that our weights themselves are weighed in air, and even were the whole weight of a volume of air equal to that of the bodies we operate with, to be neglected, very little error would result. The bodies we operate with in obtaining specific gravities are very small; and it takes about 21,400 cubic inches of air at ordinary temperatures to weigh a pound.

When we wish to find the specific gravities of fluids, one method will be to weigh equal weights of the same substance in air and in water, and the given fluid, successively taking the one to weigh in water, and the other in the fluid.

In that case we should have the weights of equal bulks of water and of the fluid to compare, and we shall obtain the same result as before.

In using this balance it is necessary to see that the solids used be not soluble in the very least degree in any of the liquids used. If they be soluble in water, for instance, we must employ other liquids in which they are insoluble,

:d whose weight, as compared with that of ater, is sufficiently known. No solid should e employed which has loose particles adhering to , for the least motion in any liquid will rub them ', and thus make the bodies actually weighed in :e air and the fluid in reality two different bodies. 'e annex an engraving of the instrument.



Spring Balance.—This instrument is sometimes ore convenient than the ordinary balance. It n be put in one piece, and is, therefore, more rtable. It consists of a metal spring, which is ongated by weights suspended at one end of it, : shortened by weights pressing on the other. he measure of lengthening or shortening is taken s determining the weights in question. In order graduate the scale for the determination of eights, one ounce, two ounces, three ounces, &c., re respectively employed, and the elongation : shortening of the spring carefully marked. 'eights which produce the same amount of ongation or shortening are considered to be ted on by the same gravitating force. A comon instance of the spring balance is found in e letter-weigher. The instrument is also freuently employed by grocers. It is subject, howrer, to one great disadvantage. It loses its ower in heat, and its elasticity increases in cold. ow if, on each occasion of employing it, we used th the standard weights and the body to be eighed at the time, and inferred the weight of e body from the effect produced by it, being equal that produced by a given standard weight, is effect would correct itself as it does in the dinary balance; for, though we could not tell osolute weights, we could compare as usual. his method, however, has been abandoned, and e graduate a scale according to the elongations shortenings produced by standard weights, at e time and at *standard* temperatures. In fact, e measure the effect produced on the spring by rtain weights, once for all, and therefore at a xed temperature, and with definite elastic forces the spring. In heat, therefore, a body, having ss elastic force in the spring to overcome, will ush it farther down on the scale than it would t standard temperatures, and will so appear eavier than it ought. The reverse effect will tain, in excess of cold.

There is yet this other defect to which spring balances are liable—that, after much use, the spring comes to lose its power, and the same effect is produced as in the case of unusual heat. Practically, also, it is usual for the spring to be concealed, in order to keep it from injury or dust. In consequence, it is not uncommon that the balance should be put all wrong for days, by some dust gathered inside, and the error not be detected.

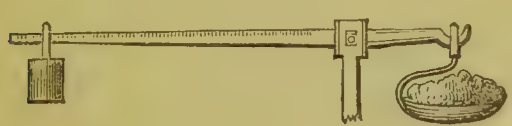
These defects preclude us from considering the spring balance generally, as much more than a convenient instrument. One advantage, however, which it has over the common balance, gives it an importance in a philosophical point of view. With the common balance, it is not a matter of any importance whether the actual force of gravity vary or continue constant at the places where it is used. For the extent of the balance itself, at any given time, gravity is always the same; but it is not the same, for example, at the equator and at Glasgow. The common balance compares the mass of a given body with that of a lb., for example, and decides it to be the same, because gravity, *at the place*, produces the same effect upon it. Remove the balance from Glasgow to the equator, and we have the same result. The common balance, therefore, assuming the forces acting at the two extremities of the beam to be equal, compares the *masses* rather than the *forces*. The spring balance measures the forces directly. A body, such as a lb., will not produce the same elongation of a spring at Glasgow and at the equator, under the same temperature. In the case of the common balance, we have the different forces of gravity acting; but in both cases they eliminate themselves, as it were. At Glasgow, the gravitation pulling at the one end of the beam neutralizes the same Glasgow gravitation at the other end; so, also, at the equator, gravitation in each case is considered twice. In the spring balance, on the other hand, it is considered only once, and a greater gravitating force, just like any other greater force, produces a greater movement of the spring. Hence the spring balance is useful in determining for us the value of the gravitating force at various localities. This gravitating force varies, since the earth is not spherical, and the mass of the earth concentrated at its centre of gravity is not equidistant from bodies at the surface. The spring balance becomes thus a measure of this ellipticity. The ordinary pendulum, however, also depending for its movements on the amount of gravitating force, measures it better. See FIGURE OF EARTH and PENDULUM.

Steel Yard.—The steel yard, or *Roman balance*, is another instrument also used frequently for convenience. It consists of an ordinary beam of iron supported on a pivot dividing it unequally. At the shorter end is a scale, in which the body

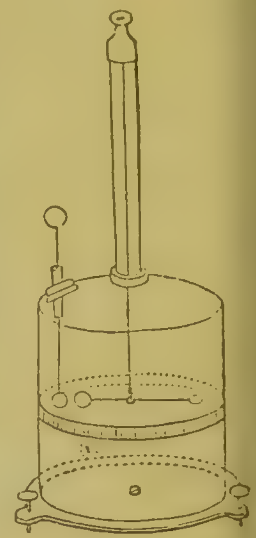
to be weighed is placed. Put there first a lb. weight, and take the same weight, for instance, and shift it along the longer arm until equilibrium results. Then put two pounds weight in the scales, and shift the weight out along the arm until equilibrium result again, and so on. Mark the points where your weight equilibrates the various standards put successively in the scales. Whenever we have to weigh a body then, we shall just shift this lb. weight along the line of the longer beam until equilibrium is produced, and then read off the scale, at the point where it is suspended.

The principle of the steel yard is just that of the common lever (see LEVER), that the weights, in order to produce equilibrium, must be exactly in the inverse ratio of the arms. It is not at all necessary that the weight which we employ should be any standard weight, such as 1 oz. or 1 lb., but it is necessary that we should always use the same weight for weighing with, which was employed when the bar was graduated off. If we lose it, our instrument can be of no more use to us, until by trial we recover it. For convenience of recovery it is better that it should be some standard, easily replaced if lost, and the selecting of that standard is just made according as the weights to be usually measured are heavier or lighter.

The steel yard is an instrument not susceptible of very great accuracy. Its only advantage is, that it enables us to employ any one weight in all our measurements. It is not employed in philosophical experiments to any extent. We annex an engraving of the common steel yard.



Torsion Balance.—An instrument by means of which we may measure very small attractive or repulsive forces. It was first employed by the inventor, Coulomb, in his electrical experiments. A very fine thread is suspended from a fixed point. A weight is added at the end of it to steady it, and prevent vertical motion as much as possible. A needle is then fastened to the thread between the top and the weight, of such length as just to follow, when it revolves, a graduated scale of degrees annexed to the instrument. When we wish then to measure the smallest forces, we make them act at the extremity of this needle, which is deflected by them from its original position, proportionally to the forces. In fact the only force which prevents the needle from moving quite freely, all round the circle, is the recoil of the thread from the thrust which this would give it. This is called the force of *torsion* (see TORSION), and as we can calculate its amount in the case of each particular thread, for a certain deflection, we can tell the amount of the attractive or repulsive forces. The needle comes to rest just when these forces and that of torsion are equal. We annex an engraving of Conlomb's torsion balance.



Balancing Wheel. See WATER WHEELS.

Ballista. The name of an ancient warlike engine, adapted to throw stones, sometimes as heavy as three hundred-weight, against the walls of a besieged city. It was nearly square-shaped. It was capable sometimes of throwing stones a distance of a quarter of a mile. The *scorpio* and *onager* were similar instruments, of which we know, however, nothing else. See also CATAPULTA.

Ballistic Pendulum. An instrument employed for the purpose of measuring the velocity of musket and cannon balls. It consists of a very strong wooden pendulum, having a thick ball of wood, plated with iron, at the bottom, against which the bullet to be experimented upon is fired. The ball ascends, pulling out a ribbon which is attached to it, so as to be capable of being drawn out with little or no force. The length of this ribbon tells us to what height the ball has risen. In fact, it will be the length of the chord of the arc which the pendulum describes in its first oscillation. If, then, we know the weight of the bullet, and the weight, form, &c., of the pendulum itself, with the position of its centre of gravity at which its weight acts, we shall be able to measure the velocity of the bullet by the quantity of motion which it is capable of communicating in this way. The process by which we arrive at the numerical result is somewhat intricate, but the reader will easily understand the principle upon which it proceeds. Whatever quantity of movement the bullet has lost, the pendulum has gained; and, if the bullet loses all its motion, the whole amount of it may be measured by the actual motion of the pendulum.

Balloon. The balloon depends upon the principle of Archimedes for its power. That principle applies to every case of bodies immersed in a fluid. According to it, every body, in such circumstances, loses just so much of its own weight as the bulk of the fluid which it displaces comes to. Suppose, for example, a stone of 1 lb. weight placed in water. Imagine, for a moment, the outline of the stone remaining as an infinitely thin shell, through which the water enters, and which it fills. Suppose this shell taken out and

weighed while filled with water, and suppose it weighed also while filled with the stone. According to the principle of Archimedes, the weight of the stone when in the water, together with the weight of the outline, as it were, of the stone filled with water, will just make up the original weight of the stone out of the water. Now, the balloon is a body immersed, like the stone, in a fluid—the air—and the same law applies to it. It loses so much of its weight as an outline of its form, so to speak, filled with common air would weigh. There is this difference, however, between the two cases:—the stone has still weight left by which it descends; the balloon, on the contrary, ascends. We shall see the point of union between the two cases more readily, if we imagine gravity—the force of the weight—as represented by a man pulling right downwards a string attached to the stone and the balloon, respectively. Suppose that another man represents the loss of weight which each experiences—the contrary weight of the equal masses of fluid, by pushing the bodies upward. In the case of the stone, the man pulling downwards is the stronger of the two, and the body descends. In the case of the balloon, the man pushing upward uses most force, and the body rises. The principle is precisely the same in both cases.

The methods of securing this preponderance of ascensive force are readily suggested. We know what it is in every case—a force, pushing upward, equal to that which draws downward a body of common air which could be contained within the outline of the balloon. We require, then, to see that the downward force be less than this. The materials of the balloon—the silk, the cords, the car—weigh more than the air they respectively displace; and, when the balloon is used for aerial voyages, the travellers weigh considerably more also. There is, then, so far a preponderance of the downward force. There is left us, however, the internal space of the balloon. Now we cannot leave this unoccupied—in which case we should have the upward force gained without any counterpoise of downward force—because, without very strong materials, the sides would be crushed in. The atmosphere, we know, presses with a force of 14¾ lbs. on every square inch. A silk bag, utterly empty, would collapse into two silk plates under this pressure, and we should thus lose our advantage as soon as we had got it. If we used stronger materials, so as to avoid this, such as iron and copper, the first preponderance which we have noted—that of the downward weight of the materials over the upward *push* of an equal quantity of air—would be very great indeed, if they were made thick enough to resist the entrance of air at every point, and we should have but little, if any advantage from our balloon. We have to *fill* it then with something which shall keep our bag distended, and so retain the space which we are anxious thus to turn to account, and which yet shall be lighter than common air. Heated air was employed for the Montgolfier balloons, hydrogen gas for the ordinary balloons made now. We have, then, the preponderance of the downward force, as far as regards materials; and of the upward, as far as regards the filled space. If the difference of specific weights of the light body and air be considerable, or of the materials and air inconsiderable, and if the proportion of space which they fill be small compared with that which the *bag* of the balloon, if we may so call it, fills, we shall have a very powerful upward force as the result of the whole.

One practical caution we should wish to repeat, because it depends upon a very important physical principle. The aeronaut who fills his balloon with hydrogen gas must not put in it as much as it can hold of the gas. If he do not—if he put in, for instance, only half what his balloon will contain—he will certainly commence the ascent with less than half the force with which he might have done so. His balloon will be squeezed down to half the size; and, according to Marriotte's law, when it reaches that, the pressures inside and out, being equal, will balance. He will only thus have half the apparently available difference of weight of hydrogen and common air. When he has ascended, however, not so quickly as he might have done, a considerable way into the atmosphere, his circumstances will have changed. The atmosphere is no longer so dense. All the weight of air beneath him, which lay on the strata of the air at the surface, does not press on the air in which he now is. It does not press then so heavily on the sides of the balloon. Now the hydrogen inside is only kept in the space of half the balloon because there is a pressure outside which balances the pressure in that case. When the pressure outside becomes $\frac{3}{4}$ of what it was at first, it will occupy no longer half the space, but $\frac{1}{2} \div \frac{3}{4}$ of the space (see MARRIOTTE'S LAW), that is, $\frac{2}{3}$ of it. When, again, the pressure becomes only half what it was, the hydrogen will occupy twice its original bulk, and fill the whole balloon. Now here lies the advantage which he has reaped from not filling his balloon. The pressure outside is now 7⅜ lbs., according to our supposition, that within, according to Marriotte's law, is the same, because, as the space of the original hydrogen increases (which is now double of what it was, the pressure which just balanced, and was therefore just equal to the atmospheric pressure) decreases in the same proportion. There is, then, no strain at all upon the canvas at the height where the atmospheric pressure is diminished by one-half, that is, about the greatest height to which Gay Lussac went. If, however, the balloon had been filled, and if it had reached this height, there would have been a pressure inwards of 7⅜ lbs., and a pressure outwards (the original pressure) of 14¾ lbs. per square inch. In all likelihood, then, the silk or canvas would burst, and the unhappy ballooner's voyage terminate. See AERONAUTICS.

Barbette. A term of fortification, denoting a sort of battery. It is a small hillock of earth, flanked by works on which cannon may be placed, which are fired over the wall, instead of through embrasures.

Barbican. A term of fortification. It marks any small outwork, like a wall—pierced with shot-holes, intended to mask a bridge or a gate, or a covered way, or any object dangerously exposed.

Barometer: etymologically, a *measurer* of *weight:* a name given to one of the most important instruments of meteorology,—its object being to measure the *weight* of the *superincumbent column of air*, and so to enable the inquirer to note its variations. In common estimation, this instrument is a *weather-glass*, prognosticating the occurrence of rain, &c. &c.—it does not, however, give any direct indication except the one now specified: the probabilities of rain, &c. are inferences only, and dependent for their degree of accuracy on the mode by which very imperfect meteorological theories have been able to connect the other phenomena of the atmosphere with its *weight*. The principle of the barometer is very simple. Suppose a tube, A B, closed at the top, and of considerable length, to be filled with mercury, or any other fluid, and then turned into the position occupied by it in the diagram, it is easy to see what must occur, if the end D is left open. It is this: the mercury will descend in the tube, and reach a level, C, determined in this wise,—the height of the column C D must be an exact counterpoise to the weight of the whole column of air resting on the open surface of the mercury at D, and so pressing the mercury or other fluid up the tube. The weight of the *column* of air may thus easily be found, if the exact specific weight of the liquid in the tube has been previously ascertained. Any liquid whose specific gravity is known will suit the purpose: water, for instance, is employed in the water barometer. On two accounts, however, mercury or quicksilver is the liquid employed in practice, viz:—*first*, its great specific gravity enables a comparatively short column, never more than thirty-one inches, to counterbalance the whole column of the atmosphere, thus confining the size of the instrument; and *secondly*—because of the high temperature of its boiling point, the elastic force of its vapour at ordinary temperatures is extremely small; so that for ordinary purposes the disturbing or reactive force of the mercurial vapour filling the space A C of the tube may be neglected. Without entering on minute details, it will be evident that the chief mechanical requisites of the barometer are implemented in such an instrument as the following:—Within a brass tube, capable of being suspended by a ring at A, or fitted by other means to be placed *vertically* with the greatest exactness, let there be a glass tube, closed at the top, and filled with mercury. The other open end of the glass tube is plunged into mercury contained in a box C C′ D D′; the surface of the mercury being seen at C C′. Attached to the brass tube is an *ivory* or *platinum* point, seen near C′; and by a screw, A′, connected with the *moveable* bottom of the box, C C′ D D′, the surface C C′ of the mercury in the box can always be brought just to touch the end of the ivory or platinum point. This platinum or ivory point is the *zero* or commencement of the scale by which the height of the mercury in the tube is measured. The upper part of the scale, as much as includes the whole possible range of the mercury, as caused by variations of atmospheric pressure, say from 25 to 32 inches—is engraven on the brass tube, to which, for minuter readings, a sliding vernier, B B B′ B″, is attached. An observation may be conducted in this wise. The lower surface, C C′, being first brought to the ivory point, the position of the upper surface is examined, as it appears through the slit E E′ in the tube. The sliding ring B B B′ B″ is then brought by an attached screw, until its plane be a tangent to the summit of the meniscus which terminates the mercurial column; a careful reading of the scale and vernier will then give the height of the column. The operation seems easy, but it must be performed with solicitude; especially as from the great specific gravity of mercury, the slightest error will cause a great error in the weight of the column deduced from it. But this process, however great the accuracy with which it may be gone through, does not suffice to enable the inquirer to state the real weight of the Air. There are *corrections* to be applied in compensation of *errors*, occasioned partly by inaccurate construction of the instrument, and partly by physical causes. The Errors in question may be classed under two heads,—those which are absolute and invariable—characteristic of the instrument under every condition; and those which vary according to the circumstances under which each observation is made. We shall discuss them separately:—I. *Absolute or Constant Errors.* One set of these absolute errors, inhering in all ordinary Barometers, and rendering them useless for delicate observation, ought to be prevented by the maker of the instrument, and the mechanician setting it up. These arise from the impurity of the mercury employed, and from the inclined position—the want of *verticality*—of the tube. If the tube be not vertical, the measured *height* is no index to the *weight* of the column; and unless the mercury be pure, and its specific gravity therefore constant, it would be equally

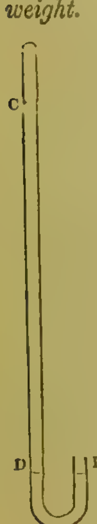


Fig. 1.

Fig. 2.

impossible to deduce *weight* from *height*. But as neither error is a necessary one, we shall consider our instrument not imperfect, through such carelessness of its artists. The second class of absolute errors cannot be avoided by the best makers, and must be *corrected* by the Observer. They consist of two items, and form what is called the *absolute equation* of each Barometer. The *first* of these items depends on a permanent physical cause—*capillarity*. On looking at any mercurial surface, within a glass vessel, any one may notice that it is *convex*—the sides being depressed. This influence of capillarity then, as exercised between glass and mercury, tends to *depress* the mercurial column; and, it does this the more, the narrower the tube. In case of a syphon barometer, such as fig. 1, the depression of both surfaces will be the same, so that this item of absolute error may be neglected; but in case of barometers like fig. 2, when the lower surface is much larger than the upper, the mercury will be permanently depressed, and the reading, as above described, will be too small. A certain quantity must thus be added to every reading; and it is required to determine that quantity. The researches of Gay Lussac and Schleiermacher, and the analysis of Poisson, have enabled calculators to construct tables for this error. It does not depend on the diameter of the column or the inner diameter of the tube alone, but also, on the dimensions of the *meniscus in a vacuum;* so that the table has *two* variables as its index. The best table is by Delcros. The depression due to capillarity being thus ascertained, it is sometimes mechanically corrected for, by giving the *scale* a corresponding *index error*, or placing the zero point *lower than it should be.* But this correction is rarely perfect, inasmuch as it presumes the non-existence of the second item of these absolute errors,—the error of the zero point itself. Rarely, indeed, can a scale be produced absolutely free from its own index error; so that instead of an alteration of the zero point sufficing to correct the depression of capillarity, the position of that point usually involves an error itself. There is no good or accessible mode of correction in the case of the mass of barometers save one, viz., a most careful comparison with some type-instrument, on which all the solicitudes of art and science have been expended. By such comparison the required absolute equation may be obtained. II. The errors, which vary with every act of observation, are also twofold. *First*, however accurately the scale may correspond to its zero point, when constructed, it is plain that it must become erroneous should the brass tube on which it is engraved *vary in length.* But that tube expands and contracts according to the rise or fall of its temperature; and the scale can never be accurate therefore, unless when the tube is of the exact temperature it had when the scale was first cut. To remedy this source of error, the rod or tube containing the scale, was, at one time, constructed of materials the least of all liable to expansion or contraction—such as *wood:* now however, a metallic tube, whose rate of expansion has been determined, is adapted for all good instruments, on the principle that it is preferable to deal with a large though ascertained error, than with a small and indefinite one. It is necessary therefore to ascertain the temperature of the *scale-tube* at every observation, and to add to, or subtract from, the height as read on the scale, a quantity due to the expansion or contraction of the tube, taking as a basis its natural temperature and height. This necessity of determining the temperature of the tube, requires that a thermometer, T T′, be attached to the tube; and that this thermometer be read off quickly at the commencement of the process of observing.—The *second* cause of error is due to variation of temperature also; but to its effects on the *mercurial column.* The object of the instrument being to give the *weight* of the air by observation of the *height* of the column; it is evidently implied that the mercury remain of a *constant specific gravity;* for then alone could similar *heights* represent corresponding *weights.* Now the specific gravity of mercury is not constant, if its temperature varies: this liquid, expanding like all others, and becoming of inferior specific gravity as its temperature increases. The temperature of the mercury must be ascertained therefore, as well as the temperature of the tube: and in many of our best Barometers this is done by aid of a second thermometer whose ball is plunged amid the mercury. Often, however, it is assumed that the temperature of the mercury corresponds, or very nearly corresponds, with the temperature of the metallic tube; and that a single observation of the thermometer will suffice. The amount of correction for these two errors grouped together, is obtained in practice from suitable tables, whose indices or variables are the observed temperatures. Such the needful corrections. but errors in the result will still remain. The process of observing the Barometer is a very nice one; and only the greatest care and large practice will secure the observer against mistakes seriously interfering with the usefulness of his results, in those more delicate inquiries on which science is now entering. Hence, as we shall see below, the very high importance of *self-registering Barometers.*—Mr. Newman of London has long enjoyed deserved reputation for the structure of Barometers. We gladly notice also the excellence of the Barometers of Fortin, as now produced by M. Ernst of Paris.

Barometer, Aneroid.—A very beautiful, portable, and accurate instrument, recently invented by M. Vidi. It was introduced to the British Association in 1848, by Professor Lloyd of Dublin. The face of this Barometer, presented in fig. 1, is four or five inches in diameter; the barometric heights being indicated by the hand. Behind the face is a brass box, about $2\frac{1}{2}$ or 3 inches deep, containing the apparatus, on which the weight of the

atmosphere acts. The apparatus itself is a little complex, but its principle will be readily under-

Fig. 1.

stood; by aid of figs. 2 and 3, which are sections of the important parts of it. D D, fig. 2, is a circular

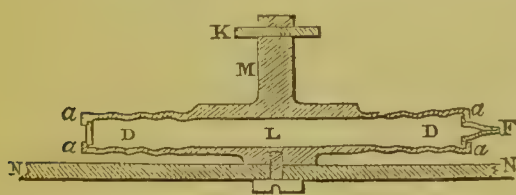


Fig. 2.

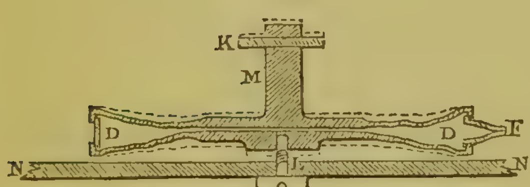
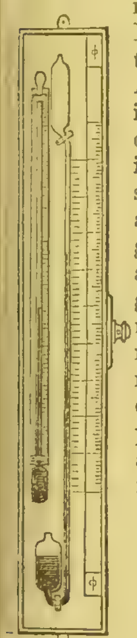


Fig. 3.

metallic case called the vacuum chamber, the sides of which are thin and corrugated, so that their elasticity is great, and therefore the readiness with which they yield before variations of atmospheric pressure. This chamber may be *exhausted* wholly or partly through a tube, F. In the state of exhaustion it would take the form of fig. 3. When prepared for use, however, the exhaustion is not complete; and the corrugated diaphragms take the position of the dotted lines in fig. 3, leaving between them a certain free interval along the whole interior of the chamber. A quantity of gas is introduced after exhaustion, by means of the variation of whose elasticity, at varying temperatures, compensation is effected for the changing capacity of the case. The case or chest being 2·5 inches in diameter, the pressure of the atmosphere upon its sides or diaphragm would be about 73 lbs. were exhaustion perfect: in the actual instrument it is not more than 44 lbs. It is easy to see that, on any sensible variation of atmospheric pressure, the two sides of this metallic chamber, will either recede from each other or approach, according as that pressure diminishes or augments: and that in consequence of such receding or approach, the free stalk or rod, M, will obtain a slight motion *in* or *out*, at right angles to the face of the Aneroid. This motion is, in the first place, changed by ingenious contrivances from a vertical motion to one parallel to the face of the dial; and this last is again changed into a rotatory one—that moving the index hand—by the application of a watch chain to a small cylinder or drum. This process of change is also made to *augment* or multiply the original very slight motion, by aid from levers; an effort so well accomplished that when the corrugated surface moves through only $\frac{1}{120}$th of an inch, the index hand on the face turns over a space of three inches. The only correction required for the aneroid is a slight one for temperature, detected experimentally thus:—Observe carefully its indication at any moment in the external air; remove it immediately before a fire, and heat it until the thermometer on the dial shall reach 100°; then notice the variation of the hand: this variation, divided by the number of degrees through which the thermometer has moved, will give the correction, whether in defect or excess, to be applied for each degree of change of temperature. The extreme portability of this little instrument, and its comparative freedom from risk of fracture, give it immense advantage over the mercurial barometer in all cases—such as in measuring altitudes—where its transport is necessary; and certainly it has well stood the strongest tests as to its accuracy. Mr. Lloyd states, for instance, that he had placed one under the receiver of an air pump, and found that its indications corresponded with those of the mercurial gauge to less than 0·01 of an inch; and, within ordinary variations of atmospheric pressure, the coincidence is very remarkable:—The sum of the heights of 109 observations by the aneroid, gave in one set of experiments 3239·712 inches, the corresponding sum with the mercurial barometer being 3239·44. The Aneroid was thus in excess only 0·272 of an inch, which, divided by 109, gives for the average error, or rather *discrepancy*, of one observation, the very small fraction 0·00249 of an inch. Modifications and different forms of the Aneroid have already been proposed: among which are those named *Metallic Barometers*. The principle is the same.

Barometer, *Siphon*, *Simpiesometer*, *Portable*.—Various forms of the Barometer have been proposed. One, the *siphon*, consisting mainly of a bent tube (fig. 1, BAROMETER), without a cistern,

in fig. 2. The altitude of the column is some-nes measured at the *upper* surface and sometimes the *lower*—the changes of position of the lower rface evidently corresponding with changes in e position of the upper one. In this latter case, rrection for the temperature of the mercury may : dispensed with, as it is the *weight*, not the *ight*, of the long column which is directly easured:—the position of the lower surface ould be the same, although the greater part of e upper column were not mercury, but some her liquid. These *Siphons*, however, are not vourites with scientific Observers. They are bject, especially, to disturbance from an in-quality of capillarity, or of the *meniscus*, hen formed in vacuo and in the air. They are so fragile; and altogether the construction is ferior. — Another modification was proposed many years ago by Mr. Adie of Edinburgh, and is of much value, through the extent of its range. As represented in the accompanying woodcut, it consists of a portion of permanently elastic gas, enclosed in the upper part of a tube, and separated from the external air by a fluid, which neither acts on that gas, nor is acted on by the external air. The gas employed is hydrogen; and, in the cistern, oil of almonds, coloured red by alkanet root. As the pressure of the atmosphere varies, the enclosed hydrogen expands or contracts, by proportional but large quantities; and the liquid, accordingly, either rises or falls in the tube, to which a scale is attached, through large spaces. The sensitiveness of this instrument, and its extensive scale, makes it a deserved favourite at sea; but, for delicate meteorological purposes, it is quite inferior to the mercurial barometer, in



onsequence, chiefly, of absorption of hydrogen by ie oil. To enable the observer to correct for mperature, a thermometer is attached. *Portable arometers*, chiefly of the siphon form, are in-ruments made with especial view to safe and asy transport. Very ingenious and excellent arometers of this kind have long been made at aris and Berlin: but they are now superseded y the Aneroid.

Water Barometers, &c.—These are instruments uite like the mercurial: the liquid employed eing *water*, &c. instead of mercury.

Barometer, Self-Registering.—There are two istinct functions of what are called self-gistering instruments:—The *first*, to present ontinuously the course or flow of a changing henomenon,—of which, without their assistance, e can learn the condition only at a few and eparate moments of time. The *second*, to enable us to avoid, by some process of *mechanical recording*, the errors inherent in every act of observation. To accomplish the first object, self-registering Barometers have been offered in various forms; but, in general, we have lost in exactness as much or more than we gained through the continuity or frequency of the record. No instrument of this kind has been produced sufficiently free from irregularities arising out of *friction* and other mechanical obstacles. Of all those with which we are acquainted, the Bryson's of Edinburgh is undoubtedly the best; the instrument marking its condition every hour on a cylinder turning regularly round, through means of its attachment to a clock. But this instrument leaves untouched the errors of observation; nay, the mode by which it provides for the subsequent measurement by the observer of the altitude or value of the marks it attains, is the weak part of it. This *second* source of error can be got rid of only in one way, by the aid of *Photography*. It is not too much to allege that the happy application of this art will ere long supersede personal observation in such cases altogether; while at the same time it affords the means of recording continuously the changes of any phenomenon in a state of *flux*, however various these may be. We do not intend to describe in this place the mechanics of this application of Photography, because these will certainly be much simplified. Suffice it, that the efficiency of the application alike to Barometric, Thermometric, Hygrometric, and Magnetic changes has been amply verified at our great National Observatory of Greenwich. The instruments in action there, were constructed by Mr. Brooke; and it is understood, that their eminent value is acknowledged, and the opinion we have just expressed fully concurred in by the Astronomer Royal, Mr. Airy. See Article PHOTOGRAHY, REGISTRATION BY, in Appendix.

Barometer, Repeating. See Appendix.

Base. In geometry, the base of a figure means properly its lowest line, if it be a plane figure, or its lowest surface if it be a solid. In trigonometrical operations, the base is a line carefully measured between two points readily accessible, from which, by measuring *angles* alone afterwards, we may obtain the length of lines not observed. Upon the accuracy of the measurement of the base depends therefore, the value of the whole series of operations. In consequence, the greatest precautions are taken in surveys, not to neglect changes of length from changes of temperature in the measuring chain—differences of level—the spherical figure of the earth's surface, and the like. What is called the *base line*, in measuring the length of a degree of latitude (see LATITUDE), is the length marked off between the points, the inclination between the verticals at which is to be answered.

Bastion. A term of fortification, applied to a work in the shape of a pentagon inserted in the

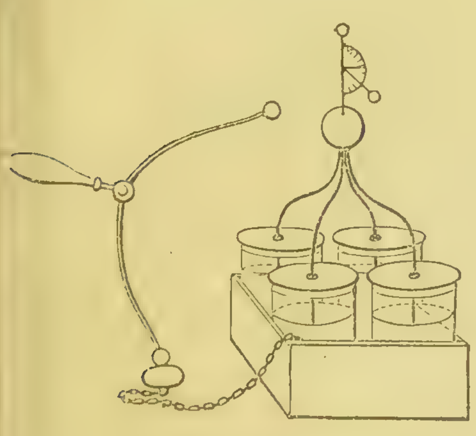
wall of a fortress. It consists of two *faces*, which form a salient angle to the spectator outside; of two *flanks*, which connect the bastion with the *curtains;* and of a *gorge*, which separates their extremities, and by which entrance is to be obtained. The bastion usually consists of a mass of earth, surmounted with turf, bricks, or stones. It was not used till about the beginning of the sixteenth century.

Battery. Several guns playing at the same place, under cover or otherwise, are called *a battery*. The full description of the various kinds of batteries will be found in every work on *Gunnery* and *Artillery*.

Battery, Electric.—Without entering in this place extensively into the general subject, the reader may be reminded that, in the absence of any positive knowledge of the intimate nature of electricity, it has been found convenient to suppose that the phenomena are produced by two fluids, which have a tendency to combine and neutralize each other's effects, or even altogether to disguise the ordinary evidence of their existence. In this state of union, the substance with which they are associated is said to be non-electrified. They are called the vitreous and resinous electricities, this mode of expression being now generally preferred to that of Franklin, who endeavoured to explain the phenomena by the supposition of only one fluid, which, when uniformly distributed, constituted the common or unelectrified state of matter. When in excess, as on a piece of glass that has been rubbed, the body was said to be positively, or when in defect, as in sealing wax after friction, negatively electrified. These names are respectively synonymous with vitreous and resinous in the first-mentioned hypothesis. When the prime conductor of an electrical machine is of a large size, a very considerable amount of vitreous electricity can be accumulated on it; but it has been found that, by the employment of the principle of what is called electric induction, a vastly greater quantity can be accumulated in a comparatively small space, and in a state more capable of producing many of the effects of this remarkable agent. Such is the purpose of the electric or Leyden jar and battery. The principle of induction may be stated thus:—If a good conductor be interposed between an electrified and an unelectrified body, the accumulated electricity (say the vitreous) attracts resinous electricity from the other body through the conductor, and the ordinary unexcited state is restored, or the body is said to be discharged. If, however, the interposed substance be not a conductor, then discharge cannot occur, but the vitreous fluid in the electrified body attracts the resinous fluid naturally in the neutral substance, and *tends* to draw it through the non-conductor, while at the same time it repels the vitreous or fluid similar to itself, this being a fundamental law of electric action. If now a path be opened for the escape of the repelled vitreous fluid, as by presenting to it a metallic rod or other good conductor, it passes off, and we have the remaining resinous fluid and the communicated vitreous acting on each other by induction across the substance of the non-conductor, disguising each other's presence, and strongly tending, by their mutual attraction, to effect a junction as soon as a conducting channel shall be afforded. In this species of action, the mutual repulsion of the particles of each of the fluids is overcome, by the attractive power of the opposite fluid acting across the substance of the non-conductor, and thus a vastly greater accumulation can take place than the self-repulsive power of each species of fluid, for its own particles, could possibly permit, were it not for this artifice of causing each fluid, by its proximity, to subdue, so to speak, the other. In this state the non-conducting substance is said to be charged with disguised electricity, and the instant a conducting circuit is established, the two species join, and the discharge is effected. Sealing wax, gum lac, gutta percha, or glass, or indeed any tolerably perfect non-conductor of electricity, may be used. A thin plate is the most effective, and in order that the different points of the surfaces may act simultaneously, a film of conducting substance must be spread partially over both sides, leaving, however, sufficient space near the borders to preserve the insulation. A plate of common window glass, coated with tinfoil on each surface to within an inch of the edge, makes an excellent apparatus for the accumulation of electricity in the manner above referred to. Being rendered carefully clean and dry, it is charged by holding it by one of the corners, bringing the tinfoil of one surface in contact with the prime conductor of the electric machine in action, the finger or a wire being kept in contact with the foil of the opposite surface, to allow of the escape of the repelled vitreous fluid. This simple and effective arrangement affords a ready means of dispelling the popular fallacy regarding the Leyden jar, viz., that the form of a jar is necessary, in order that the electric fluid may be retained in it in opposition to its tendency to flow out like water. Although it is true that any quantity of electricity might be collected on a sheet of glass arranged as indicated, by sufficiently increasing its size, still such an apparatus would be cumbrous and liable to accident. It is therefore found that the form of a jar is more convenient, and glass is the substance usually preferred, the thickness being from $\frac{1}{16}$th to $\frac{1}{8}$th of an inch. A greater thickness than this impedes the action. Each surface has tinfoil smoothly pasted on it to within about two inches of the mouth, a cork or dry wooden lid is inserted, through which a metallic rod passes, terminated at the top by a smooth ball of half-an-inch diameter, and ending below in a chain which reaches to the inner coating of tinfoil on the bottom. The ball thus represents the inner surface, and the outer tinfoil the exterior. Be-

een these the discharge tends to take place. hen a great accumulation is required, instead using one very large and therefore thick and wieldy jar, a combination of a number of aller jars is preferred. To such a collection name of Electric Battery is given. Each jar is structed as already detailed, and they are then nbined by placing the outside coatings in me-lic contact with each other, and by connecting the ole of the interiors into one, joining all the balls means of chains or brass rods, or more simply haps by binding the whole of the rods as they end from the interior of the jars, so that they y meet—and on the joined extremities placing ball, which thus represents the whole of the erior surfaces. A battery composed of six s, each exposing two square feet, is sufficient the exhibition of most of the effects of accu-ilated electricity. When powerfully charged, ich it will be by about a hundred turns of a od machine, it should be handled with caution, e being taken that no part of the body be ought near the ball. If this be attended to, exterior surface may be freely touched. The ief secret in the successful management of this, indeed of most pieces of electrical apparatus, that it be rendered perfectly clean and dry; leed it is generally essential that each jar should taken separately and rendered warm by being ld with a turning motion for a few seconds er a fire or a good flame, just before being ed. It is proper also, to prevent disappoint-ent (which is so frequent), that a pithball elec-meter should be attached to the battery, to licate, by the amount of its divergence, the ogress of the charge.

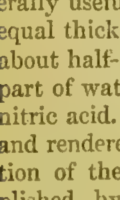
The annexed cut represents a battery, with



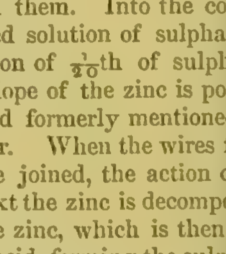
electrometer indicating the charge, and jointed discharger used for passing the charge rough the substance of an egg, which lie on e chain from the outer surface. One ball the discharger is placed on its upper sur-e, while the other is brought up to touch the ll from the internal coatings, when the spark sses, and the discharge is instantly completed.

Battery, Galvanic or Voltaic.—The apparatus to which this name is given must be regarded as one of the most curious and remarkable of human inventions. The exhibition of its effects scarce fails in exciting, in the thoughtful observer, admiration of the genius which has constructed, from the apparently inert and lifeless matter of the earth, combinations of power so strange and subtle, an instrument so potent in the laboratory of the philosopher, and now so useful in the processes of art. Of the theory of the Galvanic Battery little can here be said, nor indeed could it be of value, as all that is known regarding it rests on hypotheses in themselves full of doubt. Without entering into speculations as to the electric relations of the atoms of matter, it may be stated, as tolerably well ascertained, that there is no case in which chemical change occurs among the constituents of any substance, without a disturbance of their electrical state. In some, nay in most cases, this disturbance would altogether escape notice, were it not for certain precautions taken expressly for its observance. For instance, say that we dip a piece of metal, such as brass, into diluted sulphuric acid, in a glass vessel; the metal would be dissolved, and we should not be sensible of any electrical change. If, however, the containing vessel were tin or iron instead of glass, then, on bringing the edge of the dissolving brass in contact with the edge of the vessel, a spark of light would instantly give notice of the passing energy. To this the name of Galvanic Action has been given, and the agent is called Galvanic or Voltaic Electricity. That it is common electricity in a modified form has been proved, and the difference between the two has been compared to the difference between a mountain rivulet, or rushing cascade, rapid and powerful though small in quantity, while the other, the galvanic current, resembles the rolling waters of a broad and deep, though slowly moving river, the frictional electricity excelling in *tension*, while the voltaic current far exceeds it in *quantity*. In the absence, hitherto, of certainty as to the intimate nature of the actions in question, such phrases as Galvanic Current, Positive and Negative Pole or Electrode, &c., must be conventionally used till a more correct nomenclature shall emerge from advancing knowledge. It will be sufficient here to state as an ultimate fact, that when two different substances, together and without contact, are immersed in a substance, generally a liquid, which chemically acts on both, the two are thrown into opposite electrical states; that which is most energetically acted on becoming negative or resinous, while the other becomes, with reference to it, vitreous: and further, that these actions are most powerfully observed when the chemical action is one of oxidation, and both substances are good conductors. Though it is important to keep in view this generality in these actions, still it is known that metals undergoing solution in acids are most favourable for the energetic exhibition of the galvanic phenomena. In

the choice of the pair of metals to be put together to form the combination, it must be kept in view that if both are good conductors, then the more energetically and rapidly the one is acted on by the solution, the greater is the evolution of electricity, this being called the generating plate, and the more the other resists the action of the liquid, the greater also is the energy of the combination. The one least acted on is called the conducting plate. . The combination of two such plates is called a galvanic element, the most common being copper and zinc, immersed in a solution of one part of water, from $\frac{1}{40}$th to $\frac{1}{18}$th of sulphuric acid, according to the energy required, and $\frac{1}{80}$th of nitric acid. When such a pair is composed of plates exposing many square feet of surface, great energy is evolved. A wire connected with the copper is called the *positive pole* or *electrode*, while the same with reference to the zinc is called the *negative* pole or electrode. Any substance to be subjected to the action is placed between the poles. The current is said *to pass*. Metallic wires are heated to redness or dissipated in vapour; vivid sparks are seen; pieces of charcoal are heated to intense whiteness, producing what has been somewhat erroneously called the electric light. Powerful magnetic effects are likewise induced in coils of wire. But some of the most important actions of the current cannot be evolved by this apparatus of a single pair, however large. It is deficient in tension, and can only traverse very good conductors. To give impetus or force to the current, a combination of single galvanic elements into a battery is necessary. Before referring to the mode in which such combinations are formed, it may be as well to attend a little more minutely to the different kinds of single elements which are used. They may be divided into those of one liquid and those of two liquids. Of the first is that, the most common and generally useful, of a zinc and copper plate of about equal thickness, and separated by an interval of about half-an-inch, the exciting liquid being one part of water, $\frac{1}{16}$th of oil of vitriol, and $\frac{1}{40}$th of nitric acid. This arrangement is much improved and rendered more economical, by the amalgamation of the zinc plate, which is readily accomplished by rubbing its surface with metallic mercury, while it is moistened with one part of sulphuric acid to eight of water. By this means, first suggested by Mr. Kemp of Edinburgh, the surface of the zinc plate is kept bright and free



from oxide, while its substance is only wasted down by the acid during the time that the current is actually being used, all waste in the intervals being thus avoided. The modification usually called Smee's Battery consists of an amalgamated plate of zinc placed within half-an-inch, one on each side, of a thin silver conducting plate on which platinum, in a fine state of division, has been precipitated. The nitric acid is omitted in the exciting liquid. The superiority in this case is supposed to consist chiefly in the freedom with which the hydrogen escapes from the platinised surface of the silver, as contrasted with the manner in which it clings to the plates of the copper battery. In each of these cases of single liquid elements, the evolved current, though energetic for the first few minutes after the fresh liquid is poured on the clean metals, soon begins to diminish, and in general, in the course of fifteen or twenty minutes, it is reduced to a mere fraction of its first force, even though the acid in the solution should be nearly of its original strength. One main cause of this has been the deposition of oxide of zinc on the conducting plate, and the evolution of the hydrogen has, during the whole process, acted as an opposing process, or, at least, loss has been occasioned by the neglect of the opportunity which the hydrogen might afford of causing a deoxidating process on the conducting plate, thus rendering it more opposed to the generating plate, where oxidation is proceeding. To keep the zinc liquid from touching the conducting plate, to substitute for it on this latter plate a liquid capable of undergoing deoxidation by the hydrogen, and at the same time to allow such contact of these two liquids with each other as to admit of the current passing, are the objects to be attained. Such are the ends served by the constant battery of Prof. Daniell. A rod or cylinder of amalgamated zinc is introduced into a bag made of an ox gullet or bladder, or even of closely woven cloth, or, failing these, unglazed porcelain, in the form of a cylinder closed at bottom. The zinc cylinder &c., enclosed in its envelope, is immersed in a cylindrical vessel of copper, so much larger as to allow a space of half-an-inch clear all round between them. Into the copper vessel is put a saturated solution of sulphate of copper, with the addition of $\frac{1}{20}$th of sulphuric acid, and into the envelope of the zinc is poured the usual exciting liquid formerly mentioned, of sulphuric acid and water. When the wires from the copper and zinc are joined, the action commences. The water next the zinc is decomposed. Its oxygen oxidates the zinc, which is then dissolved by the sulphuric acid, forming the sulphate of zinc, which remains in solution, and is prevented by the diaphragm or envelope from touching and contaminating the copper, while its hydrogen passes through the moist envelope, and, coming in contact with the sulphate of copper, decomposes it, reducing the metallic copper on the surface of the copper cylinder. The sulphuric acid passes through to attack



e zinc, affording a renewal of acid for ntinuing the action. This apparatus, when operly constructed, keeps in an energetic ate for hours, and is hence called con-ant. Subjoined is a drawing of one of its rms.

Each element in Grove's Battery is constructed a similar principle. In this case platinum is ed as the conducting plate. It is, from its ex-nsive nature, used only in thin sheets, and is aced in strong nitric acid, in the interior of thin flat trough of unglazed porcelain, the ganic diaphragm, though otherwise preferable, ing in this case inadmissible. One amalgamated nc plate may be placed on each side of the rcelain trough, and the whole placed in a vessel any convenient size, containing the usual excit-g solution of sulphuric acid and water. In this se the hydrogen deoxidates the nitric acid d forms water, while the nitrous acid so pro-uced remains dissolved. After a time, however, begins to be evolved into the atmosphere, and last the nitric acid fairly enters into ebullition. his combination is of great energy and con-ancy; it is, however, in its first construction, stly, and is liable to accident. If cylinders of rbon, the best being that left by sublimation the retorts of coal gas works, be substituted r the platinum, we have the element usually own by the name of Bunsen's. Square prisms the coke can easily be cut by a marble cutter, d answer well. A piece of copper wire is wound und their upper part as the conductor, and they e inserted into the nitric acid. Great care is cessary in washing them after each time of sing, otherwise the copper is oxidated, and the rrent ceases to pass. A battery of this kind ay be constructed for £2, which, for equal effi-ency in platinum, would cost £30.

Another excellent combination of two liquids that of Callan, or the Maynooth element, as it as been called. The conducting plate is made the form of a cylindrical vessel, and is of cast on. The amalgamated zinc cylinder is placed a porous cell an inch larger than itself, for ntaining the sulphuric acid and water, one of id to eight of water. This is then placed in e cast iron cell, which is so large as to leave out half-an-inch all round of space, for con-ining the second liquid, which is composed of rong sulphuric acid, and a drachm of nitrate of otash to the ounce of acid.

Such are the chief modifications of the galvanic lement, and while undoubtedly that of Grove is e most powerful for its bulk, that of Callan, for s energy, is the most economical.

All the different species are easily made into atteries, by the combination of a greater or less umber of single elements into one arrangement, which is done by connecting the generating plate f one element with the conducting plate of the ext, and so on. For the communication of what is called the shock, to the human body, from 60 to 100 pairs of the single liquid copper and zinc combination, each plate being three or four inches square, are necessary. This would also be sufficient to exhibit most of the effects of chemical decomposition, and the heating power of the current, on thin metallic wires and leaves, as also for the feeble production of the electric light. About a dozen cells of Grove's two liquid combinations of a similar size would produce the same effects, and three dozen elements of Callan, each exposing half a square foot of zinc, is suffi-cient to produce a tolerably brilliant light when transmitted through points composed of the gas coke, formerly mentioned, and brought to touch each other as poles.

The Galvanic Battery, as is well known, is now an instrument of great practical use in the Electric Telegraph. For this purpose the form most gene-rally preferred is the simple copper and zinc plate, each about three inches square, immersed in a cell of earthenware or gutta percha containing river sand moistened by the usual diluted sul-phuric acid. This arrangement is called the sand battery. It is steady in its action, and, once ar-ranged, keeps in force about a week without re-quiring the acid to be renewed on the sand. In the offices of the Edinburgh and Glasgow Rail-way, a battery of about 100 such plates is used for the transmission of signals from one city to the other, while in the arrangements of the Elec-tric Telegraph Company, batteries of about from 1,000 to 2,000 such plates are used, owing to the great distances over which the signals must be sent. In this respect, however, much depends on the state of the weather and insulating condi-tion of the supporting posts for the wires, and also on the diameter of the wires themselves. On this curious and interesting subject, however, the article TELEGRAPH is referred to.

Since the prospect of a greatly extended use of the Galvanic Battery as an illuminating agent has presented itself, a view of the process depend-ing on the generality of the connection of the galvanic excitement as an attendant on chemical action has led to important results. It has been proposed to employ only such substances for bat-teries as will produce, as a residue, materials of commercial value. Hopes are entertained that, during the preparation of pigments, and in many otherwise necessary processes of chemical manu-factures, by proper arrangements vast amounts of electricity now uselessly allowed to waste will be evolved, in a form fit for the production of light, or the development of other actions of value. The mere mention of such effects suggests sources of power for the benefit of mankind which cannot be contemplated with indifference. Strange that such results should have flowed from the convul-sion of a decapitated frog!!

Bear, Great and Little. Two constella-tions in the Northern Hemisphere. See URSA MAJOR and MINOR.

Bell. A musical instrument in which the

vibrations of the metals, when struck, are the direct cause of the sounds.—(See ACOUSTICS) We can easily understand, according to the laws of acoustics, how bells can be constructed, from which the same impacts will produce different notes. Upon this depends the machinery of what are called musical bells, which play at certain hours of the day. The material of which bells were originally composed was in all likelihood, merely cast-iron. That now used is a compound of 80 parts of copper, and 20 of tin. This is the theoretical proportion; and Indian gongs are made exactly in accordance with it. In ordinary bells the proportion of copper remains the same, but some lead and zinc is substituted for part of the tin. This alloy is very remarkable for its great elasticity, and therefore its great capacity of sound.

Bell, Diving. It is frequently desirable to raise objects from the beds of rivers or lakes, or from the sea-bottom; *e. g.* the processes of fishing for pearls, sponges, corals, and other sub-marine products. Often again, it is needful, as in laying the foundations of piers, or in removing sunken rocks from a channel, that we should possess the power of remaining under water for a considerable period. Man does not possess this power naturally, so that he must supply it by suitable contrivances. The longest time during which a practised diver can remain under water is about two minutes. Ordinary persons are not able to support such a condition for more than half a minute. Those half savage tribes that are accustomed to water from their infancy, seem to become semi-amphibious, and can support immersion for a much longer time. The inhabitants of the South Sea Islands afford a good proof of this. A most extraordinary instance is related, on the authority of Kircher. In the time of Frederic, King of Sicily (about the commencement of the 14th century), lived a celebrated diver, by name Nicholas. He was surnamed, for his powers, Pesce (a fish). He had from his infancy been used to the sea as a fisher for corals and oysters. He came to be so accustomed to it, that he had been known frequently to spend five days in the waves, catching fish and eating them raw, for his subsistence. He was in the habit of carrying letters frequently between Calabria and Sicily (a very coarse passage, by the way). His hands and feet grew ultimately webbed, like the feet of a *goose*, and he became capable of inhaling, at one breath, enough of air to serve him for a whole day! His very glorious career was unfortunately cut short by the inconsiderate curiosity of the king. His Majesty sent for him to the palace; and when Nicholas had been found—from his unusual mode of living, this was not so easily accomplished—he came. Frederic desired him to explore the bottom of Charybdis, which, though unwilling, he ultimately undertook, upon the renewed solicitation of the King, and his flinging a golden cup into the whirlpool, which was to be the diver's own, if he could find it. He dived, and after three quarters of an hour, rose with the cup. He gave an account of the whirlpool as being lined with jagged rocks, to which clung a strange polypous fish, that stretched out its fibres in the most frightful way to catch him. Frederic, not perfectly satisfied with the account, requested Nicholas to re-descend. Nicholas allowed himself, by the prospect of a still larger cup, to be over-persuaded —dived, and perished. Every reader of German poetry will remember Schiller's exquisite ballad, the Diver, which founds on this legend.

There are two prime requisites in an apparatus for diving—the constant supply of air, and the protection of the body of the diver from too great pressure.

The first method adopted was the very simple one of letting down a heavily weighted bell vertically into the water—the diver sitting in the interior. As it descended, the air got overpressed and the water rose in the bell, but never to the top. The man sat in the top, above the water mark, and if the bell was large, could so remain for about an hour. The air became gradually poisonous by his continued respiration, and he had to be taken up.

Several evident defects occur in this instrument. The man is always kept by it, at some length from the ground. He goes through a process of inspiration, of air constantly deteriorating; and several similar objections might be pointed out. Dr. Halley's diving-bell was invented to remedy those evils. It was of wood coated with lead, and having at the top a strong glass window, by which light might be admitted to the diver below. In order to supply air, a barrel was taken with an open hole in the bottom, and a weighted hose, hanging by and fitting into a hole at the top—so weighted that it would naturally hang below the bottom of the barrel. The barrel is let down vertically by weights, and the hose is caught by one of the men in the bell, who lift it up and let the air escape from the barrel into the bell, sending it up when empty. A cock lets off the carbonic acid formed by the breathing of the diver, when the air in the bell gets filled with it. The bell is thus kept always filled with air; and the air kept constantly pure.

Two practical difficulties yet remained. When the bell descended, on a rough surface, it was apt to be caught on some rock and tilted over from its vertical position—the air escaping, and the divers perishing; and again, the whole management of the bell depends upon the people above, so that if the rope was broken, or rubbed against a stone, the same fate awaited them. To obviate the first difficulty, Spalding's diving-bell has a heavy weight attached to its middle by a rope which can be raised or let down as convenient: it is let down the moment that the bell touches a rough spot; and then, resting on the ground, it relieves the

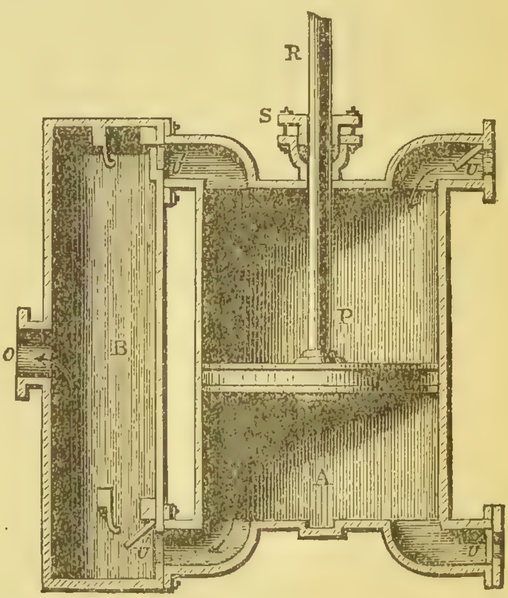
l from its weight, and enables it to rise up n the obstacle. To overcome the latter diffi-ty, the bell consists of two cavities instead of , the first or upper one having lateral holes which superfluous air may escape, and when bell goes down, this cavity is filled, by the ter pushing out the air. When the diver hes to ascend, he opens a small screw in the er chamber, which, admitting air, pushes out water from the upper chamber, so lightens bell. and makes it rise. Triewald, a Swedish gineer, introduced further improvements. At bottom of the sea, the light is scanty, and lley's contrivance of a strong glass window in top, or Spalding's, of one on the sides, did not e so much as was occasionally desirable. ewald made his bell of copper, tinned over in inside, so as to give a very strong reflecting ver, further increasing this by placing three ong convex lenses near the top of the bell. He o added a set of chains, suspending a plat-rm, so that, when not at depths rendering pressure inconvenient, the diver might stand on it with his head just above the water. He ained in this way the air purer than he could, sitting quite dry. Triewald arranged also, a l of tubes inside, by which, even if sitting, diver might breathe the air nearest the bottom the bell.

Halley noticed a very curious and interesting enomenon in his diving bell. While holding hand in the light coming through the win-w at the top, he found the back of it appear *red*, ile the palm of his hand, held downwards, ing the water, and illuminated chiefly by the ht reflected from it, was *green*. The explana-n was given by Newton. The sea-water nsmits the red rays of the spectrum most dily, and reflects most copiously the violet.

Bellona. One of the Asteroids. For *ele-nts*, &c., see ASTEROIDS.

Bellows. When we wish to direct a stream air against any object, an instrument is used led the Bellows. It is of various constructions, t the principle in all cases is very much the ne. The reader will readily understand it by ing a pair of common bellows in his hand, d commencing to use them. The bottom lid, plate of wood, is supplied with a valve opening ly inward, and shuts when the bellows are not use, by its own weight. Closing also the pipe the bellows, is a valve, which opens outward ly. When, then, he pulls asunder the plates the bellows, the air attempts at both places enter, in order to supply the empty space thus ade. The valve at the mouth, opening out-ard only, will be shut by this rush of air; the lve in the bottom plate, opening inward, will ad-it it. When enough air has been admitted, he ueezes the plates again together. The air seeks avoid the compression by escaping through e valve, and the rush of air closes the bottom lve, which does not open outward, and opens the valve at the nozzle of the bellows. The air which is thus taken from the ordinary atmos-phere by which the bottom valve is surrounded, is directed with considerable velocity through the nozzle to any point whether we desire to transmit it.

In using the bellows for large furnaces, it was found that the inconstant blast thus supplied was a source of considerable loss of advantage. The fire receiving the current of air burned violently for the moment (oxygen being supplied as fast as consumed), and next moment lost much of its heat. It was impossible thus to use the bellows, when it was desired to subject a body to a con-stant heat. To prevent this, two bellows were for some time employed—one of which was filling with air while the other was exhausting it. This secured the desideratum of a permanent supply of air, but it did not sufficiently secure a uniform supply—because the air emitted at one part of the compression of such bellows could not always be the same in amount with that emitted at an-other. The instrument which we engrave is per-haps the best and the simplest of those employed for the removal of this defect.



R P is a piston wrought generally by a steam-engine—where convenient by a water-wheel. U and U are the places where the air enters by pushing the valves which open inwards, but not outwards. At U′ and U′ there are valves which open outwards (communicating with the tube O), but not inwards, so as not to permit the air once collected there to drive back again. When the piston descends the valve U admits air, and the valve U′ expels air previously admitted into the under part of the cylinder. When the piston ascends, U admits air, and U′ expels that admitted in the descent. Air is thus collected in O, and emitted at a uniform pressure, and with uniform velocity, therefore, by a stop-cock or a valve in the

pipe o, leading to the object upon which the air is to be driven.

One objection to the process described cannot fail to be evident. It is this—that the cold air constantly supplied will be a constant source of cooling to the furnace. It is indispensable to the continuance of the flame that air should be supplied; and as many operations in the arts require the highest temperatures that can be reached, it became necessary to destroy this effect of the admitted air without destroying its power of supporting combustion. The method adopted—the *Hot Blast*—requires merely the heating of the air before admission into the furnace. In this way there is a constant temperature preserved, as well as a constant supply of oxygen. The Hot Blast is now almost universally adopted, as it affords this very important advantage, and that with greater economy of fuel, than, with the older forms of Bellows, was possible for the furnace. The details of apparatus, &c., will be found in works on *Metallurgy*.

Belt. See SATURN and JUPITER for an account of the planetary rings and belts.

Betelgeuse. The brightest star in the constellation Orion. It is of the first magnitude.

Binocular. See TELESCOPE and VISION.

Binomial (Technical.) A Binomial is an Algebraic quantity composed of two parts separated by the signs of addition or subtraction. The theorem for the development of binomials—the *binomial theorem* of Newton, which is engraved on his tomb, as being one of the most beautiful of his discoveries, we merely give here, without proof. A demonstration of it will be found in any good elementary work on Algebra. It is this:—

$$(a + b)^n = a^n + n \, . \, a^{n-1} b + n \, . \, \frac{n-1}{2} \, .$$

$$a^{n-2} b^2 \; \&c. \ldots\ldots \ldots\ldots n \, . \, \frac{n-1}{2} \ldots\ldots$$

$$\frac{n - \overline{m-2}}{m-1} \cdot a^{n-\overline{m-1}} b^{m-1}., \; \&c.$$

Bissextile. The natural measure of time in small portions is the interval between two successive returns of the sun to the meridian. A measure no less natural than the day is for the smaller portions, is provided by the year for the larger portions. Upon the civil year, the interval between the sun's appearances on the same point of the ecliptic, depend the variations of the seasons. The year contains 365 days, 5 hours, 48 minutes, 50 seconds. Suppose, then, the year and the day to begin at once this year—next year will commence at $5^h \; 48^m \; 50^s$ A.M.; and when we calculate the number of days from the commencement of the year for any purpose, we should get confused between these and the ordinary day from meridian passage to meridian passage. The year must thus somehow be got to be considered for social purposes as commencing with the day, or very great confusion will arise. Julius Cæsar saw this, and appointed that in every fourth year there should be two days both reckoned as the 24th Feb., and that the months should stand as we have them now—February by this reckoning having really every fourth year, 29 days, and being considered to have only 28. This gave for three successive years 365 days, and for the fourth 366. Cæsar thought that thus a cure would be provided for the evil, and that once in every four years, the commencement of the physical year and physical day would coincide. The 24th of February was called "sextus Calendas Martii," and hence the year which doubled this day was called "bis sextus Calendas Martii," and we call it *bissextile* or *leap* year. This idea of doubling the day is legally affirmed by a statute of our own Henry III. Had the year been 365 days 6 hours long, Cæsar's anticipations would have been verified; and though it was nearly so, yet the little errors, constantly accumulating *in the same direction*, threatened serious inconvenience—tending to make our year commence in spring instead of winter, and destroying the value of the weather maxims for the month, on which the farmer and many others rely. When Pope Gregory XIII., in 1582, reformed the Calendar, the leap year had gone on about 11 days before the true year, and, in consequence, there seemed to be a slow retrogression of the seasons. To prevent this, and to render the positions of the seasons permanent in the year, except so far as physical causes might operate, it was enacted that so many days should be passed over, and the mischief already done thus repaired. Provision was also made against new danger of a similar sort. It was decreed that each century, which is naturally a leap year, should be an ordinary year, except those centuries the number of which should be a multiple of four. The effect of this was that 1600 was a leap year—that 1700, 1800, 1900 are not, although divisible by four—that 2000 is, and so on. The average year thus procured is 365 days $+ \frac{1}{4}$ day (due to leap year)—$\frac{3}{400}$ (due to the three years in the 400, which ought to be leap years). This would give a year of 365 days, 5 hours, 49 minutes, 12 seconds. The correction requisite to reduce to a true year would be 22 seconds, and there would not be an error of a day therefore, for $\frac{24 \times 60 \times 60}{22}$ years, or 3927 years. If it be further enacted, when the occasion calls for it, that each 4000th year shall be an exception to the exception to the rule of leap years, a similar calculation would show the error, per annum, on an average to be $\frac{1}{4}$ of a second, and an error of one day would not occur in 216,000 years.

Black. The absence of all capacity of reflecting colour constitutes a perfect black. See COLOURS.

Block of Pulleys. See PULLEY.

Blue. One of the seven primary colours. ;e COLOURS.

Bode's Law of the Distances. This *law*, ; it is termed, expresses a very curious relation nong the distances of the several planets of our ılar system from the sun; and of the satellites om their primaries. It is wholly *empirical*, *i.e.*, e know no physical origin or cause for it; evertheless, and notwithstanding the existence of xceptions or irregularities, it assuredly does point › some *conditioned* arrangement in our system.— With regard to the planets and the sun, the ıw may be presented as follows:—Write the ames of the planets in a line, and under each lace the number 4. Beneath the 4 under Mer- .ıry place 0; beneath the 4 under Venus write ; beneath the 4 under the Earth write *twice* 3; eneath the 4 under Mars *four* times 3; then *'ght* times 3, and so on. Add their several ›lumns as under:—

Mer.	Ven.	Earth.	Mars.	Ast.	Jup.	Sat.	Uran.	Nept.
4	4	4	4	4	4	4	4	4
0	3	6	12	24	48	96	192	384
4	7	10	16	28	52	100	196	388
3·9	7·3	10	15·2	27·4	52	95·4	192	300

'he numbers in the lower line are the actual dis- ınces of the p'anets from the sun, on the scale hat the earth's distance is 10. The general con- ›rmity is too great to result from accident. The xistence of the Asteroids at distance 27·4 was redicted by Olbers, through consideration of Bode's Law, because of the gap between Mars nd Jupiter. The grand breach of the Law is in he case of Neptune, a breach which might be xplained if we knew the cause or physical origin f the Law itself.—II. A principle of order quite orresponding, although in its *indices* somewhat lifferent, may be traced in the only two groups f satellites with which we are yet fully ac- quainted. *First*, with regard to the satellites of Jupiter. The constant number here is 7; the ıumber to be multiplied, 4; and the multiplier, 2½. Notice the correspondence as below—the Roman letters indicate the satellites:—

	I.	II.	III.	IV.
	7	7	7	7
	0	4	10	25
	7	11	17	32
True dist.,	6·9	11	17·5	31

Secondly, as to the satellites of Saturn. The con- stant number in this case is 4, and the other parts of the series very simple, viz.:

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
	4	4	4	4	4	4	4	4
	0	1	2	4	8	16	32	64
	4	5	6	8	12	20	36	68
True dist.	4	5·1	6·2	7·9	11·1	25.7	33	74

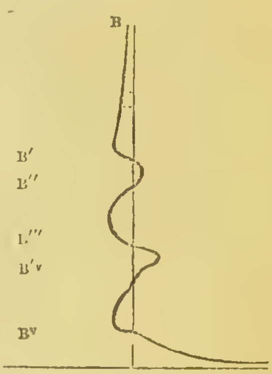
There is considerable irregularity in case of the last three satellites; but is it not some compensation, that the lately discovered satellite, *Hyperion*, or the *seventh*, might have been suspected to exist, as well as the Asteroids, because of the gap between the sixth and eighth, as indicated by this Law?—Of the satellites of Uranus it would be premature at present to conclude anything.

Boiling. See EBULLITION.

Bomb. A hollow globe of iron filled with powder, shot off from a mortar, and bursting by aid of a fusee, which is lighted when it goes off, but—being made of slowly consuming paper—does not reach the powder till the bomb falls. Bombs are generally ten, twelve, or eighteen inches in diameter. *Luminous* bombs are used to illuminate, or serve as signals. The invention of bombs is claimed for the Venetians as early as 1376, but other authors name Malatesta, prince of Florence, as the inventor.

Bootes. (Βοῦς, an ox). The herdsman. One of the ancient constellations. It is frequently called Arctophylax by the ancients (the guard of the bear). Bootes is represented as a man with a club in the right hand, holding two dogs by a leash in the left. Arcturus, of the first magnitude, is the principal star.

Boscovich's Theory. A remarkable speculation by an illustrious Italian, concerning the ultimate constitution of *Matter*. As it has frequently and profoundly affected the structure of *physical* theories, it is right to advert to it here. Looking at matter as resolved into its ultimate Atoms, Boscovich imagines that each ultimate Atom may exist to any other Atom in various relations; the actual relation in which they do exist towards each other, depending chiefly on the interval between them. The curious subject is best illustrated by a diagram. Let there be two atoms, A and B. At great distances these are affected towards each other by the attraction or affection termed gravitation; which affection, increasing as B approaches A (within sensible distances), may be represented by the curve above the line from B to B′—the *ordinates* of which, indicating the *force* of the attraction, increase, according to the well-known law, as B approaches A. Suppose that at B′ terminates the space of *sensible* distances, and the purely atomic forces begin to act. These atomic forces cannot be all attractive; nay, the ultimate one, between Bᵛ and A′, must be a repulsive one, as represented by a curve below the line A B; for, if it were not, the two Atoms might be made to *coincide*, or, what



is the same thing, matter would not be *impenetrable.* But between B^{v} and B$'$ there may be not one transition only, but several, from attraction to repulsion; as hypothetically indicated by the curve in the diagram. At any distance at which the curve is above the line A B, the two atoms will be mutually attractive, and the body they form will, according to Boscovich, be *solid.* At any of those distances when the curve is below the line, the particles would repel each other, and, according to Boscovich, constitute *gas*, or permanently elastic fluid; while, when the second particle is at B$'$, B$''$, B$'''$, &c., they would be to each other in a condition of indifference, and would thus constitute a *liquid.* The Italian philosopher presented it as the ultimate aim of physical inquiry to detect all the windings of this hypothetical curve, and define the nature of its branches. One thing his conception would indicate as *possible*, viz., the transformation of one apparent description of matter into another, *independently of chemical change.* For instance, if the two atoms, in the different positions of A B^{v}, A B$'^{v}$, A B$'''$, A B$''$, and A B$'$, constitute *liquids*, these could not be the *same liquids;* so that *transmutation* might be effected through mere change of relative position; and so as to those various positions, constituting solids and elastic fluids.

Boyle's Law. See MARRIOTTE.

Brachystochrone. The name of the curve connecting two points along which a body descending would fall most rapidly. The *cycloid* would be such a curve if there were no resistance of Air. John Bernouilli first proposed the problem of the Brachystochrone in 1696.

Breast Wheel. See WATER WHEELS.

Brewster's Law. The optical law, generally so named, is this—the tangent of the angle of polarization is equal to the refractive index of the polarizing material. This requires manifestly that the line of the reflected ray, when polarized, should be perpendicular to that of the refracted ray. There are several other optical laws discovered by Brewster, and passing current under his name. They have generally been merged in higher laws.

Bridges. The principles upon which ordinary bridges must be constructed, in order to secure their stability, have been already given in the article ARCH. Certain data there referred to —such as the strength of the wood or iron, or stone, of which the mass of the bridge is composed, to resist the strain made by the weight of the parts above it, and by the loads passing over the bridge—are discussed under STRENGTH OF MATERIALS. We shall in this article give a brief account of some points connected more with the practical art of bridge-making than with the theory of bridges, but still depending upon that theory and illustrating it.

Bridges are either fixed or moveable. The latter are sometimes—as drawbridges—constructed on the ordinary principles of the fixed bridge. They, in fact, are fixed bridges, but one part of their mass, instead of resting, as usual, on immovable props in the earth, is connected with it by props which are jointed to such immovable portions, and in which the jointing may be removed as occasion requires. What are called flying bridges, floating bridges, bridges of boats, &c., depend upon quite other principles, and are really altogether different in character from the ordinary bridge. They are boats of a peculiar construction merely; fitted with contrivances like the gangway of a steam-boat, which they carry about with them, and apply easily to shores of any height; thus affording ready passage from these shores to the boat. They are, in fact, mere ferry-boats, with gangways attached, sometimes having a smooth deck, with no sails or rigging, and constituted by nailing planks above two or three boats placed together. They are generally drawn across the stream by pulleys, and ropes fastened over it.

There are three descriptions of pier bridges. These are the ordinary bridge with piers, the suspension bridge, and the tubular bridge.

Considering then the *fixed* bridge, and proceeding in the order of their construction along its various parts, we shall understand where the mathematical principles of the theory of arches must be applied, and comprehend generally, the other principles upon which the art of bridge-making rests. In choosing which kind of bridge ought to be thrown over a particular space, there are certain evident considerations to be attended to,—for instance, the kind of traffic for which it is needed. Thus, a suspension bridge, admirably adapted for foot passengers across a wide channel, would vibrate too much if locomotives were to pass over it. Again, for example, a bridge over the Clyde must be so constructed that ordinary boats can pass one another beneath its arches; and this will require a somewhat wider arch than might have been found otherwise desirable. For such ends suspension bridges are admirably adapted—giving the whole width of channel, and not affording any obstruction to free passage, if the boats can lower their masts. Further: where we have a channel too wide for an ordinary pier bridge to be thrown over it without great difficulty and enormous expense, and too deep to permit us to get readily at the foundations of our piers; and also requiring that navigation of all sorts be permitted freely to pass below, even where the masts cannot be lowered; and demanding, further, that locomotives pass over it, we are driven to the construction of a *tubular bridge.* The nature of the soil, too, on which the foundations of piers must be laid, requires to be known, as a first element of the calculation. If we cannot secure such a soil near the point where we wish to construct our bridge as will suit our purposes, we have either to obtain an artificial foundation, by gratings and other contrivances,

or are driven to the use of a suspension bridge. Still further, it is a point of importance to consider the exact position of the bridge, with reference at once to the soil at its two extremities, and to the convenience of traffic. Where, for example, a bridge is to be thrown over between the mouths of two streets, both ending at the water-side, we have no choice; and if the line between them be oblique to the course of the water, the bridge must be oblique also. Where choice is left, we should never construct a bridge inclined to the line of the current. The reason is sufficiently evident. The whole value of a pier bridge depends upon the stability of the piers in their position. Hence everything that can bring unnecessary strain on these piers must be avoided. But, an oblique current, as in the first figure in figure 1, would strike against the pier, not as in the second, with the force of the mass of water, $c\,c'$, while the other water flowed along the side of the pier and through below the arch quietly, but with the force of the additional mass $c\,c''$ super-added. Hence, with such an obliquity there would be, in addition to the pressure of the superincumbent weight of the bridge and passengers pressing on the pier, and the concussion due to the rush of water beneath striking on the pier, and of the same width as it, also the concussion due to the mass $c\,c''$ to be sustained. Not only would this affect the pier directly, but the violent repulsion of so large a mass of water, $c'\,c\,c''$, would produce eddies about the pier, and these tend to undermine the foundations upon which the pier rests.

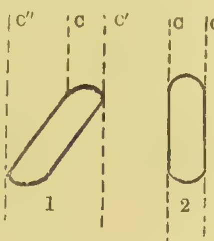


Fig. 1.

Sometimes, however, in spite of these objections, oblique bridges become advisable—for example, when we cannot obtain otherwise a good foundation, and can thus obtain one on rock—always the best for such a structure. Practically indeed the objection of greater expense from the greater length is the principal one, for most bridges on a tolerably good foundation will support a rush of water many times as great as that to which the front of the piers is usually exposed.

We have spoken of the piers in the water. These are the sources of greatest difficulty and danger in the formation of bridges. To bridges placed over a road or narrow street no piers are necessary except at the extremities; when a line of railway for example crosses a dry road, piers are far more easily constructed than in case of the ordinary bridges.

The form of the arches itself, is of inferior consequence. Many bridges have been swept away from the effects of the water in undermining their piers; while but few accidents have resulted from the adoption of arches not scientifically constructed. We can readily understand, moreover, how expensive a proceeding it frequently must be to find a good foundation for piers, in the middle of a rapid and tolerably deep current. The workmen cannot work under water well; or if, by the diving-bell, they do work under water, it is slowly and laboriously. Another difficulty also presents itself very frequently in the soil in the beds of rivers—often consisting of a light, loose, sandy material, on which it is unsafe to rest any substance which will press heavily upon it. In these cases it is usual to create a sort of artificial foundation. The present ordinary mode is by what is technically called a *coffer-dam*—a hollow space formed by a double range of piles, with clay rammed in between. Hence, it is a prime object with the engineer to lessen as much as possible the number of piers.

Piers have an unfortunate tendency, also, to catch floating wood, ice, &c., drifted down by the stream, and so to submit the mass of the bridge to more violent shocks, and to impede the navigation of the river.

The Putney, and Battersea, and Vauxhall Bridges over the Thames, afford examples of all the clumsiness of appearance, and practical inconvenience of the numerous piers that were once customary—besides having this added to their list of faults, that, being in a navigated river, they impede the passage of boats, and have thereby frequently proved sources of extreme danger. The more modern construction of Southwark and New London Bridges displays, in its striking contrast, very forcibly the position which we have been maintaining.

Sometimes — indeed, pretty frequently, the danger arising from the use of too narrow arches accrues to the bridge itself, as much as to the passengers below it, by narrowing the effective passage way of the stream. But as the same volume of water as before requires to pass where the bridge is placed, the water must flow with much greater velocity through the bridge; that increase increasing with the increasing space occupied by the piers. But this increase of the velocity of the water tears a large quantity of soil away with it, and deepens the stream. An undermining of the foundations of the piers takes place; and careful and sound construction alone can check this.

A bridge of Smeaton's construction at Hexham was swept away on the occasion of a violent flood, partly in consequence of this defect. The immense sheet of water, whirling down trees and stones, caught on the numerous piers, then thought necessary for the stability of the bridge, and finally swept it off.

Engineers sometimes make the space between the piers unnaturally wide, to avoid this very defect. A small island in the middle of the stream over which the bridge at Pont de Neuilly was to pass, was removed for symmetry,

and very few piers were erected. The current, which had formerly ran with considerable violence through the narrow space between the shore and the island, had now the space widened. The removal of the island more than made up for the insertion of the piers. In consequence, the stream ran more sluggishly than before, and the equilibrium being disturbed in this direction, the bed of the river began to fill up—becoming shallower and shallower by the formation of a deposit from the sides greater than was carried away by the current. In consequence of inequalities in the bed of the river, one side has become almost silted up by this alluvial deposit, and the arches there closed. The navigation on one side being thus seriously impeded, boats are driven to the other channels. There they meet with a current which has become alarmingly rapid. The silting up of this portion of the bed has left a very small portion passable, and through this the mass of water runs violently, deepening it much, and rendering navigation dangerous, at the same time that it permits the formation of a sort of lagoon at the side, which will gradually deposit more and more of the clays and mud that at present choke up the river.

In the construction of bridges, there are *five* important points to be considered. The choice of their position; the vent or egress to be allowed to the river; the form of the arches; the size of the arches; the breadth of the bridge.

The first of these points is determined, as we have already pointed out, by economical considerations—such as the nearness to a roadway; the convenience and security of a foundation; the convenience of approach; the necessity of leaving a free passage in a navigable river, and the like. The second depends very much upon the determination of the velocity of the stream. This varies, indeed, at different times It is generally greater in winter, and especially at a time of floods, than in summer, when the water is generally low. It is not the method, however, to take the mean velocity for the year as the datum from which the bridge is to be constructed. It must be built so as to be able to bear the heaviest stress to which it will be subjected—capable of sustaining the shock of a mass of water greater than ordinary coming down with unusual velocity. In fact, this is one of the fundamental points in the construction of a bridge—that it allow a free passage through its arches to the waters of such sudden floods as the river is liable to, and that its arches be sufficiently strong themselves to resist the violence of these attacks. This latter has been already alluded to, and the necessity for a firm foundation consequent upon that pointed out. Another caution evidently suggests itself. It is this—that the top of the intrados (as it is called, namely, the inner curve of the arch) be considerably above the level of the highest water mark which the river makes, even on the exceptional occasion of inundation. If it be not so, it would clearly result, that in an inundation continuing for any length of time, the bridge would in the first place, by checking the water entirely, cause a flood over of the adjacent country; and in the second place, such an enormous pressure would be brought against it by the constant downward rush of the upper stream that no bridge of any excellence of construction could stand against it long, or help being severely injured by the continuance of such a flood even for the shortest period.

To avoid such evils, necessitates frequently the building of the bridge in places where the banks are pretty high, or the construction of artificial mounds to serve in the same way, with expensive works for the convenience of approach. The old method, which made the bridge rise at the top very much like a semicircle, is entirely given up in modern bridges, as unsuited to the necessities of the great traffic which they must all now be able to support. But this is not the only way of avoiding this inconvenience. It arises as much, indeed, on ordinary occasions, from the greater or less pressure in high floods, due to the form and position of the arch.

Thus take two arches (fig. 2) of the same form,

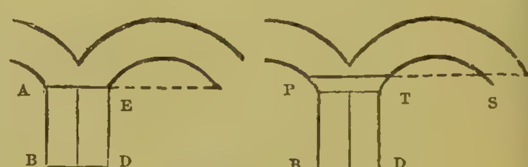


Fig. 2.

but the one built on such a pier, that at high water —represented in the figure—the water just reaches A, which is called the *springing* of the arch or point from which it commences, while in the other the pier is so low that it reaches the line P T S—the water being at the same height in the two cases, and the arches of the same form—the only difference being in the height of the piers in each case respectively. In the first instance, the water catches on the space P T B D. In the other, only on the space A B E D. There is therefore much less hold given to the descending stream in the one than in the other—that is, it has much less force to push the upper part down the stream, and so overturn it. In a bridge constructed like the first specimen above, a very little rise above the ordinary flood mark would insure all the calamities already described; and the sweep of the water on so large an extent of the building would produce a violent unsettling of its foundations, along with a multitude of eddies, from the large mass of water which, unable to pass the bridge, is thrown back every moment in violent conflict with the descending current. These eddies, producing an uplifting of the soil round the foundation in proportion to their amount, intensify all the effects already described. There is yet another peculiarity in this arrangement which acts even still more effectually. The

river requires, for the maintenance of the velocity above the bridge, that no part of its course be interrupted. If it be at all interrupted, as by the piers it must be, we have seen that an increase of velocity and of depth will take place. If, however, as in the second arch in fig. 2, the piers where it catches the water be regularly built, so that the space interrupted bears a regular proportion to the space left free, the velocity will increase below the bridge, and the bed deepen, until the balance is restored. This will not be materially disturbed by the rising of high water, as the interrupted space will bear the same proportion to the whole open space as before. But, in the other case, this equilibrium or balance will be disturbed immediately when a flood comes. The proportion of the intercepted space becomes forthwith inordinately large, and the velocity, on the principles already established, increases inordinately also, with the violent tendency to deepen the river, and sweep away the foundation of the bridge. The water, therefore, striking upon the bridge with this very great velocity, will be much more dangerous (in the proportion of the square of the velocities) than the same mass could have been at the ordinary rate of passage.

It follows from this reasoning, that it is of very great importance that the springing of an arch should not be below high water mark.

It is necessary, in constructing a bridge, that we should know *accurately* the actual stress to which it will likely be subjected in the event of a flood, and also in ordinary cases. For this purpose, we must measure the velocity of the stream in its natural way, and also in its extraordinary state of floods, near the place where the bridge is to be constructed. This is not so easily done however. Various methods have been proposed for its accomplishment. The simplest, and as it yet seems, the most effectual, is to let a piece of light wood float down the stream, from one fixed point to another. The time when the wood is dropped into the stream, and when it passes the second point, is accurately noted; and, the process having been repeated a number of times, the mean of them is taken finally. We obtain thus, the velocity of the water at the surface. But is this velocity not the same as that below? Evidently not. The water at the bottom goes slower because there is a bed of stone or other matter to retard it. The water in the middle is retarded to keep pace with this slower motion, and accelerated to keep pace with that of the top water. It moves at a mean rate. Calculations have been entered into, to show the connection between the surface speed with the average velocity of the whole water of the place —from top to bottom. No very decided results have been arrived at, but one sufficiently satisfactory in practice is stated by M. de Prony thus:—

$$\text{U} = \text{V} \cdot \frac{\text{V} + \text{A}}{\text{V} + \text{B}}$$

where U is the mean velocity required, and V the surface velocity, given in metres per second. He finds $\text{A} = 2.37187$ and $\text{B} = 3.15312$ to give results best reconcileable with both theory and experiment.

A useful consequence of this formula is, that when the velocity V varies between $\text{V} = 0$ and $\text{V} = 3$ metres per second, that is between 0 and 10 feet per second, which are about its usual limits of variation (0 and 7 miles per hour being nearly equivalent), the mean velocity will vary between $\text{V} \times .74$ and $\text{V} \times .87$. We may take therefore as a fair rule for ordinary application, though it is only a good approximation—

$$\text{U} = \text{V} \times .80 = \frac{4\text{ V}}{5}$$

This gives the mean velocity of the mass of water. But in order to know the shock that will be sustained by a bridge, and the new velocity which will be occasioned at the bridge by the amount of interruption which the piers cause, we require to know further, the amount of water which passes with this velocity. This will clearly be determined by the section of the mass of water at any place, multiplied by the velocity just found for that place. To find this former, we must cross the stream, sounding its depth all over, and marking the depths. We can then gain an approximate knowledge of the surface of the section, which we may suppose represented (fig. 3), by the figure having the level line at

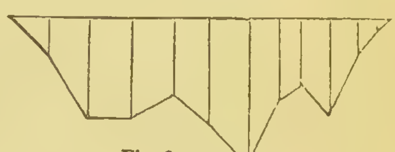


Fig. 3.

the top—the surface of the water; and the curved one at the bottom—the bed of the stream. We take the level one, and measure along perpendicular to it, lines equal to the length obtained by the soundings, at points corresponding to those where they were made; and by joining the ends of these lines, we see that, even with a pretty irregular bottom, we obtain a polygonal figure almost identical with it,—whose area, easily calculable, can be substituted for the accurate vertical section of the stream at the place. We know, therefore, the amount of water passing near the bridge, and the velocity with which it passes—that is, we can calculate how much shock the piers will have regularly to sustain; and also approximate to discovering what velocity the stream will acquire beneath the bridges, when any proposed system of piers interrupts the current.

The *form* of the arch is the next point of importance. There are three forms of special interest. 1. The old semicircular arch. 2. The elliptical arch, usually made, however, as a putting together of several circular arches of different radii; and 3. The segmental arch.

The semicircular arch is used almost exclusively in the more ancient bridge. It is more easily constructed than the other kinds, and it is more solid when it is constructed. But, as its height is half its width, it would require either very high banking, in which case, if a bridge with piers were desirable, it would probably be employed, or else the springing of the arch would be nearer low water than high water mark, and thus the inconveniences already pointed out would result. There is the further objection that it does not offer so agreeable and elegant a "*coup d'œil.*"

The other forms, the flat-vaulted arches, as they are called, and the segmental arch, have each their peculiar advantages and disadvantages. They both present less obstacle to the passage of water. Either may be employed where the springing of the arch is above high water mark, without causing such an inconvenient rise from the side to the centre of the bridge, or requiring banks higher than are usually to be found. In the flat-vaulted arches each circular arch is nearly the semicircle of a much smaller form, and the same advantages of solidity and stability securable in the first case are so with them. The lateral pressure of the *voussoirs* (or stones forming the actual arch) is very considerable in the case of the segmental arch, and care has to be taken that the crown does not sink down after the arch is formed in this construction.

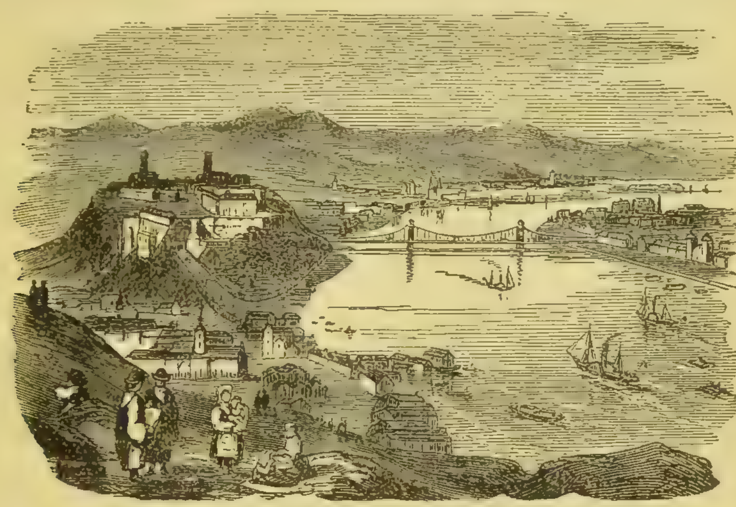
Bridges are constructed either of wood, of stone or brick, or of iron. The first is objectionable on account of its rapid decay, especially under constant exposure to water. Some kinds of timber indeed are less liable to decay. Thus oak piles, when entirely submitted to this action, are not liable to rapid decay; but if one part of the same log be in, while another is out of the water, unequal action produces a rapid decay—like the galvanic current, decomposing substances through which it circulates. The ordinary woods moreover change their bulk according as they are wet or dry, and so produce a warping or a bursting action, tending, so far as it goes, to disintegrate the bridge. The cheapness and the convenience of procuring wood is its great recommendation. Besides, when a more secure building, such as a stone bridge, has to be erected, it always requires to have a sort of platform of wood, called *centering*, erected beforehand, and continued as the work proceeds, for the use of the workmen. The material for this, with a little more work, would construct a tolerable wooden bridge. A bridge of iron is objectionable because of the violent expansion and contraction of the material in heat and cold, and from this cause it is not so extensively used as it would otherwise be. Principles of compensation, however, similar in principle to that employed in the compensation balance of a watch, may go far to correct this evil. The other contingency—of the oxidation of iron from exposure to rain and air, is avoidable by painting and other devices which, however, add a large per centage to the original cost of the bridge. No structures of the ordinary bridge kind, so little obstruct the course of a stream as iron bridges.

Suspension Bridges.—When a chain of iron, or of any metal, is suspended at two points, it takes a certain form depending upon the weight at the various points. The action of gravitating forces draws it downwards—resisting forces, of indefinite capacity, at the points of suspension, keeping it from falling entirely; and cohesive forces transmit this retention from point to point of the whole mass. We can readily understand that these forces will come to balance after bringing the chain to a certain definite position; and that unless the cohesive forces or the retaining power of the points of suspension fail, there will be always a position in which the curve will remain fixed; which position once reached, new forces will be required, again to disturb it. The principles upon which these curves are determined belong to the higher mathematics. If the forces which pull a curve be at right angles to its line, and all equal, an arc of a circle will be formed, as we might have expected from the uniformity of the action and the regularity of the figure. If all the forces applied to be vertical—if, for example, a chain be used, uniform in mass, and permitted to hang freely, a peculiar curve called the *catenary*, or chain curve, results. If the mass be not uniform, there will be deviations from this curve, in the shape of lines that will give greater deflection with the greater weights. If, for example, the forces which act upon each point of the chain are proportional to the lengths of the projections of the small equal parts adjacent to the points, the resulting curve will be a parabola. If the chain be constructed so that these forces are applied to successive points, instead of along the whole chain, a polygon would be formed, having the low points of suspension for vertices, all which points hang along a parabolic curve. We have said that these results will occur, and such curves be formed, unless the cohesive forces of the material of the chain be too weak to stand against the other divellent forces. We can imagine forces of any kind applied to a chain so fixed, and this fixity remaining if the chain be strong enough. This is the idea on which the suspension bridge of the present day is constructed.

Two towers are erected, of such height, that the chain may have its natural swing, and the passage of the river or road not impeded by the bridge hanging over it. A chain of very strong iron is then fixed to the ground at the two ends, and passed up through a passage in the tower, leaving sufficient length to hang in the curve contemplated. Subsidiary chains are suspended to it by various points, and to these is hooked a roadway con-

ructed of planking, over which passengers cross. he chief strain here is upon the towers, which .ust be capable of resisting the turning tendency ' the whole weight in the centre to draw them .to the river; and on the chain. The origin of ıe strain is threefold. First, the weight of the ·iginal chain; secondly, that of the subsidiary ıains by which the roadway is suspended; and ιirdly, that of the roadway and of the passengers. he chains must be strong enough to sustain the ·nsions to which they are severally subjected, ;herwise the bridge will fail.

There is one serious difficulty attaching to the suspension bridge, but it is so serious that it seems to preclude the use of this elegant contrivance except for passengers. The bridge has no stability. Two causes are readily assignable. In the first place, the iron of the chains is liable to great expansion or contraction by heat or cold. And in the second place, the whole structure hanging in the air is liable to oscillate with the wind. No contrivance will probably do away with this latter evil, or indeed with either. Both may, however, be con-



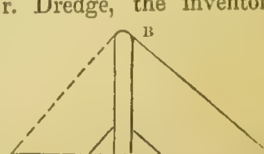
Suspension Bridge across the Danube connecting Buda and Pesth.

iderably mitigated. The best method for doing his is to use as few subsidiary chains as ·ossible; just as in the other construction of ·ridge it was found necessary to diminish the .umber of the piers.

An advantage evidently arising from the plan f using very few chains, is this: no two bars f iron are found to expand always alike. Now, f we imagine the vertical and subsidiary chains o expand differently, we shall have this result: he weight of the whole mass pulling the less xpanded downwards, and the more expanded .elieved of that weight. The whole mass of the ·ridge may thus, and at times does, hang upon ·ne or two chains, and these must be made so trong as to be capable of bearing it. Hence, here is a great waste of material in these chains. f 40 be used, then the whole weight will require o be carried by $\frac{1}{40}$th of the mass used (roughly), ınd if only 5 be used, by $\frac{1}{5}$th of the mass. The atter mass, therefore, may be just eight times ess than the former, and must expose, therefore *cetores paribus*), a great deal of less surface to he action of the wind. We can go so far, herefore, towards the diminution, but not in this vay to the destruction of vibratory effect.

Such are the chief principles, upon which the suspension bridge is constructed.

A very elegant variation of the ordinary sus- ·ension bridge goes under the name of Dredge's suspension bridge. The strain upon different links of the suspending chains will clearly not be quite the same. Thus, in an ordinary chain, the strain will be greatest at one end, and least at the middle. Mr. Dredge calculates, upon mathematical principles, the variations to which the strain is thus liable; and constructs these chains such that the middle links be light proportionally to the strain they have to bear, and the end ones so much the heavier. This secures an economy of material, important in the construction of the suspended chains. He also uses oblique suspending rods instead of vertical ones. The still greater equalization of the strain, and the lessening of it by this particular invention, are capable of mathematical illustration of a not very complex nature. In the bridge, the chains become nearer and nearer the horizontal, the nearer they are to the middle of the bridge. A very beautiful specimen of this construction of bridge is formed across the river Leven, at Balloch, on Loch Lomond, built under the personal superintendence of Mr. Dredge, the inventor.



The figure illustrates the principle. Let F A be a part of the roadway, supported by the chain A B; then the force due to A F will pull vertically downwards—being that of gravity

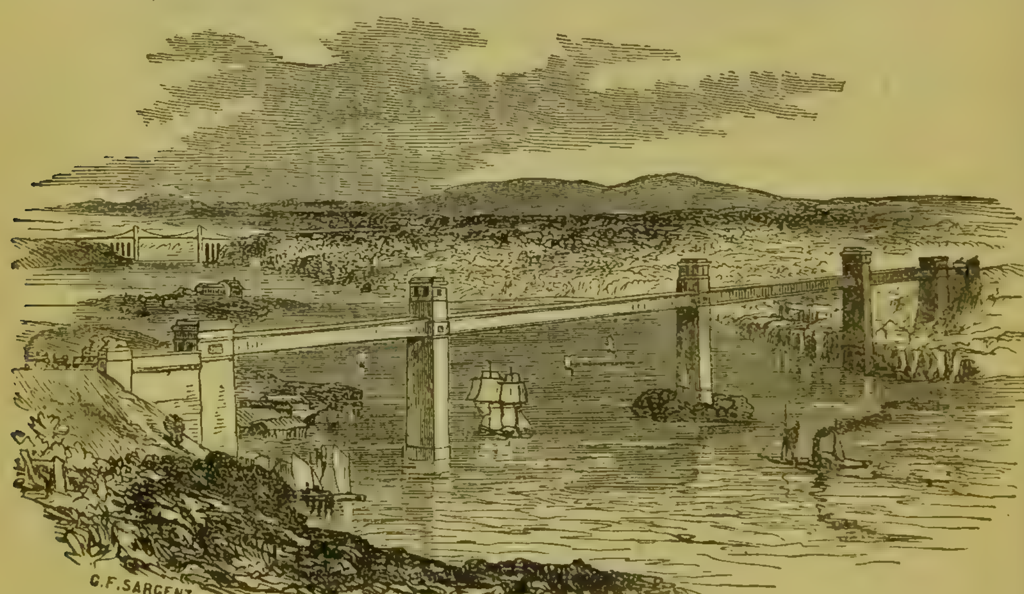
—having, besides, a tendency to make the mass rotate, that is to disturb the fixity of the system by drawing B (suppose the top of the tower) inwards. This is resisted, and there remains a vertical force which we may imagine to pull right down, acting on the rope, through F. This vertical force can be decomposed into two—one perpendicular to the line A B, and acting on F, a supported joint, and another along the line A B. This force along the line is evidently smaller than that vertical one of which we spoke, which is the only one acting in the ordinary suspension bridge, and the strain is therefore less. The only limit to the capabilities of this construction is the danger of throwing too much weight on the fulcrum F, and of causing a turning effect upon the top, B, too considerable.

The suspension bridge, though giving an admirable passage to ships below, was not yet quite capable of fulfilling all the requirements of a modern bridge. Many of them require to admit of the passage over them of very heavily laden trains. Now these two advantages of free roadway, and of sensible stability, could not be combined in the suspension bridge. We annex an engraving of a suspension bridge.

The *tubular bridge* was devised to combine these advantages. We shall avoid, however, entering into its principles at length.

The chief point upon which the construction of tubular bridges rests, is the capability of wrought iron to resist a tension much greater than cast iron can. The old iron arch bridges were almost all built of cast iron. Cast iron bears a transverse strain better, and the wrought iron a strain lengthwise; and at the time of construction the bridge might be made, so, that when without passengers, the two should work completely together—the transverse strain borne by the cast iron, and the lengthwise one by the malleable iron. Unfortunately, this is not the purpose for which bridges are constructed, and it is easy to conceive loads so placed on the line, that each strain would fall to the mass least fitted to sustain it. A melancholy instance of this occurred on the bridge over the Dee, on the line of the Chester and Holyhead Railway. It broke down completely on the 24th May, 1847, occasioning serious loss of life.

The tube *through* which the roadway runs in the tubular bridge sustains all the weight in its various parts. It was a question of the utmost importance to determine, where the great stress on the tube would be, and what would be the form best calculated for strength. Both of these matters required experimental determination, and a series of experiments as elaborate as any on record were entered into, under the direction of Mr. Robert Stephenson, by Messrs. Fairbairn and Hodgkinson, the results of which will be found de-



Britannia Tubular Bridge across the Menai Straits.

tailed at considerable length in the recent supplement to Brunell's Theory of Bridges. It came out in the course of these investigations, that the Circular and Elliptical tubes were not nearly so well fitted to sustain a weight passing through them as the much more simply constructed *rectangular one;* and the remarkable fact was elicited, that the iron ought to be made thicker at the top of the tube than at the bottom, because *there* was the greatest strain. This is a consequence of the fact already noticed, that wrought iron is much less capable of sustaining a transverse load than cast iron is, although on the other hand it is far more capable of bearing a lengthwise strain.

The magnificent series of inventions by which all the details of these bridges have been perfected, has, perhaps, no parallel in modern engineering. Every point connected with their con-

:uction, the riveting of the plates, the construc-ɪn of the tube, the elevation of it without ntering or scaffolding of any kind, has given ope for an ingenuity, equally remarkable for ɪ energy and persevering application. This st idea—that of erecting a bridge without affolding—gave Mr. Stephenson the first no-ɔn of a tubular bridge. It had been proposed raise a bridge over the Menai Straits, to faci-ate intercourse between Dublin and London. ɪe Admiralty, however, required that, in the nstruction of this bridge, there should be a clear ·ight of 100 feet left across the whole channel, en during the time of construction. This—to ɪy other man—would have been a simple veto ɪ the construction of a bridge of 1,500 feet long; ɪt it conducted Mr. Stephenson to the construc-ɔn of the Britannia tubular bridge. See .KEW-ARCH.

Brittleness. A property of bodies, which though solid, yet are so weakly bound together, at a very small mechanical force suffices to parate their particles. They can be easily duced to powder. The cohesive force between eir perceptible particles almost vanishes, but ey differ from liquids in possessing a consider-ɪle cohesive force, acting between the particles hich are so small as almost to be imperceptible.

Burning Glass. We shall see in the articles ATOPTRICS and DIOPTRICS, that rays of light di-rging from one point, and falling upon mirrors peculiar shape, may be thrown back from em, so as all to intersect in one point, and that incident upon a lens of peculiar shape, they ay pass through it, emerging in such a direc-ɔn as also to concentrate about a particular ɪint.

Thus, for the first case, we shall see that small ncave spherical mirrors will throw back rays ming from a point tolerably distant, so as to tersect very nearly in one focus; and if the ys be parallel, as some rays are, or the point ɔm which they come very distinct, which is the ɪuse of this parallelism, we may have with a ncave mirror which has a very small central ɪgle, this concentration of rays as accurately as e may choose. The rays incident upon a herical mirror convex to these lines of inci-ɪnce, do not pass, when reflected, through the ɪint from which they diverge at all. Their fo-ɪs is behind the mirror.

We shall see, also, that a paraboloid mirror, ɪrned concave to the line of incidence of the ɪys will reflect parallel rays, as from the sun, or ɪoon, or a star, accurately to one point, and ɪat an ellipsoidal mirror will reflect rays diverg-ɪg from the one focus, accurately on to the her. With a mirror of the hyperboloid form ɪe same result would be obtained as with a con-ex spherical mirror; the rays, when reflected, ould appear to diverge from a new point, but ɪis being behind the mirror, they would not ac-ɪally pass through it.

Now, the sun's rays possess a heating as well as a luminous power; and although different rays do possess this power in different degrees, yet, as the different nature of rays causes no deviation in any of them from uniform laws of geometrical reflection, and as none of the component parts of white light are separated in ordinary reflection, we may consider each ray of ordinary light as simply possessed of heating power. Let a number of heating rays, therefore, diverging from any source of heat, fall upon a mirror from which they are accurately reflected, so as all to cross one another at a given point. It is plain that at that point there will be a very great concentration of heat, and that bodies placed at it may be subjected to a very high temperature. This is the principle upon which burning mirrors are constructed. The readiest source of heat is the sun, and the mirrors hitherto employed have been made principally so as to concentrate its rays. In the expressions by which the radius of the mirror and the distance of the luminous point from it are connected with the distance from it of the focus of reflection, we have simply to make the distance of the luminous point become infinite, as in comparison with all ordinary terrestrial distances it may in this case be fairly considered, in order to apply the ordinary formulæ to the action of sun rays.

The substance of the mirrors should not be transparent. But, for the second case, that of burning lenses, the transparency of the glasses is the property on which their power depends.

We find (see DIOPTRICS and LENS) glasses of this character, that they gather light coming from a point, and falling scattered on their surfaces into another point behind the lens. Some, indeed, like the convex mirror already noticed, only scatter the rays less, but still allow them, after passage, to go on diverging, as if they came from a point through which they do not, in reality, pass at all. These lenses are as useless for our purposes as the similar mirrors were, but wherever we have lenses which concentrate luminous rays after passage, towards a point through which they really pass, we can use them for burning glasses. Frequently any lens which could conveniently be made, more frequently any which we may actually have in possession, will not throw the rays exactly toward the point where we desire to concentrate them. And in that case we may combine two or more, so as to prevent the convergence of the rays to a point too near, by a slight dispersive power, or to make the concentration still more intense than after passage through the first lens. Here, however, we are met by the fact of the dispersion of ordinary light by refraction through lenses. There is one focus for the violet, another for the red, another for the blue rays of the spectrum. If, therefore, the heating power of the solar ray be uniformly distributed among the various homogeneous rays, we shall require to use for burning lenses the

same arrangements which we have already seen to be necessary to prevent chromatic dispersion. It has been fortunately, however, found that the more refrangible rays, those towards the violet end of the spectrum, possess very little heating power. The experiments of Sir W. Herschell, in 1800, proved that the calorific effect at the various points of the prismatic spectrum was greatest for the extreme red rays, and extended even considerably beyond it, and beyond the spectrum. Hence we may take the sort of mean index of refraction for the heating rays to be very nearly the index of refraction for these extreme red rays. By convenient lenses accordingly, we may concentrate these rays at the foci of our lenses, and obtain a considerable heating effect also upon objects very near the focus of the lens, upon either side of it. Arrangements similar to the achromatic arrangements, by which the dispersion of colours is prevented, would undoubtedly, however, very largely increase the heating effect.

The principle upon which burning glasses are constructed is, therefore, simply this,—that the heating rays, either from the sun and other heavenly bodies, as is most usual, or from other sources of heat, be concentrated as nearly as possible to a point. This is effected in burning mirrors without regard to the dispersion of heterogeneous rays; and, in the case of burning lenses, we may either employ arrangements akin to those in achromatism, or with simple lenses may construct them as if intended to collect the extreme red rays. The burning lens is convex, as the burning mirror is concave.

The use of burning glasses is certainly very ancient. Aristophanes mentions them in "the clouds." The celebrated exploit of Archimedes, who set fire to the Roman fleet at Syracuse, by means of a burning mirror, rests upon a mass of historic testimony not easily set aside. The use of lenses is certainly not much more modern. Pliny mentions balls or globes of glass or crystal, which, when exposed to the sun, are capable of transmitting sufficient heat to set cloth on fire, and Clemens Alexandrinus and Lactantius suggest the employment of globes of glass filled with water.

In order to give some idea of the power of these instruments, we shall transcribe an account of one made by Mr. Parker. His lens is made of flint glass, and when exposed, has a clear surface of 32 inches diameter. Its thickness in the centre is $3\frac{1}{4}$ inches, and its focal length 6 feet 8 inches.

He used, along with this, another lens having a clear surface whose diameter is 13 inches, whose thickness in the centre is $1\frac{5}{8}$ inches, and whose focal length is 29 inches. This lens, when arranged along with the other, still further concentrates the converging rays which have already passed through it. It was placed by him so that the focal length of the two was 5 feet 8 inches. A number of experiments gives the following series of results:—

Substances fused, with their weight, and the time occupied in fusion,—

	Time in Seconds.	Weight in Grains.
Wrought Iron	2	12
Common Slate	2	10
Pure Silver	3	20
Pure Platinum	3	10
Nickel	8	16
Cast Iron—a cube	3	10
Pure Gold	4	20
Crystal Pebble	6	7
Lava	7	10
Asbestos	10	10
Bar Iron—a cube	12	10
Steel—a cube	12	10
Garnet	17	10
Pure Copper	20	33
Onyx	20	10
Zeolites	23	10
Pumice Stone	24	10
Oriental Emerald	25	2
Jasper	25	10
White Agate	30	10
Oriental Flint	30	10
Topaz or Chrysolite	45	3
Common Limestone	55	10
White Rhomboidal Spar	60	10
Volcanic Clay	60	10
Cornish Moorstone	60	10
Rough Cornelian	75	10
Rotten Stone	80	10

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Calculus, the INFINITESIMAL; or the *Transcendental Analysis:*—the names usually given to that great branch of the Mathematical Sciences, which, since the time of Leibnitz, has commanded, by one general method, the most arduous problems in geometry, mechanics, and physics. Perhaps the simplest definition that can be given of this potent Algorithm is the following:—*In ordinary Algebra, the values of unknown quantities, and their relations with each other, are detected by aid of equations established* BETWEEN THESE QUANTITIES DIRECTLY;—*it is the fertile artifice of the Calculus, on the other hand, to arrive at such systems of direct equations by means of other equations primarily established not between the* quantities themselves, BUT BETWEEN CERTAIN DERIVATIVES FROM THEM, OF ELEMENTS OF THEM. It is found much more easy in practice to determine the equation between such elements and derivatives, than between the primary quantities; when this is accomplished, the problem may be said to be *laid* analytically; and the subsequent process—that, viz., by which equations between the primaries, are deduced from the relations of the derivatives—is a simple process and difficulty of pure analysis. The fundamental requisition of the Calculus, was evidently this—to establish rationally, the notion of these *derivatives* or *elements*. Three schemes have been proposed, amounting to the same thing in practice,

liffering in the strictness of their logic, as as in the facility of their application. If one d find the true logic of the new Calculus, rse must still be had to that memorable sec- of the *Principia*, in which Newton exposes ieory, and demonstrates the chief theorems, in Method of the *Ultimate Values* or *Limits* of Ratios of variable quantities. This concep- or principle of *Limits*, is now universally ted in establishing the foundations of the scendental Analysis by all rigorous logicians; s it easy to see that any other course is open. atisfied with the conception of *Limits*, as not rely analytical one, and also dissatisfied for ar reasons with the original method of nitz—the illustrious Lagrange offered a new of contemplating the origin of these *deriva-* Presuming to have demonstrated that any ion of x—$\phi \cdot x$, could, after x had received ncrement h, be developed in a series as v:—

$(x + h) = \phi\, x + A_1, h + A_2\, h^2 + A_3$ - &c., in which A_1, A_2, A_3, &c., should con- the variable x only—he required that these cients, A_1, A_2, A_3, &c., should be accounted successive, or the *first*, *second*, *third*, &c., atives of $\phi \cdot x$: and so averred that the re of these derivatives could be presented as ng out of a purely algebraical relation or —the gap thus being filled up that had ared to separate *in logic*, the Algebraical the Transcendental Analysis. Notwith- ling the fair appearance of the *Theorie des tions*, it cannot be said that Lagrange has eded. No equation, on one side of which is nfinite series, can be termed independent of dea of *limits;* simply because it means that inite side is the limit of the infinite side; nor the several steps by which Lagrange reached lemonstration of *Taylor's theorem*, unmixed the same class of considerations. And what ll more formidable, is the fact, that the appli- n of the Calculus under its new form, turns for the most part so cumbrous and incon- ent, that Lagrange himself abandoned it in *Mecanique Analytique*, for the much easier esses of Leibnitz. This latter extraordinary us—to whom the first systematic statement of principle of the Transcendental Analysis is ntestibly owing—presented it originally under *ird*, and in use, still its favourite form, that *finitesimals.* Not wholly unlike the manner avalieri in his once famous method of In- sibles, Leibnitz considered all quantities or nitudes made up of *infinitely small elements* nfinitesimals, named *differentials;* and he ved that in the great majority of cases, the ions of the quantities concerned in a problem, ht be much more easily expressed, by equations veen these elements, than between the primary itities. Sometimes it turned out difficult to blish even these equations; but in that case, end might be gained by decomposing the differentials themselves into other infinitely small elements, and seeking relations among such *second differentials:* or still further, differentials of the third order in descent might be resorted to; and so on in the same manner. There cannot be a doubt that Leibnitz's conception is of all others the most facile in practice; so much so, that it may be said to be the only conception in the mind of any one extensively employing the Calculus; but at first it appeared objectionable in principle. The idea of Infinitesimals itself, cannot be termed a just or pure analytical idea; and in the working of it out, according to the methods of its inventor, one is called upon at every step to reject infinitesimals as being *incomparable* with finite quantities, and the higher orders of infinit- esimals as being incomparable with lower ones,— a process bearing much too close a resemblance to a mere process of approximation, to be received without scruple as the instrument of exact in- quiry. Geometers did not soon succeed in dis- covering why, out of methods wearing on their front the garb of mere approximative methods, the exactest results were, through some neces- sity, evolved. D'Alembert, Euler, and others, sought the key of the puzzle in vain. At last Carnot detected it. The methods of Infinitesi- mals are correct, through effect of an *exact com- pensation of errors:* if the equations originally formed are in error, or, as Carnot termed them, *imperfect*, through neglect of quantities infinitely small in comparison with the quantities entering into these equations,—*a process occurs of precisely the reverse nature when these auxiliary equations are finally raised to equations between finite quan- tities;* which last are therefore and necessarily *perfect* equations. The logic of the processes of Leibnitz, vindicated in this way, the sole objec- tion that remains to the method of this geometer, has reference, as we have already said, to its being founded on a conception like that of infini- tesimals.—Such in general the nature of the ideas on which the Transcendental Analysis is founded; let us now enumerate and briefly deli- neate its main divisions. It is sufficiently evi- dent that a Calculus which proposes to discuss and determine the relation of quantities from re- lations established between other quantities de- rived from them, must consist essentially of two principal parts. *First*, it must explain the rules by which, the primitive quantities being known, the analyst may determine their *derivatives* (*fluxions*, *infinitesimals*, *differentials*); and *se- condly*, it must explain that inverse set of rules or methods by which the primary quantities may be deduced from, or detected by aid of, these derivatives. We shall offer a few remarks on each of these great divisions.

(1.) *The Differential Calculus.* It is the object of the Differential Calculus to lay down, how we obtain the derivatives of each of those few simple functions or modifications of quantity which are alone recognized in analysis; and this, whether

these functions are presented singly or in any form of combination. The functions are—the *sum* of variables, their *difference*, their *product*, *quotient*, *power* or *root; exponentials*, *logarithms;* and *direct* and *inverse circular* functions. Now, the Differential Calculus, as thus defined, may be termed *complete.* We can *differentiate* at will any of these known functions, or any combination of them whatever. It is of no consequence whether the function to be differentiated (or made to yield its *derivatives*) be explicit or implicit: the known methods of derivation apply to all cases; and with equal ease, any function may be differentiated a second, third, or any number of times, or pursued to any order of its derivatives. This Calculus in itself is susceptible of many important and interesting applications, such as to problems of *maxima* and *minima;* but these we cannot at present notice.

(2.) *The Integral Calculus.*—The *Inverse* portion of the Transcendental Analysis is very far from being as perfect as the former. Its methods, instead of being general, are little better than happy artifices whose origin is mostly due to the occurrence of difficulties in the treatment of geometrical, mechanical, and physical problems; nor are there many analysts who would be inclined to question the opinion of Laplace, that *integration* is one of those difficulties whose general solution we cannot hope for. The remarks that can be offered here are so few, that we shall wholly leave out of view the question of the integration of implicit functions, or of *differential equations*,—an immense subject, of which, in comparison with its extent, we can scarcely be said to know anything. Explicit differential expressions are those that take on the following form:—

$$\phi x.\ dx.$$

—simple enough as so expressed; but the integral even of these cannot be found unless with regard to a few of the more elementary forms of $\phi\,.\,x$. If $\phi\,.\,x$ be either an *exponential*, a *logarithmic*, or a *circular* function, the integral of the foregoing expression cannot be directly found unless in a very few and simple cases; to treat which even, generally requires great ingenuity. One of the leading artifices applied in this case, is the *Integration by Parts* of John Bernouilli; by whose means a difficult expression may often be reduced to another more easily reduced. If, on the other hand, $\phi\,.\,x$ includes only the common algebraic functions, we still cannot treat the case generally, unless $\phi\,.\,x$ be *wholly rational.* If it is irrational, it cannot be treated at all unless in the few cases in which, by substitution, it can be rendered rational. It may be said, therefore, that the Integration of rational functions is really the only portion of the great inverse problem of Analysis over which we have yet entire command. Want of space constrains us only to refer by name to methods of Integration by *approximation*, or to that curious Transcendental Arithmetic, known as *Definite Integrals.* We cannot omit, however, an expression of regret that we are not enabled to explain, in reference to Differential Equations, the most ingenious and admirable method indicated by the name of *Singular Solutions.* The student must apply for details to some of the excellent text-books now common in this country.

(3.) *Calculus of Variations*, *Calculus of Finite Differences*, *Calculus of Functions.*—For brief notices of these important off-shoots from the original Transcendental Analysis, we refer to VARIATIONS, DIFFERENCES, FUNCTIONS.

Calendar. A distribution of time, accommodated to the uses of ordinary life. In seeking for some base upon which to rest such a division, the first that suggests itself, as being the widest, for which we have an evident physical ground, is the division into revolutions of the sun round the earth, or of the earth round the sun, *i. e.*, from equinox to equinox. Upon this depends the cycle of the seasons. Another unit, however, independent of this, seems to have equal claims on us; viz., what is called the civil day, or the interval of time from one passage of the meridian by the sun, until his next passage of it. If, then, we should adopt *either* of these as the unit of time, and the standard of our measurement, we should have good physical grounds upon which to proceed; but would require a peculiar nomenclature for parts of the unit not *aliquot* (as in the case of the day and the year); or for multiples of the unit by fractional or mixed numbers, for the odd hours (5 hours, 48 minutes, 50 seconds), of the day. In fact, the day and the year are each of them so important, that we are forced to give up all hope of the adoption of one standard, and a uniform system, and consent to the admission of two independent units. We could, indeed, speak of the year as $\frac{3155693}{8640}$ of a day, or of a day as $\frac{8640}{3155693}$ of a year, and there would be some advantages in so doing; but these would be far more than counteracted. We are forced, therefore, to employ the two units conjointly. Attempts have been made even to introduce another independent unit. The period of the moon's revolution, next to that of the earth's annual and diurnal period, is undoubtedly the most prominent, and it has accordingly been chosen. This has not, however, been generally adopted. The system almost universally used rests upon the smaller of these natural units—the civil day. We endeavour to adapt the year, by various contrivances, (see BISSEXTILE) to the day; so that we may not have the commencement of our civil years, far from the actual commencement of the true year; while we are enabled, for the sake of convenience, to consider the year as made up of a number of *whole* days. We are desirous of this latter result, for the facilitating of calculations, which would be very tedious if we

to consider in what part of the year any ment of any particular day might chance to ; and of the former, that so we may be enabled compile rules of meteorology for our obser- ions,—rules that may be applied without nging our time from the diurnal standard of surement to the solar, or *vice versâ.* A cal- ar approaches perfection, the more closely it ils those two conditions.

A sort of Epicycle system has been introduced it on to this kind of calendar, which is in- ded to secure the same practical coincidence ween the commencement of the month and of nation, or revolution of the moon; while it mits us to count each lunation as made up , number of whole days. Attempts to recon- practically the commencements of the year . the lunation, have also been made. The enians thus tried to reconcile the commence- nt of a month, composed of a number of whole s, and a lunation, and made the months to sist of 29 and 30 days respectively. This hod might do extremely well, but for the in- venience that 12 of these months would only e 354 instead of 365 days, and that the re- ining 11 days would confuse the arrange- nt. In fact, practically it has been found in st nations that the unit of a lunation is by no ans so important as that of a day or of a year hat the physical circumstances which complete ir cycle along with it, are comparatively few; l that as the introduction of it into the calen- complicates further a system which is, for the nmon people, already complicated enough, it is necessary and would be troublesome. Hence, 12 is nearer than any other whole number of ations to the space occupied by a year, we ide the year into 12 months, appointing the nber of days in each month as approximately, haps, as the case will admit of, to be 31 and

What is called the *luni-solar* calendar, has, fact, been found quite useless. The Grecian l Macedonian calendars were based upon it, l the calendars of the Eastern nations, the Hin- s, Chinese, and Japanese, still are. It is not d in Europe at all, except by the Jews.

What is called the purely lunar calendar—in ich the return of the moon is taken as the ef unit—but which rather is the one resting the *two units* of the day and the lunation, little physical foundation. It is, in conse- ence, not very extensively used. Here we e to consider the lunation, as consisting of so ny whole days, and we have to arrange the mber of days in the month so that 12 of them l come to make up a period of 354 days, 8 rs (the period of 12 lunations). If we make h month to consist only of whole days, the allest cycle which we can employ will be three es that, or 1063 days, in which 36 lunations ll be accomplished. The professors of the Mo- mmedan religion use this lunar calendar; t the confusion which it introduces into the seasons, and the practical restraint it puts upon agriculture, by making their various periods of time not at all correspond to the recurrences of the principal influences of meteorology, have prevented its being introduced anywhere else.

Some of the more ancient calendars made very slight attempts at any reconciliation of the two standards of the day and the year. These could only be, and were only tolerated, in the infancy of astronomy.

We shall just mention rapidly in conclusion, the methods by which the day and the year have been sought to be kept as bases of one system, by successive nations.

The Egyptian method is perhaps the most ancient, and one of the most interesting. In theory it is almost identical with the Julian method, yet how different in its practical results! It divides the year into 12 months of 30 days each, with 5 additional intercalary days (*i.e.* belonging to no particular months, but makeweights, as it were). This year of 365 days, however, is in error $\frac{1}{4}$ day each year, and, therefore, 365 days ($1460 \times \frac{1}{4}$, days) in 1460 years (*magnus annus*). Hence at this period we should come back to our true starting place, and the commencements of our civil and natural years would agree. It is needless to remark on the inadequacy of this method. It would only be endurable for $\frac{1}{6}$ part of the time, during each *magnus annus*, within which the error would not be more than a month either way; but for the other 1217 years we should have the seasons all altered from their standard positions by at least more than a month.

The Persian method made nearer approach to a good calendar. They had the same system of 12 months of 30 days each, with 5 additional intercalated days; but they added a thirteenth month (30 days) at the end of each 120th year. This secured a counterpoise for the errors of a quarter of a day per year, accumulated during that period. During 60 years of each 120, however, the year was still more than a fortnight out of place, and though its restoration to a true state was indeed provided for, the fact of that error was not removed, and the same provision which at first brought back the true year brought back the error afterwards. The Julian method, and the Gregorian, with the various improvements which may be applied to that, when necessary, are detailed in the article BISSEXTILE. The Julian provided for a restoration, every four years, of the agreement, and if the true year had been $365\frac{1}{4}$ days, would have been perfect. The Egyptian and Persian methods failed also in considering this as the true year, although the close approach to the truth is much more remarkable in this case than the slight deviation from it. The methods by which the day and the lunation are brought to agree, are detailed in the article CYCLE.

Calibre:—the diameter of the chamber of fire-arms. The word is mostly used in regard to mortars, howitzers, and swivels. The dimen-

sions of cannon are usually indicated by the weight of the bullet.

Calippic Period. See CYCLE.

Calliope. One of the asteroids. For elements, &c., see ASTEROIDS.

Caloric:—a technical term once extensively used in physical science. It was meant to stand in that opposition to, and connection with, *Heat*, that *Cause* does to *Effect*. By *Caloric*, physicists meant the hidden cause of the sensation and phenomena of *Heat*. The term is now dispensed with. The entire theoretical part of the subject will be treated in this Dictionary, under HEAT.

Calorimeter. An instrument intended to measure the amount of heat contained in bodies. Various calorimeters are in use. The one perhaps most simply explained is constructed as follows: Take a cylinder and put in it a known quantity of the hot substance, the amount of heat given off by which it is to be examined. Surround it with another cylinder filled with unmelted ice, which, in order to prevent communication of heat to it from the air, is enclosed in a third cylinder also filled with ice. A pipe from the bottom of the second cylinder conducts the water that flows off. It does not communicate with the water melted from the ice in the third cylinder, although its mouth be placed quite at the bottom of the second, for the ice remaining in the third being lighter than the water in it, this water is kept at the bottom, and is not permitted it to reach the mouth of the tube. The body inside will then be reduced to 32°, and the water that comes out is measured. Now it is known that it takes 141° Fahr. to convert ice into water. If we know then the amount of ice melted, 141 times the number of lbs. in it is the number of thermal units Fahrenheit given off by the body experimented upon. If, further, the number of lbs. in the body be known, we can easily calculate the number of thermal units given off by each lb. of the substance, in falling from the temperature at which it was introduced into the cylinder to 32° Fahr. A lb. of water would fall in that case by as many thermal units as the degrees in temperature, less by 32°—and dividing that result by this, the *specific heat* of the body is obtained. To ascertain this is the ultimate aim of the calorimeter.

This method, however, which Lavoisier and Laplace employed, is found, though theoretically, very excellent, to give practical results far from trustworthy. Another consists in mixing a certain weight of the body, to be examined, with a definite weight of water, and observing the temperature of the mixture. Thus; suppose a pound of metal filings, at 80° Fahr., be set to float in a lb. of water at 100° Fahr., and the resulting mixture be found at the temperature 99° Fahr., this would prove that the loss of one degree of *temperature* from water compensates for a gain of 19° in *temperature* of the mixture. The amounts of heat lost and gained must be, however, the same, and hence it only takes 1-19th part of the heat to raise a lb. of the metal through one degree that it does to raise a lb. of water. The specific heat then is 1-19th. This is the principle on which the process founds. There is also the method of cooling, which gives the most satisfactory calorimetric results of all. The substances are placed within cylindrical vessels, heated to the same temperatures, and allowed to cool down. The cylinder, with a constant form and quality of substance, radiates off the same amount of heat for the same *given temperatures*, so that the time of cooling of the respective bodies through, say 1° of *temperature* will indicate the comparative amounts of heat which each body possessed. If thus, the one takes five minutes and the other two-and-a-half, there is given off twice as much heat by the first body as by the second, with the same lowering of temperature. It takes, therefore, *vice versa*, twice as much heat to raise the first through one degree of temperature as the second. The specific heat is thus found. The reader will readily see that the two bodies will not be *quite* at the same temperatures, at any instant after the first, and the process above, founding on a law that assumes this, will require certain modifications.

Calotype. This name, signifying beautiful picture, is the term chosen by Mr. Fox Talbot to designate the exquisite process of his invention, by which the images of the camera obscura are fixed on paper, from which afterwards other photographic copies are taken, and pictures produced equal in accuracy and effect to those of the celebrated daguerreotype, and, indeed, in some respects greatly superior. The distinguishing peculiarity of the calotype consists in the use of the organic substance, gallic acid, to heighten the sensitiveness of the salts of silver. The general fact that organic substances, when mixed with solutions of nitrate of silver, tend to aid its decomposition by light, has been long known. For example, we may safely expose without change to the rays of the sun a bottle of nitrate of silver dissolved in distilled water. But if the water has been river water, or contains even the smallest quantity of animal or vegetable matter, an immediate decomposition commences, and in a short time a black precipitate at the bottom of the vessel shows us that we have neglected to cover the bottle with an opaque wrapper. Mr. Talbot has the merit of having taken full advantage of this property, and thus rendered an inestimable service to photographic art.

Since the first publication of the process, experience has shown that in some of its details it may be somewhat improved. It will not be necessary here minutely to specify many of these, but rather, to prevent confusion, to confine ourselves to two, which seem to be the best in their effects and the most certain in their results. The first to be mentioned, viz., that in which the

illo-nitrate of silver is used, is the most sensi-ve, and will, therefore, be used for portraits id other purposes where speed is required. The her, wherein the acid nitrate of silver in part kes the place of the gallic acid compound, is ss delicate, and, perhaps, more certain, requiring longer time of exposure in the camera, and is erefore only suited for landscape views and the pying of still life.

The paper selected should be free from spots hen seen by transmitted light; Carson's an-ers well. Being cut to the required size, it is be subjected to the first process, called iodizing. is is done, either by the double process—that by the application first of the nitrate of silver, id subsequently by the iodide of potassium—or the simple process of applying the iodide of ver at once. The latter is now considered the eferable mode; iodized papers are also sold in e shops ready for use. The double process of *dizing* is as follows:—The paper being fixed silver pins to a board slightly larger than self, is held nearly vertical, and brushed evenly id thoroughly from above downwards, with a lution of nitrate of silver, twenty grains to the nce of distilled water. The brush must be rge and soft, and no running or superfluous quid must be left on the paper. It is now lowed to hang and dry. In some respects, a tter process is to employ a larger quantity of e solution, and to float the paper on it in a flat sh, slightly turning up the opposite edges pre-ously, and taking care that the back be kept y. Supposing now that the silver coating has en, by either of those processes, properly ap-ied, by the light of a taper or candle the paper now to be placed with the prepared side on the rface of a solution of iodide of potassium, twenty ains to the ounce of water, with the addition of e grains of common salt, this being poured to a flat dish of the proper size. Care must be ken to keep the upper side dry, and that the wer be thoroughly wetted by drawing it back d forward. It may be allowed to remain on e solution for about three quarters of a minute, a whole minute, but not longer, as the iodide silver would be redissolved. Having dripped the superfluous adherent solution, place the per on its back to allow time for a complete netration, and the thorough change of the trate to the iodide of silver, so that no black ains afterwards may result from neglect. It is w to be floated with the prepared side down-ard, on a vessel of clean water, which easily oistens the whole surface, as it has been placed it when only about half dry. It is allowed remain for ten minutes, in order that the tre which has been formed on the paper, and e remaining iodide of potassium may be washed ay. It is now to be hung up to dry. In is state the paper will keep, if enclosed in book, and light excluded, till the period ien it is to be used in the camera. The next part of the process, which is called *exciting* the paper, is only to be carried out shortly before it is to be exposed—indeed the shorter the better, and in general more than twenty-four hours should not elapse. The room must be thoroughly darkened, and no other light used than that of a candle at the distance of a yard. The exciting liquid called the gallo-nitrate of silver is prepared by mixing equal quantities of the two following solutions, viz., a saturated solution of crystallized gallic acid in distilled water (excess of crystals being always kept in the bottle, and allowed to subside for ten minutes before pouring out), and a solution of 50 grains of nitrate of silver to each ounce of distilled water, to which last has been added $\frac{1}{6}$th of its bulk of glacial acetic acid. Solutions of half these strengths are more certain in their results, though less rapid in their action. This mixture is to be made in such a quantity only as is indispensably necessary for properly moistening the surface of the paper, and is to be used immediately, as it begins in a few minutes to undergo spontaneous decomposition. For applying it to the iodized surface of the paper, either of the following modes may be adopted. A piece of smooth and flat plate glass is to be converted into a shallow trough by having a small slip of paper pasted round it, to prevent any of the liquid running off, if it should happen to come to the edges. This plate being rendered level, the requisite quantity of the gallo-nitrate is to be spread over it with a glass rod, and the prepared paper gently pressed into contact with it, all over, care being taken still to keep the upper side dry. The paper may also be pinned down by silver pins to a piece of wood, and a smooth glass rod laid on it. In front of the rod, and in contact with its whole length, the gallo-nitrate is poured, and is then spread over the paper by gently and steadily moving the rod forward over the surface. As soon as the surface has been wetted by either process, the paper must be immediately removed into a vessel of water, and washed by moving gently, changing the water several times. It must be allowed to drain for a little, and then placed in the camera, or if that is not convenient, it is pinned up to dry, and, while still somewhat damp, put into the slide or the portfolio with care, remembering the tender nature of the sensitive surface.

The time of exposure for this very sensitive paper will, of course, vary with the strength of the light, but in general from thirty seconds to four or five minutes will suffice. On being removed from the camera slide, which must be done only in the feeble light of a candle or a small piece of yellow glass inserted in a window-shutter, no picture will be visible, the process called *development* being necessary. This is effected by the reapplication of the same gallo-nitrate solution, and in the same manner as before. As soon as this is applied the picture is to be held against a

jet of steam issuing from some such apparatus as a common tea kettle. The development must be watched, the high lights appearing first as dark spots, and other parts coming out successively till even the smaller details begin to appear, when it is to be immediately washed by dipping in water.

The picture is now to be *fixed*, or rendered insensible to further change, by steeping in warmish water, renewing it several times—and is then to be pressed between folds of bibulous paper. It is now allowed to soak for about a day in a solution of hyposulphate of soda of the strength of half an ounce to twenty ounces of water. Nothing further is necessary than to thoroughly wash this last solution out of the paper, by allowing it to soak in a large quantity of water for several days, frequently changing the water, until every trace of a sweetish taste is gone, in order to insure that none of it be left. If this be not attended to, the effect will be a slow decomposition of the dark parts of the picture, by which, in the course of time, it will be injured, if not altogether obliterated.

The picture so obtained is what is called a *negative* picture, that is, the lights and darks are reversed, so that another process becomes necessary for again reversing this, and producing the same appearances as in nature. In this case the camera is unnecessary. The negative is placed with its prepared side on the sensitive surface of another sheet of paper; a piece of glass is pressed on both to insure close contact; sunlight is allowed to penetrate the negative and to darken the parts opposite the lights on the second or sensitive surface, while the parts opposite the opaque portions of the negative are protected; and we thus get a reversed copy, with the lights as in nature. This is called printing.

Various modes of *preparing the sensitive paper for printing* have been adopted, in some of which the albumen of eggs is used to give additional sharpness to the impression. The following process answers well. Dissolve fifty grains of nitrate of silver in an ounce of distilled water, and, drop by drop, add strong liquid ammonia, till the liquid, which at first becomes turbid, again clears, taking care not to add more than is sufficient. Prepare the paper by dipping it in a solution of common salt in water, of the strength of two grains to the ounce; press it in blotting paper, and having allowed it to dry, the forementioned solution of silver is to be brushed over it in the manner formerly mentioned in the preparation of the calotype paper, or it may be spread on the glass slab, which is preferable. Allow it to dry, and the paper is fit for use. It is to be placed under the negative, as formerly mentioned, and exposed to light. The progress of the printing must occasionally be watched, by removing the glass and papers into a darkened room, and cautiously lifting the edge of the negative, which is prevented from shifting its place by the finger or a little gum. The process is only to be arrested after the exposure appears to have been somewhat excessive by the darkening, slightly, even of the most protected parts, or what are the highest lights. The subsequent process, of fixing, restores them, having a tendency to lighten the whole picture. It is now to be steeped for a short time in warm water, and soaked afterwards in the same strength of a solution of hyposulphate of soda, as used in the calotype negative, and well washed in a succession of waters, so as to remove the last trace of hyposulphate

The modified process of the calotype, in which aceto-nitrate of silver is used, with only a small quantity of gallic acid—alluded to at the beginning of this article, may now be described. A solution called the aceto-nitrate of silver is to be prepared as follows:—Nitrate of silver, thirty grains; acetic acid (glacial), one drachm; water, one ounce, can be mixed together. The iodized paper, as in the ordinary calotype, is, by either of the processes formerly described, to be evenly wetted with the following mixture, prepared at the moment only in barely sufficient quantity:—into a small glass measure or conical wine glass, in which has been inserted a filter of bibulous paper, about the size of a half-crown piece, drop, so as to filter it, four drops of the aceto-nitrate solution, then removing the filter, add two drachms of distilled water, and then drop in four drops of the saturated solution of gallic acid. This having been spread on the iodized paper, is to be allowed to remain on for fifty or sixty seconds, when it is to be blotted off with a single sheet of bibulous paper. Allow it to remain until half dry, and it is ready for the camera. The time of exposure in the camera may be from four minutes to half-an-hour, according to the nature of the subject and the strength of the light. It is now to be developed, which ought to be done within a few hours after its excitement. This must be carried on, as well as the preparation, in a darkened room, and with no other light than that of a candle. For the development, first, as much of the gallic acid solution must be poured on as when spread with the glass rod, to cover the surface; then pour on about a similar quantity of the aceto-nitrate of silver, and having also spread it, allow the whole to rest during four or five minutes, after which, pour on and spread a little more gallic acid, repeating the same operations till the development is completed, taking care in each of the processes to wet the paper quite to the edge, to prevent warping. It is now to be washed in three or four waters, and afterwards fixed as in the process already described.

Camera Lucida. This instrument is more recent in construction than the Camera Obscura. There are two different instruments to which the name is applied. The first is that of Dr. Hook. It is described as a contrivance for making the

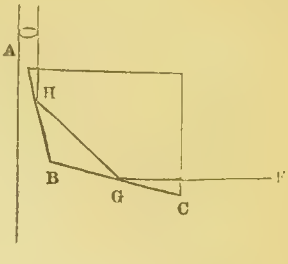
ıage of anything appear on a wall in a light om, either by day or by night. He requires large hole, about a foot in diameter, to be ade in the wall opposite to which the represen- tion is to be given. The object is placed out- le, inverted, and a convex lens also placed ıtside. An image will be distinctly seen upon .e wall, if the object be very powerfully illumi- ıted—and in its proper position. If the ob- ct cannot be conveniently inverted, as may be ıagined sometimes, two convex lenses must be nployed, at distances proportioned to their re- ıective focal distances, and the distance of the ɔject in question. A full account of this instru- ent is given in the *Philosophical Transactions*, o. 38. Its only peculiarity is, that it serves a light room as the camera obscura in a dark ıe.

The Camera Lucida of Dr. Wollaston is an strument susceptible of much more varied ap- .ication. It is intended to facilitate the per- ıective delineations of objects. In its simplest rm, the camera lucida is merely a piece of nooth glass fixed at 45° to the horizon. An ıage from a horizontal object falling on this lass, will be partly reflected, and that in the ertical (according to the principle of equality of ıe angles of incidence and reflection), and an eye ›oking down vertically will see the image, and e able to trace it out upon paper below, from ıe transparency of the glass. As the reflection, owever, is only once here, the image seen on the aper will be inverted. Even in this case, we ın describe the outline with a pencil on the aper below.

If, again, a smooth mirror be held at 22½° the horizon, and a plane glass at 22½° from ıe vertical, an image will be reflected doubly, ill represent itself vertically, and will be shown ı its true position. This, again, can be traced ut distinctly with the pencil.

A practical difficulty, however, suggests itself. he image on the glass, and the pencil print on ıe paper, are at distances sensibly different in ny ordinary instrument; hence the eye, fitted receive impressions from the one, is not so to eceive them from the other. A convex lens is nployed, in order, in effect, to make the image f the glass and that on the paper coincide. he same object is attained by the use of a con- ave glass between the object and the mirror. his latter method is better adapted for near- ighted people.

The most usual form, however, in which the amera is found is that of a prism, which answers he purposes of the mirror and the glass. It is f this form, having the angles which B C make rith the horizon, and B A with the vertical, re- pectively 22½°. The ray F G is not permitted, ccording to the refractive laws, to be thrown rom a rarer medium into a denser, at more than certain angle. Hence it is all reflected from up, on B A at H, and reflected from it again up in the line H E. Now, the prism not being so transparent as the plane glass plate, some means must be adopted of seeing past the prism. One part of the eye, therefore, sees the image at H, and the other part in fact sees the plane paper below. But the laws of our mental nature will not readily permit us to imagine a double sensation corresponding to the true double vision, and the paper thus appears to us to have the image of the object traced upon it. The same methods are applicable here as before, to make the image and the paper coincide as nearly as possible.



Copies obtained by the instrument of any drawings, may be either on a larger or smaller scale than the original. If the copy is to be equal in size to the drawing, the paper must be as far from the eye-piece as the object from the prism, and so in proportion.

The instrument is difficult to use satisfactorily, and many people, probably from some physical peculiarity in the visual organ, are not able to employ it well at any time. It offers, nevertheless, considerable advantages. Its small bulk and portability are remarkable. It is not in the least injured by being made small. A good instrument will pack in a box eight inches by two, and half-an-inch deep. There are, further, no lines distorted, as we shall point out to be always the case with the camera obscura, to a greater or less extent; and the field of observation, generally, may be more than twice as wide as the distinct field of the camera obscura.

Camera Obscura. Of the two optical instruments that are termed cameræ or chambers, this is the more ancient. The first account of it was published in Baptista Porta's *Magia Naturalis*, 17, 6, the first four books of which were published in 1560, although the work was not completed until 1590. The invention has been claimed on very slight grounds for Roger Bacon. The construction which Porta gives is the following:—A circular hole is to be made in the shutter of a window, from which there is a good view of any prominent object, not too near, and in this hole a convex glass, single or double, (fig. 1) is to be placed, having a focus at the distance of from six to twelve feet. It should not be less than three feet at least, or the images will become very small, or greater than fifteen at most, or they will become confused and indistinct, and the colouring faint. Let the room be completely darkened, except through the light so admitted. At the focal distance from the aperture place a piece of paper, very white, and bend it as near as possible to the shape of a segment of a spherical surface, having the focal distance for radius. If

Fig. 1.

this paper now be fixed on a frame of this figure, and attached to a moveable stand, for the purpose of accurate adjustment to distinct vision, the apparatus will be completed. The images of all the objects outside will be seen very distinctly upon the white surface, in an inverted position, but with the greatest fidelity. Every movement in any of them will be beautifully represented on the paper.

The inversion of images is an objection which

Fig. 2.

is readily removed (fig. 2). Or, if a large concave mirror be placed at a proper distance before the picture, or if two lenses be employed, in a tube which draws out, instead of the one described, we shall have the images in their natural position. Fig. 3 will explain the manner

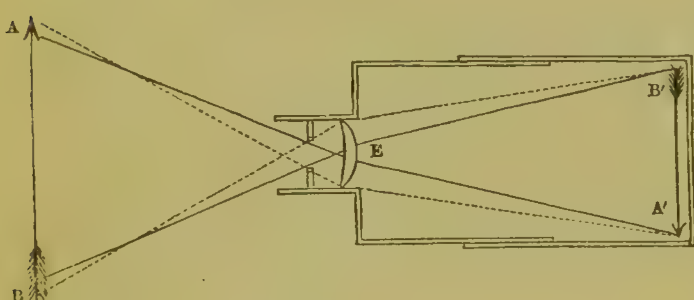


Fig. 3.

in which the image is formed. It is obtained, inverted, at the posterior focus of the lens. The ray A E passes down as A′ E. The effect of the lens so far is a simple concentration of the rays. The circular form of the frame is the most advantageous that can be adopted, in order to have the whole picture as nearly as possible in the focus of the lens. If the paper were plane, the picture would be only distinct in the middle, shading away into indistinctness on the sides. The proportions of its parts could not be preserved. The outer rays would be dispersed more than the inner. The magnitude of the image so obtained will be in proportion to the relation between the distance of the object and the focal distance of the lens. A good deal of light shining upon the object is necessary to render the picture quite distinct, and to represent the colour clearly. The instrument is so interesting that very many

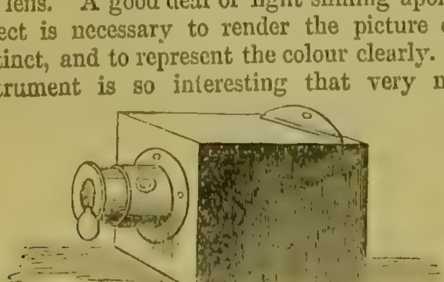


Fig. 4.

portable cameræ have been constructed. The one represented here is perhaps the simplest.

In the modern camera the delineation may be made, as in the original darkened chamber, upon paper bent into a circular form. Dr. Wollaston proposes that, in order to be enabled to employ a plane of paper, a meniscus lens should be employed, instead of the single or double convex, and this would, if the concave side be next the object, produce a greater lengthening of the marginal than of the central rays. This lengthening may be so adjusted to the size of the lens, and the ratio of its radii, as to render it safe to employ a plane surface.

Fig. 5.

The camera was, until recently, considered chiefly as an interesting philosophical toy. Since the invention of photography, it has become of the greatest importance. The accuracy of the pictures depended, in the first instance, on the possibility of obtaining a distinct clearly defined optical image on the photographic material, and as very many objects can only remain at rest for brief periods, this was to become possible in the shortest space of time. The latter requirement is answered partly by the sensitiveness of the chemical preparations spread over the photographic surface—partly by the intensity of the light which the camera can throw upon the object.

The improvements immediately rendered indispensable in the camera by those requirements are pretty evident. The length of the focal distance in the old cameras was not material. In the photographic camera it became of the greatest importance, at once for convenience, and from the necessity of a rapid impression already pointed out, and a concentration of light as great as possible. Achromatic object glasses were at once seen to be necessary to preserve the image clear and distinct; and above all, it became of importance that it should become possible to throw the picture accurately in its correct proportions on a plane surface, even though the field of view was pretty large. To a certain extent the latter is impossible, but science and art together have given a perfection to the photographic camera which could not have been readily anticipated. For the details of the instruments by which these requirements are now sought to be satisfied, see Hunt's *Photography*.

Canals. Artificial courses of water for sanitary, agricultural, or commercial purposes. The first kind are intended to drain marshy ground, or inundated territory; the next to distribute water from an elevated reservoir over the unwatered country; the third and most important, for the transmission of merchandise—sometimes even for the passage of ships. Some of these latter are constructed along the bed of a stream,

just beside it, because very little requires so to be cut through where the stream has worn a passage for itself. They make use of the water of the adjacent stream, merely replacing it, with its natural irregularities, by a perfectly regular stream. The canals which have *locks*, are however, more common, because capable of containing larger supplies of water in general, and requiring no original arrangement of circumstances in a locality, such as it is not always easy to procure. The lock is simply a part of the channel where naturally there would be a fall of water from a descent in the level of the country, and where two moveable folding doors, as it were, are inserted from top to bottom of the canal.

Cancer (*the Crab*). In the division of the ecliptic, Cancer occupies a place between 90° and 120° from the vernal equinox. It contains no very large star. It is surrounded by Hydra, Leo, Lynx, Gemini, and Canis Minor. Its sign as a zodiacal constellation is ♋. It has for nearly 2,000 years, in consequence of precession, not been found between 90° and 120° from the equinox, *i. e.*, coinciding with the sign. See TROPIC.

Canes Venatici (*Hunting Dogs*). A constellation added by Hevelius. On the celestial globe the dogs are represented as held in a leash by Bootes. The constellation is near Coma Berenices and Ursa Major.

Canis Major (*the Greater Dog*). A constellation containing Sirius (the dog-star). The constellation is found by the presence of this star, which is in the continuation of the line through the belt of Orion. The surrounding constellations are Argo, Orion, Monoceros, Lepus, and Columba Noachi.

Canis Minor (*the Lesser Dog*). A constellation situated near Canis Major. The Procyon, of the first magnitude, is its most remarkable star. The constellation may be found by means of this star, which lies in a direct line between Sirius and Pollux. It is also in a line through Sirius, perpendicular to the line from Orion's belt to Sirius.

Capillarity. The manifestation of a description of molecular action between solids and liquids in contact, which disturbs the natural level of the liquid surface. The general phenomena, indeed, are familiar to every student, and may be very easily observed. Take a solid cylinder of glass, and plunge the end of it into water, the water—(provided the cylinder be clean)—will ascend all around it, forming a curve as annexed. If a plate of glass, instead of a cylinder, be placed in the same circumstances, the water will ascend in a curved film a certain way up its sides. If a glass tube of small bore take the place of the cylinder, not only have we the phenomena just described, around its convex surface, but within the tube itself the water rises above its external level by a considerable space. And finally, if two glass plates, fixed at a small distance from each other, have their ends immersed in the water, it will ascend to a certain height between them, and remain there apparently suspended. The quantity of liquid thus elevated varies with the liquid; nay, with some liquids it is *negative*, that is, instead of elevation we have a depression;—if mercury is employed instead of water, for instance, the foregoing phenomena are all reversed.—The study of this curious subject brings before us three distinct classes of considerations, viz., the question as to the numerical laws observed by the general phenomena;—the question as to the physical causes on which these phenomena seem to depend;—and an inquiry into the conduct of different liquids in relation to these laws and causes.

Fig. 1.

I. *General Numerical Laws.*—These laws express the relations between the ascent or descent of any liquid, and the bore and shape of a capillary tube or the distance between the parallel plates that bring out the same phenomenon. We owe the first experimental investigation of the subject to Gay-Lussac. The undertaking was one of great difficulty, alike because of the minuteness of the quantities that had to be handled, and the necessity of securing that these narrow tubes be in a right condition. Nevertheless, this able physicist, through effect of the nicety and judgment so honourably distinguishing French inquirers, succeeded in establishing three fundamental laws:—(1.) The elevation or depression of the same liquid in tubes of the same nature, but of unequal diameters, varies inversely as the diameter of the tube:—(2.) In the case of two parallel plates, separated by a small distance, the elevation or depression also varies inversely as the distance between the plates:—(3.) The variation of level occurring in the case of parallel plates, is half that which is produced in the case of a tube the diameter of whose bore is equal to their distance. (This latter law, however, was presented rather as a theoretical deduction from the two previous ones.)—The recent progress of physical inquiry has not established any truth more important than this—definite numerical laws, deduced from early experiments, and indicated by early theories, ought scarcely ever to be received *absolutely*, or held by otherwise than as valuable approximations; and the subject of capillary attraction furnishes no invalidation of the salutary rule. The foregoing numerical conclusions undoubtedly receive great support from their agreement with the physical theory of Laplace—a theory coincident with that of Young, and resting on the hypothesis that the volume of liquid which is raised above the level, is in proportion to the *contour of the section of the solid wall, whatever may be the curvature of this contour.* Since Gay-Lussac's now comparatively early time, new and extended investi-

gations have been projected and carried out,—among the most successful of which we must signalize those of Simon (of Metz). But undoubtedly our amplest contribution has recently come from M. Wertheim. The first account of his experiments is given in the *Comptes Rendus* for May 18th, 1857; and the superior, if not unchallengeable accuracy of his methods cannot be doubted. Wertheim has arrived at the following important general theorems:—(1.) *Two parallel planes* raise a constant volume, whatever may be their distance, and even when this distance is infinitely great: — (2.) The constant capillarity calculated from experiments with *narrow* tubes, is equal with some liquids, and with others superior, to that determined by means of two planes: it is accidental that with water the relation between these two values is equal to $\frac{\pi}{2}$, as pointed out by Simon: this relation is nearly 2 for chloride of iron, and 1 for other liquids:—(3.) *Wide* tubes give a value comprised between the two preceding ones *when the latter differ*, and equal to them *when they coincide*. This latter takes place when the liquid is *alcohol*. Now this is the only liquid by means of which the theory of Laplace was sought to be verified by Gay-Lussac. Had he made use of water, the formula would have been found utterly in fault; and Frankenheim has subsequently shown that even experiments with alcohol quite deviate from determinations by the formula, when tubes were used half an inch in diameter:—(4.) In proportion as the radii of *convex* cylinders diminish in departing from the plane where this radius is infinitely great, the volume raised continues diminishing for some liquids; for others the diminution commences at a certain limit of curvature, and increases gradually and apparently indefinitely. *Æther* seems to present nearly a constant volume. The presence or absence of viscosity does not affect this constancy or variation. Viscosity retards the movement; but when the equilibrium is established, it is not found to influence sensibly the definitive state. The most important result of Wertheim's investigation is not the mere invalidation of Gay-Lussac's numerical laws; it is that which disproves the hypothesis of Young and Laplace. Wertheim is of opinion that some new element must be introduced;—to which conception we shall again advert.

II. *On the Physical Causes of Capillarity.*—As indicated above, this part of our curious subject has not reached a condition that can be reckoned satisfactory. We shall, however, state exactly what the prevailing speculations have been, and what are their bases.—The received doctrine rests on the assumption that forces of a definite and powerful kind are exercised by molecule over molecule of any fluid mass; and that similar affections, positive or negative—that is, attractive or repellent—regulate the conduct of the molecules of liquids towards molecules of solids, with which they are either in contact, or in a neighbourhood so close that sensibly it amounts to contact. The exact character and the peculiar energies of these forces remain matter of speculation; but over the point as to their reality no doubt can longer rest. Under article FORCES, MOLECULAR, the reader will find a succint and, it is hoped, a satisfactory statement of the remarkable experimental contributions of M. Plateau to our knowledge concerning this almost evanescent subject. Let us see then what can be made out of the unquestioned existence of such molecular forces, in elucidation of these singular capillary phenomena.—(1.) Let a distinct notion be formed of the nature of the disturbing effect as regards the *level*, should these relative molecular forces induce within a tube, or between plates, a *convex* or a *concave* surface upon the liquid therein contained? It is not difficult to make the general case intelligible. *First*, let the surface be convex, as in the diagram:—This can only ensue in consequence of the particles near a and b, and of course in all similar positions, down the tube A B C D, being attracted towards the sides of the tube;—in other words, the phenomenon indicates an excess of action of

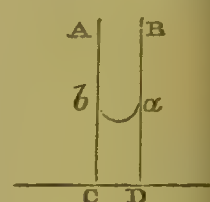


Fig. 2.

the solid molecular forces composing the walls of the tube, over that exercised by the liquid molecules on each other. Now, in the neighbourhood of a and b there is thus induced a lateral pressure on the part of these liquid molecules—deducting in so far (by the principle of liquid equality of pressure) from their downward effect, as determined by their ordinary gravity or weight. The column a b, C D, must therefore be on the whole *lighter* than it would have been but for this disturbance; and, therefore, to procure equilibrium, it must be a higher column than the external one. *Secondly*, if the case were reversed, that is, if the molecule of the liquid were driven off, or repelled by the molecule of the solid wall, its downward force must be increased exactly in that proportion, and the level of the counterbalancing column therefore *depressed*. No geometrical diagrams can offer a plainer account of the physical actions now under contemplation. But the question remains as to the *amount* of the action. As already indicated, Laplace inaugurated his elaborate theory (and Poisson in this has followed him), by the assertion that the elevating power must be the contour of the capillary surface, be that the contour of a small tube or the interval between plates. It is easy to see, however, that a doctrine so general can take no account of the special nature of the capillary or molecular forces: nor does it even really advance science, when general truths are thrown into mathematical forms that become despotic and absolute. The physical problems slurred

ver by Laplace and Poisson,—problems which oncern the whole important part of the question,—are again happily being stirred. Wertheim has felt it necessary to look into the henomena from a point altogether new and resh. He thought at first that these important nomalies might receive their solution in the dmission that the angle of contingence varies vith the curvature of the wall; but this is not he case. "It will be necessary, therefore," he ays, "to have recourse to another hypothesis, nd, I believe, to take into account the variable hickness of the liquid stratum or sheath which dheres to the solid body. This hypothesis has een suggested to me by a series of experiments which I have made upon the elevation of the olution of protochloride of iron between two arallel iron plates, which I fixed to the poles of . Ruhmkorff's electro-magnet. The lower exremities of these plates were immersed in the olution, and the values h and b for the different listances 2 a, were already known. A current vas then passed into the apparatus, of which the ntensity was gradually increased and measured, vhen the magnetic liquid was seen to rise beween the two planes, often to two or three times ts original height, whilst the surface acquired he curvature suitable to this new elevation; but or each intensity of magnetization the volume aised remains evidently constant, whatever be he distance of the two planes; in one word, hings go on as if the constant capillarity had een doubled or tripled. We know, however, rom the experiments of Brunner and Mousson, hat the attraction of the liquid upon itself is not ltered by its magnetization; and, on the other hand, the minuteness of the changes of form which the liquid undergoes when the polar suraces are not immersed in it, and the fact that he augmentation of the volume elevated is indeendent of the distance of the planes, prove that we have not to do with an effect of magnetic attraction exerted at a distance. I think, therefore, that these facts can only be explained by the increase of thickness of the adherent stratum, an increase which may be proved directly. It will be understood, also, that as every change of temperature may cause this thickness to vary, the influence of temperature may be very different from that foreseen by the theory, only taking nto account the dilatation of the liquid."

III. It is clear, therefore, that the entire subject remains unexplored. It is not likely that existing difficulties will be cleared away so long as inquirers follow the old path. It is something, therefore, to find a new one opened.—In a remarkable memoir by Mr. Waterston, printed in the *Philosophical Magazine* for January, 1858, an attempt is made to establish universal relations between the capillarity of any liquid and the latent heat of its vapour;—it following from the modern theory of heat, that this latent heat is the accurate measure of the force of cohesion between the particles of the liquid. This inquirer, after inaugurating a special and quite original mode of experimenting, has examined the capillarity of a great many liquids in relation to this idea, and his results go very far to confirm his views. Indeed he considers it possible to compute the latent heat from the capillarity, and he has given instances, especially in the case of mercury, of the success of that method,—in proof of course of the soundness of its foundations. We may fondly hope that the whole subject will speedily be reinvestigated under a fuller and wider view of its conditions.

Capra (*the She Goat*). A small northern constellation.

Capricornus — *Capricorn* (*Goat-horned*). The tenth sign of the zodiac.

Capstan. A species of the wheel and axle, employed in lifting heavy weights of any sort, especially in the manœuvres of casting and weighing anchor.

Carnot's Function. This important coefficient may be considered as the connecting link of the phenomena produced by heat on all kinds of substances, and may be defined as the coefficient by which the difference between two temperatures if infinitely small (if only a degree Cent., or less, for instance, in practical approximations), must be multiplied to calculate how much of the mechanical value of a unit of heat supplied at the higher temperature, can be converted into work by a perfect thermo-dynamic engine, having its refrigerator at the lower temperature of the two. It is a quantity, expressive of a relationship between temperature and pressure, or between temperature and work, which has an absolute value, the same for all substances for any given temperature, but varying with the temperature in a manner determinable by experiment. It may therefore, as Professor Thomson has pointed out, be used as the foundation of an absolute system of thermometry. Carnot, the discoverer of the function, was not in possession of experimental facts by which he could determine its true form. These, however, have been recently obtained by Messrs. Thomson and Joule, by determining the changes of temperature experienced by air in passing through a porous substance from a state of high, to one of low pressure. They have thus proved that Carnot's function is approximately equal to the mechanical equivalent of the thermal unit, divided by the temperature by the air thermometer from its zero of expansion. Thus, at 0° Cent., Carnot's function $= \mu = \frac{1390}{273 \cdot 7} = 5 \cdot 078 =$ the mechanical effect in foot pounds derivable from a thermic unit Centigrade passing from 0° Cent. to — 1° Cent. For further information, see HEAT, MECHANICAL ACTION OF; also, *Reflexions sur la Puissance Motrice du Feu*, &c., par S. Carnot, 1824; "Memoir on the Motive Power of Heat," by E. Clapeyron, Taylor's *Scientific Me-*

moirs, vol. i., part 3; "An Account of Carnot's Theory," by Professor W. Thomson, *R. S., Edin.*, vol. xvi., part 5; "On the Centrifugal Theory of Elasticity," by Professor Rankine, *R. S., Edin.*, vol. xx.; "On the Dynamical Theory of Heat," by Professor Thomson, part 4, *R. S., Edin.*, vol. xx., part 2, and *Philosophical Magazine*, vol. iv.; "On the Thermal Effects of Fluids in Motion," by Messrs. Thomson and Joule, *Phil. Trans.*, 1854, part 2; "On the Moving Force of Heat,'" by M. Clausins, Berlin, 1850; Holtzmann "On Heat and Elasticity," 1845; and Helmholtz, "On the Conservation of Force," 1847.

Cassiopeia. A constellation on the side of the pole opposite the great bear, marked by five stars of the third magnitude, forming a figure like an M. These stars may be found by drawing a line from Capella to the bright star in Cygnus, which will pass about half-way between them. The figure of the constellation is that of a woman sitting in a chair, with a branch in her hand. In 1572, Tycho Brahe noticed in it a new star, first, on Nov. 11th, at which time it had a lustre greater than that of any of the fixed stars—greater than that of Jupiter, and nearly equal to that of Venus. In January, its light was less than that of Jupiter; in February and March, about equal to that of the fixed stars of the first magnitude. It went on so, diminishing in lustre, until in March, 1574, it disappeared. Tycho supposed that the same star had appeared in the same way in 945 and 1264. The authority for this is not very reliable; and comets appeared about these times, which may have been mistaken for it. Sir John Herschell suggests a possible reappearance in 1872. It was certainly more distant by far than any of the planets, for at no time had it a sensible parallax.

Castor. One of the bright stars in the head of Gemini, from which and Pollux, that constellation is named (*the twins*). It is, of the two, nearest the pole, and among the most remarkable of the double stars.

Catalogue, Astronomical. A list of stars, with their *mean* or corrected right ascensions and declinations for a given epoch. The importance of such catalogues cannot be over-estimated. They form one of the leading contributions of any age to the great subject of the *sidereal motions*. We have several old catalogues of much value; but it is only on documents constructed by aid of the instruments of recent times that future science will rely in prosecuting the more delicate and immense inquiries now opening up.

Catapulta. An ancient warlike engine adapted to throw darts against the defenders of the walls of a besieged city. In its shape and other essential points it was very much like the more recent *cross-bow*. See BALLISTA.

Catenary. The curve formed by a flexible cord, of which two points are fixed, and upon which no forces but gravity operate. Usually the cord is considered as homogeneous. When the density varies in any regular way it takes a shape slightly different from that of the ordinary catenary. The equation of the curve is $y = \frac{m}{2}\left(e^{\frac{x}{m}} + e^{\frac{x}{m}}\right)$. This curve possesses several very remarkable properties. One is that its centre of gravity is lower than that of any curve of equal perimeter and with the same fixed points for extremities. The curve, where the weight is proportioned to a horizontal projection of the part considered, is a *parabola*. This and the ordinary catenary are of chief importance in the theory of suspension bridges (*q.v*). The properties of the catenary will be found admirably discussed in Duhamel's *Cours de Mecanique*, vol. 1.

Cathode. See ANODE.

Cation. See ANION.

Catoptrics. The science of Optics treats of the phenomena of light, and of the physical causes of these phenomena. The former we have reduced to laws, and from these laws we can deduce geometrically effects either already verified by, or, perhaps, anticipatory of, experiment. The question of the physical constitution of light has been long debated, and has only been recently put upon a satisfactory basis. With the phenomena alone, the present article has to do.

The phenomena of geometrical optics are principally three—the phenomena of light passing through a homogeneous medium, or through a vacuum—of light passing from one medium, or from vacuum, into vacuum or another medium—and finally, the phenomena of light incident upon a medium at its surface, and thrown back from its surface as incapable of passage.

The first of these may be treated of under optics properly; the latter two, which form the most important departments of geometrical optics, are generally designated the sciences of *dioptrics* and *catoptrics* respectively. The latter is the subject of the present article.

The first important theorem of catoptrics is this, that the incident and the reflected ray are situate in the same plane with the perpendicular to the surface, from which the reflection is made, at the point of reflection. Thus, if a ray be incident upon a floor, and if we draw a perpendicular to the plane through the point of incidence, and apply, suppose a piece of paper to the two lines, so as just to lie upon them, the reflected ray would be reflected somewhere in the plane of the paper.

This, however, gives us but little information. It tells us something that the ray cannot do,—viz.: turn to the one side or the other; but it does not give any definite idea of its course. This is supplied by the next law, that the course of the line of reflection in the plane so fixed is determined by making, with the perpendicular to the surface of incidence, on the same side with

ie incident ray, an angle, equal to that made : it with the incident ray.

Two definitions which may here be given, will iable us to express this law very simply. The *igle of incidence* of a ray of light is the angle hich that ray makes with the perpendicular to e surface of the substance on which it strikes. he *angle of reflection* is the angle similarly ade by the reflected ray.

The laws, therefore, of catoptrics may be ex- essed thus:—When a ray of light is incident oon a reflecting substance, the reflected ray is the same plane with the incident ray, and the igles of incidence and reflection are equal.

Assuming then, the perfect exactness of these ws, we shall see that catoptrics is not at all oncerned with the non-homogeneity of ordinary ght rays. In other words, though rays do iffer in essential qualities, and though white ght is made up of rays different in nature, it ill not be decomposed or separated by re- ection into rays of different colours. This one w, of the equality of the angles of incidence id reflection, holds for all kinds of light. Hence ys which, though really different, come mingled gether on a surface, will leave it undecomposed. his is a most important result, and at once lieves us from the necessity of instituting in- iiries upon the various reflective (like refractive) owers of different light rays. We shall now oceed to detail some of the more important opositions of catoptrics.

And the first proposition we shall state, is iis; — when a number of parallel rays fall oon a plane reflecting surface, the reflected iys corresponding to them will also be par- lel. This will be clearly seen by any one who ts a number of rods parallel to one another, id inclined to a floor, and then bends them over ntil they shall be again inclined equally to the oor. It will be readily seen to be in strict ac- ordance with the geometrical laws of plane sur- ces, thus:—the planes containing each incident iy, with its corresponding reflected ray, have vo lines, which meet one another in each arallel, namely, the perpendicular to the original lane and the original rays. It follows, there- re, that the planes themselves are parallel. Ience, as the reflected rays make equal angles with these perpendiculars, which are parallel, and in parallel planes, they also are parallel. The figure will illustrate this, but the best illustration may be made by the student himself arranging two rods as di- rected, and fixing two traight rods at the two points of incidence on he floor to serve for the perpendiculars.

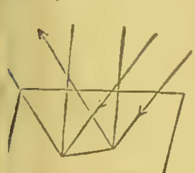


Fig. 1.

Our next important proposition is the following —that if a pencil of light diverging from, or con- erging to a point, fall upon a plane mirror, the re- flected rays will have the appearance of diverging from or converging to a point at an equal distance on the opposite side of the mirror. Thus, let a number of rays, Q A, Q B, Q C, diverge from the

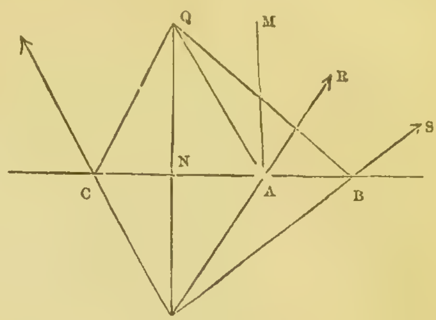


Fig. 2.

point Q, and fall on the mirror at A, B, C, they will appear, when reflected, to diverge from the point q, at an equal distance from the mirror, at a distance on the other side equal to N Q. Draw A M, and let R A be produced through A to meet Q N produced in q. Then $\angle$ Q A N = $\angle$ R A B (by the reflective law), and $\angle$ R A B = $\angle$ N A q (I. 15), therefore, $\angle$ Q A N = $\angle$ N A q. Hence the two triangles N A Q, N A q, have one side common and two similarly situated angles equal, and are therefore in every respect equal— N Q being equal to N q. The reflected ray A R appears, therefore, to come from q. This proof determines, then, the length of N q, and it has been shown equal to N Q by a method equally applicable to the other rays as to Q A. Hence *all* the rays will appear to come from q, and the proposition is proved.

When we have two or more plane mirrors at which luminous rays are successively reflected— as for example, a second mirror, from which the reflected rays in the last figure will be a second time reflected; and a third, from which three will be again thrown back; and a fourth and a fifth, and so on; the point of divergence or conver- gence of the rays after the second reflection will be found, simply by supposing the focus of the first reflected rays to be a real point from which really emanate luminous rays. Reflected light is reflected, just like light coming from a luminous point, and has lost none of its properties in this kind. It has lost none of its component rays, as we have already proved. We can, therefore, apply the principle already demonstrated for un- reflected light, in the case of light reflected from a plane mirror, again reflected in another, and that perhaps re-reflected from a third and fourth.

It is frequently an interesting point to discover the amount of deviation of a ray of light which has been reflected at each of two plane mirrors, in a plane perpendicular to the line of their in- tersections.

Let S A be the ray, so incident on the mirror A, and reflected back a second time from B. The

angle D is called the angle of deviation, and its amount is required.

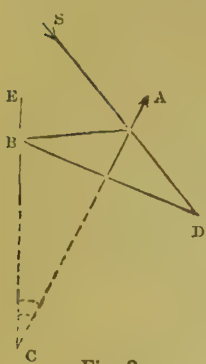


Fig. 3.

The angle $SAB = ABD + ADB$. Now, $SAB = 180° - 2\,BAC$ and $ABD = 180° - 2\,ABE$. Therefore, $180° - 2\,BAC = 180° - 2\,ABE + ADB$. Hence $2\,ABE - 2\,BAC = ADB$, but $ABE - BAC = BCA$. $\therefore 2\,BCA = ADB$. That is, the angle of deviation is equal to twice the angle of inclination of the two mirrors.

An investigation exactly similar would make it easy to find the angle of deviation after three or four, or any number of reflections from plane mirrors—all the reflections being made, however, in the planes, to which the successive lines of intersection of the first and second, of the second and third, of the third and fourth mirrors, are perpendicular.

This proposition has been grounded on the hypothesis that the mirrors are plane. It is evident, however, that as no part but a very small part, in fact, the point of incidence, of the mirror is at all concerned with the reflection of the ray, the proposition would hold equally good whether we have plane mirrors or not, provided only that a very small space, indefinitely small, coincided in direction with the planes, whose angles of inclination we consider in determining the angle of deviation. It is evident that the mirrors may be of any form which will permit the reflection of the rays so, and which will not be so bent as to catch one of the reflected rays again after reflection. We may have them, therefore, curved mirrors, of any form whatever, and the angle of deviation of any ray after two reflections from curved mirrors will be double the angle of inclination of the two planes which touch the mirrors at the points of incidence of the rays upon them. This idea of tangent planes will be readily conceived, if the reader take any ball, and apply a plate of glass or paper, so as just to touch it in a point without being itself bent, or the ball indented by pressure. He will easily see that there is only one direction in which he can apply the plane of the paper or glass to the ball, so that it will just do so, for each point of application, and so, there is only one direction in which he can apply the plane to the curved mirrors already described, and therefore, the angles of inclination of each successive pair of planes are quite determinate quantities.

The law of reflection can therefore be applied to the case of curved surfaces, by considering them as coincident with plane surfaces, tangents to them, or having for an indefinitely small space the same direction as them.

Having established this principle, then, we are able to apply the law of reflection to the case of curved mirrors, and to deduce the various effects certain to result in that case.

We shall just examine the case of rays reflected to one point, and crossing one another's directions in that point. This, though not so important as the corresponding problem in dioptrics, the discovery of surfaces of accurate refraction—at all events, for the purposes of the optician—is sufficiently important for other purposes to deserve attention.

If a pencil of parallel rays fall upon a concave parabolic reflector, in a direction parallel to the axis of the body (the line in its centre is called the axis), the rays will be reflected so that these lines of direction, after it, shall pass through one point. Of this proposition we can offer no proof. It rests upon the assertion, itself demonstrable in the higher mathematics, that the perpendicular to a parabolic surface, at a given point, bisects the angle made by a line through it parallel to the axis, and one from it to a point, fixed for any given parabola, and called its focus.

A similar surface of accurate reflection will be found in an ellipsoidal shell, on the interior of which rays fall, diverging from the one focus of the ellipsoid, by which these rays will be reflected in lines all passing through the other focus of the shell.

Another still might be found in a hyperbolic reflector, from one focus of which rays proceed, falling on the convex reflecting surface, which will send them back to the other focus.

If, in these cases, the mirror be silvered on the one side instead of the other, and if the same rays, continued indefinitely, be imagined to fall upon this new reflecting side, we obtain a series of propositions almost identical with those already given, and having application to convex reflection of the shapes just specified. Thus, if a pencil of parallel rays be incident in a convex parabolic mirror, parallel to the axis, the reflected pencil will diverge from the focus of the parabola.

This proposition, and the one corresponding to it, are by far the most important of those now submitted. They apply to the cases most frequent in actual nature—of solar, astral, and lunar rays. These, coming from a great distance, diverge so little, compared with that distance, that they may be considered as perfectly parallel. If, therefore, a system of parallel rays fall on a parabolic mirror (as for example, any of the rays specified), they will be reflected to the focus, if they fall upon the concave mirrored side; and from the focus, if they fall on the convex mirrored side.

We find thus then points of accurate reflection in various circumstances, and it is clear that, as we are able to describe as many ellipsoids and hyperboloids as we choose when the true foci are given in each case, we might make a mirror of any particular curvature that we might want, capable of reflecting rays of light coming from a

en point on to another given point. But the struction of such mirrors (paraboloids, ellip- ls, hyperboloids) is very difficult, and they therefore very expensive. On the other id, spherical mirrors are constructed with aparative ease. If then, we can obtain such rors capable of producing all the effects due the more expensive kinds, even approximately, will be convenient to adopt them. It becomes, refore, necessary to investigate the properties spherical mirrors.

And first, with respect to the more frequent ugh less general case of parallel rays incident a reflecting surface.

Let O A pass through the centre, parallel to pencil of rays which we consider, and let S P

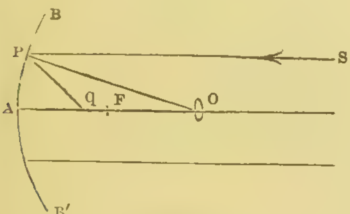


Fig. 4.

one of these rays. Then the tangent to the sur- B A B′, is like all the tangents to circles— pendicular to the radius O P. If, therefore, consider the circle as equivalent for a small tance to a thin plane mirror, coinciding with tangent, on whose surface the incident ray falls, we shall have, by the law of reflection, angles S P O, q P O equal (angles of incidence l of reflection). Now, S P O = P O q. Hence angles P O q, q P O are equal, and the triangle O is isosceles (having the sides P q and O q ial).

We might, by trigonometrical methods, find length of the line O q in any given circum- nces. We shall confine ourselves to some re special cases. If the angle P O A be very all, which it will be, for a given length P A, he radius be very large, the figure P q O becomes y nearly a straight line. The three sides ne to coincide. The proposition just estab- ied does not at all depend, however, upon the e of P O A, and will still hold good, there- e, in the case now imagined. If we suppose triangle becoming indefinitely, or even infi- ely near to a straight line, from the increase O A, or the diminution of P A, the proposition ll continue to be true. In this case, however, sum of P q and q O will be indefinitely near ial to P O, and they may, therefore, each be isidered as equal to its half, or to the half of which is equal to it. Hence the point of arly accurate reflection of parallel rays falling on a circular mirror, having a very small gular range (*i. e.* representing a very small gle at the centre), will be the point of bisec- n of that radius of the mirror which is paral- to the pencil of rays. A F is called the *prin-cipal focal length*, as being the focal length which serves for the greatest number of rays; all of those which correspond in description to S P, with regard to the part of the mirror on which they fall, being equally reflected in directions very nearly passing through F. The line A F then is equal to half the radius of the circle.

If A P represent a moderate angle at the centre O, q O will be the half of the sum q O + q P. This sum is greater than P O (I. 20) or O A, and q O is therefore greater than the half of O A, or than O F which is equal to it. If the angle P O A become larger than 60°, the angle O P A (joining P A) would be less than 60°, and the ray would be reflected on to the mirror again before passing so as to cut O A. The same laws of reflection would apply to this case, which, as specula or mirrors are not usually made which correspond to a central angle of 120° (twice 60°), does not very often occur.

The caustic curve (see CAUSTIC) is an effect of the law of reflection also. When a reflecting curve is exposed to the parallel rays of the sun- light, a luminous space, as in fig. 5, with a curved outline, is made by means of the reflect- ing lines. The space is that on which the re-

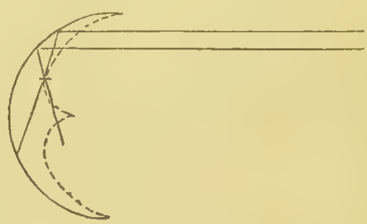


Fig. 5.

flecting lines mostly intersect, and in which, therefore, there is a greater concentration of luminous rays.

The more general case, of the refraction of luminous rays by spherical mirrors, when these rays diverge from a point, remains to be con- sidered. We shall take the mirrors as concave to the ray, and shall find that by altering the signs, and affixing different values for the various quantities specified in the question, we will be enabled to comprehend in the solution of this case all the cases of spherical mirrors.

Let P A P, be a section of the mirror, and let Q O A be its axis, Q being the luminous point, from which diverging rays, Q A, Q P, fall upon the mirrored surface. Let F be the principal focus, as already de- fined, and O the centre

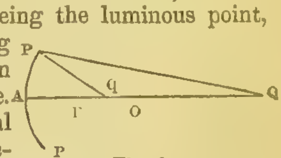


Fig. 6.

of curvature of the surface. Let Q P also be one of the incident, and q P the corresponding re- flected ray.

Then, as before, Q P O is the angle of incidence, and O P q the angle of reflection, and since these are equal Q O : q O : : Q P : q P (VI. 3). This ex- pression is universal, and suitable trigonometri-

cal expressions might be found which would give the value of the distance q o in terms of the angle P O A (the fixed angle of the lens), and of the instance Q O, and the radius O A. We shall limit ourselves to a less comprehensive case. Suppose the arc P A to subtend an inconsiderable angle at the centre, which may be the case either by P A becoming itself very small compared with P O, or inversely by O A becoming very large compared with P A. Then Q P and Q A may be taken as in fact equal, and so also q P and q A. In fact, in that case, each pair of lines approach very closely to each other, and therefore become very nearly equal. The proportion already established, and always maintained, as not at all dependent on the relative magnitude of P A, will now become

$$Q\,O : q\,O :: Q\,A : q\,A$$

Calling Q A $= u$ (the perpendicular distance of the point Q from the mirror), passing in this case through the centre O, and q A $= v$, and O A $= r$.

We get

$$u - r : r - v :: u : v$$

$$\therefore u - r : r - v :: u\,r : v\,r$$

$$\text{and } \frac{u - r}{u\,r} = \frac{r - v}{v\,r}$$

$$\text{or } \frac{1}{r} - \frac{1}{u} = \frac{1}{v} - \frac{1}{r}$$

$$\text{and } \frac{2}{r} = \frac{1}{v} + \frac{1}{u}$$

$$\text{Now } \frac{r}{2} = f$$

Hence, taking reciprocal equality, $\frac{2}{r} = \frac{1}{f}$

$\therefore \frac{1}{v} + \frac{1}{u} = \frac{1}{f}$ in the case when the arc P A, becomes by any method, small in comparison with the radius, and hence subtends a small arc at the centre of the circle.

It is quite evident that as a ray, q P would have been reflected to Q, the same proof, and therefore the same result will apply, if q be supposed the luminous point, to the rays from which, we desire to find the point of accurate reflection.

This expression serves, then, in the case of rays falling upon a concave mirror. If, however, the mirror be turned in the opposite direction, silvered on the opposite side from that where it was silvered before, the radius changes its sign—that is, we might imagine a mirror with a gradually diminishing radius of curvature, having the centre of the circle of curvature constantly between Q and A, and constantly approaching A, until the radius becomes $= 0$, and we may imagine the same process to continue, the centre to keep still moving onwards in the same direction, passing A, and being supposed, in order to suit algebra, which does not take direction into account, to become less than nothing, or negative. Similarly, the point q, going along with the radius, and keeping on the same side of A, makes q A or v also negative, while Q A or u remains as before. In the ultimate equations, however, we do not find r, but f, which is evidently affected by the same circumstances, and of the same sign. Therefore

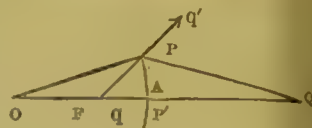


Fig. 7.

$$-\frac{1}{v} + \frac{1}{u} = -\frac{1}{f}$$

$$\text{and } \frac{1}{v} - \frac{1}{u} = \frac{1}{f}$$

The same remark, already made in the case of concave mirrors, that if q be the luminous point, the proof and the result might be alike employed for determining the focus of accurate reflection Q, may also be repeated here.

Taking now the two equations,

$\frac{1}{v} + \frac{1}{u} = \frac{1}{f}$ for mirrors concave to the incident rays,

and $\frac{1}{v} - \frac{1}{u} = \frac{1}{f}$ for mirrors convex to them, let us proceed to examine the various cases which occur:—

Let $u = \infty$ (become infinite), then $\frac{1}{u} = 0$, and in either case $\frac{1}{v} = \frac{1}{f}$ and $v = f$.

That is, that in the case of parallel rays incident on a mirror, either convex or concave to their direction, the rays are reflected *to*, or are reflected so as to appear coming *from* a point in the axis of the mirror, within its completed circle, and at a distance from A $= f = \frac{r}{2}$; as we have already shown in considering this special case.

If u be greater than r, then in the first case $\frac{1}{v}$ is greater than $\frac{1}{r}$ and less than $\frac{1}{f}$ and v is less than r and greater than f.

In the second case $\frac{1}{v}$ is greater than $\frac{1}{f}$ and therefore v is less than f. In the first case, then, q will lie between F and O (fig. 6), if Q lie on the opposite side of O that A does. In the second (fig. 7), q will lie between F and A, if A Q be greater than r. In this second case, if u be any positive quantity, however small, that is, lie at all on the opposite side of A that O does, the same consequence will hold.

If $u = r$, then in the first case, $\frac{1}{v} = \frac{1}{r}$, and $v = r$ (that is, all rays coming from O will be reflected back upon O)—as we should have expected, seeing that the luminous rays, all being radii, will all be perpendicular to the surface of the mirror.

'$u = r$, in the other case, $\frac{1}{v} = \frac{3}{r}$ and $v =$ or $q\,A = \frac{O\,A}{3}$, if $A\,Q = A\,O$.

' $v = r$ this case, in $\frac{1}{u} = \frac{1}{r}$ and $u = r$, or rays are reflected to the luminous point, as re. This case is, in fact, exactly the same ıat in which we have already got the same lt, because the reflecting substance will now concave instead of convex to the incident .

' u be less than r, and greater than f, then $\frac{1}{v}$ ss than $\frac{1}{r}$ and v is greater than r, in the case. That is, if the luminous point come ccupy such a position as q, the point of accu- reflection will pass along towards Q—a re- which we have already seen cause to antici- . In the second case, we would obtain a lt exactly identical with that already pointed when u is positive at all. If Q were in this to change its position, giving a negative line, would be brought back to the case already idered, for concave mirrors.

f, again, $u = f$, then, for concave mirrors, $= o$, and therefore v is infinite. This is pre- ly the application of the proposition that in y case, if either of the points q or Q be the inous point, the other will be that of accurate ection, to the case of parallel rays—for the re- ed rays, converging to a point at an infinite ance, are, in fact, parallel. If $u = f$, in the of convex mirrors, the same result will be ined as is got when u is taken positive. If $= -f$, for this case—we are brought back to case of concave mirrors.

f u be less than f, then $\frac{1}{v}$ is negative, and v lso negative in the first case. That is, if Q . 4) be situate between F and A, the rays will eflected as if diverging from some point on side of A, opposite from F and A. If (fig. 7) e less than f, we have precisely this case, and be less than f, we find a particular case of u g positive—a supposition already examined.

f $u = o$, then, in the first case, $\frac{1}{v} = -\infty$, if $u = o$ in the second case, $\frac{1}{v} = \infty$, and $= o$.

f u become negative in the first case, we ert to the case of convex mirrors, and in ordance with that, find the formula giving v positive and greater than f. If it become ;ative in the second case, we obtain the for- læ for concave mirrors instead of convex ones. t is interesting to deduce the expression for the case of plane mirrors from the formulæ just found. They may be considered as small parts of circles of no curvature, and infinite radius, because it is quite evident that the nearer a circle becomes to a complete plane in any large part of it, the larger its radius must get. Suppose, then, $r = \infty$., then $f = \frac{r}{2} = \infty$ also, and $\frac{1}{v} + \frac{1}{u} = \frac{1}{f} = \frac{1}{\infty} = o$, $\therefore \frac{1}{v} = -\frac{1}{u}$ and $v = -u$ that is, the point of accurate reflection is on the opposite side of the mirror from the point of divergence of the rays, and at an equal distance from the mirror. We should have had the same result by taking the other formula for convex mirrors $\frac{1}{v} = \frac{1}{u}$ and $v = u$; seeing that the positive values of v and u are now obtained by taking them on the opposite sides of A.

It is important to notice that in every one of the special cases considered, it was only necessary to take u as positive in the one case, and negative in the other, to make the two cases coincide. In fact, we have seen that the two cases differ by having the signs of two of the quantities different. When the sign of the other also becomes different, we get a formula something of the form $-A = -B$, which is readily convertible into $A = B$, the original expression. Taking it geometrically, we see, indeed, the same thing. The whole system of mirror, rays, luminous point, is merely transferred to a new position in space, and as physically it makes no difference in the results what position in space a system may occupy, we should expect this total change to restore the exact equivalence of the formulæ. A physical change would be generally wrought in the results of any system, by some relative alterations of positions; but when the whole position alters, maintaining all the while its internal relations, there is no difference of results.

An important proposition, which is of great value in enabling the student immediately to deduce many of the results just given, and in concentrating them into one, is the following. The focal length is a mean proportional between the distances of the conjugate foci from the principal focus

Taking the proportions already obtained—

$$u - r : r - v :: u : v$$

$$\text{or } u - r : u :: r - v : v$$

we get, by composition and division $2\,u - r : r :: r : 2\,v - r$

$$\therefore \frac{u - \frac{r}{2}}{\frac{r}{2}} = \frac{\frac{r}{2}}{v - \frac{r}{2}}$$

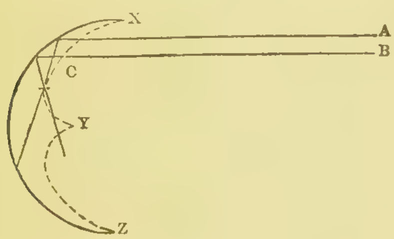
$$\text{or } \frac{F\,Q}{F\,A} = \frac{F\,A}{F\,q}$$

and therefore $F\,Q \,.\, F\,q = F\,A^2$

If we remember that, as has been shown in the various cases which we have specially examined, the conjugate foci always lie on the same side of the principal focus, the proposition will enable us in any case very speedily to arrive at the results which we may desire.

It is essential to remember that in all the cases just examined, and in all the propositions now established, the arc of the mirror is considered as being very small, compared with the radius of the arc. If it be not so, these propositions will not hold good, and, in fact, there will be no point of accurate reflection.

Caustic. When a number of rays from a point are reflected or refracted from a reflecting or refracting surface, the line which would be made up by the series of intersections of the successive reflected or refracted rays is called the *caustic* curve. For example, if a series of luminous rays, sent from a point so distant that the rays are practically parallel, be reflected from a semicircular rim, the curve X Y Z represented in the figure will be the *caustic* curve. Suppose two



very near rays, such as the rays A and B, intersecting each other when reflected in the point C, then the curve is the line made up by an infinite number of such points. It will be practically exemplified if we place a semicircular rim of tin upon a table exposed to the sun's rays. The curve will be the line of division between the bright space on the table and the dark one. It will, indeed, be evident, by producing the reflected lines, that no point beyond the line of the curve can get more than half of the light reflected from the one-half of the semicircle, while points between the rim and the curve are exposed to the light reflected from the whole.

We may have a caustic formed in the same way for any reflecting or refracting surface, exposed to light from points situated in any position. The theory of caustic curves is very interesting. It was first discussed by Tschirnhausen. Malus treats very fully of it in his *Theorie de la Double Refraction.*

The special name given to the caustic of reflection is *catacaustic*, sometimes simply *caustic*, and it is this that is employed above;—the curve of refraction is called the *diacaustic* curve (*q v*).

Centaurus (*The Centaur*). A southern constellation which scarcely rises in our latitude.

It is situate near Ara, Lupus, Libra, and Virgo. The stars α Centauri and β Centauri are of the first magnitude. Sometimes it is made one constellation along with Lupus, and it would appear that various of those which we have mentioned have been united with it.

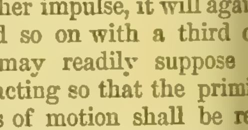
Centering. The subordinate arches of wood or slight stone-work, employed for the convenience of the architect in laying the materials of our larger bridges, are called *centering.* They depend equally with the bridge upon the mathematical principles of the arch. See BRIDGE and ARCH.

Centigrade. See THERMOMETER.

Central Forces. A body under the action of any one force moves uniformly in a straight line. The law of *Inertia* declares that a body moving so, will, if not interrupted, continue to move so for ever. If then a body move either with changes of velocity or changes of direction, there must be some new force, the introduction of which has produced these. If, for instance, a body moving at a uniform rate increases or decreases its velocity—as a steam-engine—we know that additional force has been exerted in the one case, and that contrary forces have been brought into play in the other. If, again, as in a ship tacking, or a projectile falling, a body change its direction of motion, there must have been a like introduction of new forces. The theory of central forces in its elementary stage treats chiefly of the latter case.

Suppose that a body, moving along at any rate, receives a shock; it will proceed to move in a new direction (the new direction being a straight line, though not the same as before). If, again, it receive another impulse, it will again change its direction, and so on with a third or fourth impulse. We may readily suppose a series of such impulses acting so that the primitive directions and lines of motion shall be restored, and that the body be thus made by the compounding of each successive motion with the result of each new impulse, to describe the circumference of a polygon. If we suppose these impulses communicated in a very rapid succession, we shall have a polygon with a constantly increasing number of sides, and if we suppose them added at infinitely small intervals, we shall have a curve. All motion then, in a circle, depends upon such a communication of successive impulses at infinitely small intervals of time—each impulse driving the moving body in a direction different from that in which it moved previously.

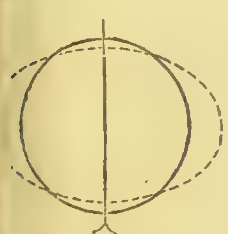
In some cases of curvilinear motion, indeed, no such impulses are at first discerned. If, for example, a bullet be attached to a string, and made to rotate round it as in a sling, there is a curvilinear motion produced, yet we only see at first sight the one force of the *impulsion* which we give the body. If, however, we increase the velocity of the bullet or its weight, we shall find that the string is broken. Now, the bullet moves



·endicular to it in every case, and as it quite free so to move, we should not ːct that this impulse had been the cause of breaking. In fact, at every point, the ball move perpendicularly to the string, but it did continue to move in the line thus given it at point, but diverged towards the centre. The at A would have moved along to C in a given ː, whereas it does move only to D. Supposing ı the lines considered to be very small, there ːt have been a downward force acting upon string, and pulling it to the centre equal to The string remaining straight, there has ı an equivalent force pulling the string from centre called into action. This force is the ːion of the string; and simply means the c, which its cohesive properties enable the ng to resist, without being broken. If this e A E be so great, that the string cannot sus- ı the opposing force of its equivalent, the ng breaks, and the body flies off. This then ːalled the *centrifugal* force. The *centripetal* ːe is simply the adverse force which acts ·n the body to prevent its moving along the gent A C.

[The *centrifugal force* (as it is called) varies as square of the velocity of the body at the ment, and in the inverse ratio of the distance n the centre. (In the case of bodies moving ːurves not circular, the centre spoken of is that a circle which would just touch the curve at point.) As the velocity at any point varies the radius inversely, the centrifugal force ·ries as the inverse cube of the radius.

An interesting illustration of the centrifugal ːe will be obtained, if we set a circular ring of ·ing steel upon an iron bar, which is free to rotate in its socket, and cause violent revolution. The figure will represent the result. The middle and extremities of the circle perform their revolutions in the same time. Hence the velocity, and therefore the centrifugal



ːe is very much greater at the former than at the ːter points. The spring then tends to whirl off the ː, and most where the centrifugal force is great- . There is thus a pressure generated, which ·lges out the ring as represented. This is shown a large scale in the great terrestrial inequality the polar and equatorial diameter. The line of ː poles corresponds to the line of the rod, and ː ring may represent a diametral section of ː earth's mass. The centrifugal force is at no int sufficient to overcome the attraction of the rth's mass, and so all motion must be along its rface. The centrifugal force, however, at the ·les is nothing, or very nearly, and there is erefore a pressure towards the centre due to the ·ole attraction. At the equator, again, there is a very great centrifugal force, arising from the earth's rotation round the poles. A particle at the equator describes its 25,000 miles in the same time as one near the poles describes one mile of circuit. Hence, not the whole attractive force, but that, less by this centrifugal force, presses the mass at the equator to the centre. The earth, then, is as a ball of soft matter unequally pressed; it will be squeezed out in those places where the pressure is least. Therefore, it is higher at the equator than at the poles. If the velocity of the earth in its rotation round its axis were about seventeen times as great as it is, this effect would become so marked that a body at the equator, not retained by cohesive force at the surface, would fly off. There is a diminution of rather less than $(\frac{1}{17})^2$ in the weight of a body at the equator and at the poles. See EARTH, DENSITY OF THE, and EARTH, FIGURE OF THE.

Centre of Gravity is that point of a body which, being supported, the whole body will remain at rest, although subjected to the action of gravity.

In this definition, it is assumed that such a point does exist—that there is some point in every body, or which may be supposed connected with it, which, being supported, the whole will be so also. This assumption is made upon the following ground:—All bodies may be imagined to consist of a large number of particles of matter, connected by cohesion. Suppose, then, to take the simplest case, a body composed of the

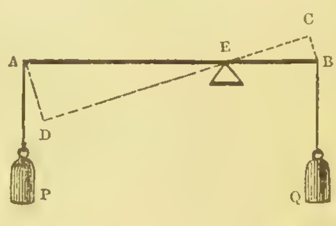


Fig. 1.

two particles A and B, connected together, and both acted on by such a force as gravity. Then, as gravity may be supposed to act for any average distance A B, constantly in the same direction, imagine A P and B Q to represent its line of action. The force upon A and B will be proportional to their masses, and therefore they will just fall with the same speed, and preserve throughout their relative position. B might be considered as made up of (say) four pieces, each equal in mass to A, and each piece would then be acted on by a force equal to that upon A, and the four pieces, in falling, would move all together, and would move just as A moves. Hence, if A B be a material connection, suppose an infinitely thin rod, so as to have no sensible weight of its own, it would not be at all twisted from its original position with respect to the horizon: so that exactly the same motion would be produced upon a mass equal to A and B together,

by a force equal to the sum of those acting upon A and B. If we suppose, then, a force equal to that with which gravity would act upon A and B together, acting somewhere and producing equilibrium in the whole system,—we shall obtain that which we can substitute for A and B separately. Now, it is evident that this force must act between A and B. In any other case A and B would fall round the line of its direction. If B be the heavier (say four times), it will have four times the tendency to fall round the line of force in any case that A has; but the velocity which gravity produces is the same for A and B. Now, in order that the line A B may remain steadfast, the spaces described by B and A in falling round the line of application of this force, would be just in proportion to the lines B C and A C (C being its point of application). Hence, A C must be just in the same ratio to B C that the force applied to B is to that at A (see LEVER); and if it be so, equilibrium will be produced. If, then, the point C be supported, there will be no motion, and the actual motion of A and B might be replaced by a motion of their sum at the point C, under the gravitating influence due to the united weight.

In all this we have not required to consider what position A B occupied with regard to the direction of gravity. It therefore comes to the same thing, whether we imagine it oblique or not. If then, we have merely A and B, with the same weight at the same distance, C will always be their centre of gravity. There does exist, therefore, in the case of *two* material points connected into one body, a point between them which, being supported, the system remains at rest, and upon which the strain is equal to the combined weights.

But as the same thing may be shown regarding any number of points, it may be concluded that in every given body there exists a point which can be determined, (and which remains in the same place *in the body* as long as the body remains the same, whatever position it may assume,) at which the gravitating forces which really act upon all the individual points of the body may be supposed concentrated, when we wish to consider the body either in motion or at rest. In fact, we may suppose, (theoretically) the body removed, and a heavy material point weighing the same as the body substituted in its place.

The stability of a body (see STABILITY) in its position of equilibrium is that property by which, if disturbed, it tends to return to rest in its original position. If the centre of gravity be supported from above, any disturbance would cause the body to oscillate like a pendulum round the point of support, ultimately, (from friction, resistance of air, &c.,) coming to rest. If the point of support be very far from the centre of gravity, the body will rapidly return to comparative rest, under the same disturbing forces as would have caused a protracted motion had the two points been near. If the support be obtained by holding the centre of gravity itself, as by pushing a fixed rod through it, the equilibrium will be preserved in any position which the body may assume. There will be no tendency, in fact, to fall round the point of support. If, again, that point be below the centre of gravity, as when a body rests upon a table, then the equilibrium will be unstable, if there be only one point upon which the body rests. If the base be considerably extended around it, small disturbances will remove the centre of gravity, indeed, but the vertical through it will still fall within the base, and gravity will restore the body in some cases to its original position, in some not. If a vertical through the centre of gravity fall within the base, however, there will be equilibrium. This is strikingly illustrated by the hanging tower of Pisa, a building like that below, with its centre

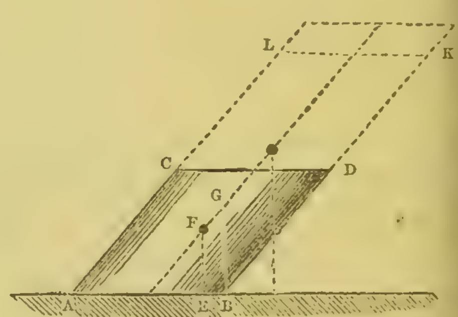


Fig. 2.

of gravity as shown, and the vertical through it, falling within the base. If we suppose another storey added to the tower, the centre of gravity might be conceived to be raised till the vertical would just fall at the edge. In this position there would be equilibrium, but the least horizontal force coming against it would upset it (a slight breeze for instance). If another storey be added to the tower, the vertical from the centre of gravity would no longer fall within the base, and the tower would fall. We can explain these successive results by supposing the tower removed entirely, and the weights collected at the various points which are successively the centres of gravity.

It remains to describe the method by which the centre of gravity may be found.

In regular bodies it is very easy to see where the centre of gravity will be. Thus, in a circular or square plate of homogeneous matter, the centre of gravity will be the centre of the figure, or rather, if we imagine it cut in the middle by a section parallel to the two surfaces, it will be the centre of that section. In a homogeneous cube or sphere, the centre of the figure would again correspond with the centre of gravity. In short, wherever we have a regular figure made up of a series of pairs of points, corresponding in weight and in distance from a point of the

re, that is the centre of gravity. This con- ion, it will be evident, includes bodies not e homogeneous. A sphere, for instance, made f concentric shells might be composed of any l of matter, and the shells of any different ls of matter, such as iron, brass, tin, &c., if shell remained quite homogeneous.

sually, the masses with which we have to are not so constructed. If their surfaces orm to any law, and their density varies rding to any other law, depending upon tion, we may suppose mathematical solutions he problem. If, as in most ordinary bodies have a mere agglomeration of particles, we ire to determine the centre of gravity by ex- ments in each case.

et the body be suspended by a string, and wed to find its position of equilibrium. Then, ve have already pointed out, the equilibrium st be due to the action of some force counter- ncing gravity. That force is the tension of string, and this must therefore be directly osite to the line in which gravity acts on the y. This line passes through the centre of vity, and so therefore must the direction of string. Mark the direction of the string ugh the body. Suspend it by another point, we shall have a second line of direction sing through the centre of gravity of the body. centre of gravity is therefore a point through ch both of these lines pass. But as the lines only both pass through *one* point, *that* point st be the centre of gravity.

t is quite indifferent what points we select as se by which we may suspend the body. There nly one centre of gravity in every body, and lines will always intersect as we have shown hat one point.

f finally, we have two or three bodies united a system, and of each of which, perhaps from ir symmetrical figures, we know the centres of vity, we may find that of the whole system a method already indicated. Imagine the ies removed, and heavy points with weights al to them, placed at their centres of gravity. en the centre of the first two will be found by iding the line joining them into parts in the o of the weights, the lesser being next the ater weight and *vice versa*. We can suppose m removed now, and a mass equal to their placed at the point of division so found. is combined in the same way with the third ld give the centre of gravity of the three, and milar process might be pursued for any number, il we obtain finally the centre of gravity of system.

Centre of Gravity; *Conservation of the tion of.* One of the most important among eral theorems in Rational Mechanics; first nonstrated by Newton in his fourth Corollary the Third Law of Motion, and afterwards eralized by D'Alembert. It may be expressed follows:—*The mutual action of the different bodies of a system on one another, cannot, whatever be the nature of that action, alter in any wise the state of the centre of gravity of the system;* so that, if the only accelerating forces influencing the system be these reciprocal actions; or, what is the same thing, if the external forces affecting it be not *accelerating*, but *instantaneous* forces, then *the centre of gravity of the system must either remain motionless, or move in a straight line.* The singular advantage of the foregoing theorem is this;—however complex the system may be of which one is treating, or whatever the form of the body whose motions are being investigated, the problem concerning the motion of its centre of gravity is as simple as if we were treating the elementary case of the motion of a *point.* And as all movements of *translation* appertaining to any system, are virtually movements of its centre of gravity, it follows, that when considering any such problem, we may neglect those effects of the mutual action of the bodies composing it, the determination of which generally constitutes the chief difficulty of Dynamics.

Centre of Gyration is the point at which, if the whole mass of a body rotating round an axis or point of suspension were collected, a given force applied would produce the same angular velocity as it would if applied at the same point to the body itself.

Suppose, for example, a wheel pushed in any direction by a certain force, it will only act originally at the point of application. By virtue of the rigidity of the body, however, its action will be transmitted to the whole wheel, and a certain motion produced. Now, if we imagine the mass of the wheel collected at the centre of gyration, and if we imagine the same force acting on the same point, which now must be supposed to be without weight, but rigidly connected with the mass at the centre of gyration, the motion produced would be exactly the same as that of the point corresponding to this in the wheel. The angular velocity is uniform for the whole wheel, and is the same as the angular velocity of the centre of gyration in these circumstances.

The centre of gyration bears a strong analogy to the centre of oscillation. It differs only in this, that whereas, in that case the operating forces are supposed to act at every point of the moving body, in this there is only one force acting upon one point.

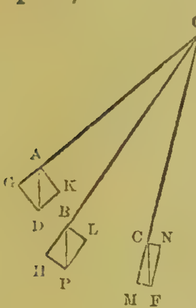
It is found by the following rule:—Divide the moment of inertia of the rotating mass (the sum of the products of the weight of each point of it by the square of the perpendicular distance of that point from the axis of rotation) by the mass of the body, and extract the square root of the quotient. This will give the distance of the centre of gyration from the axis.

The centre of gyration of a straight line revolving round one of its extremities is at the distance $\sqrt{\frac{1}{3}}$ (*i. e.*, 5·8) of the length of the line from that extremity.

The plane of a circle revolving about the diameter has its centre of gyration removed from the centre by one-half of the radius.

The distance of the centre of gyration from the axis of motion is always a mean proportional between that of the centres of gravity and oscillation. Any two of these, therefore, being given, the other may be determined.

Centre of Oscillation. If we conceive a set of points, such as A, B, C, connected by infinitely light though rigid rods with the point O, and allowed to vibrate as pendulums round that point, we shall have the several motions commencing with different velocities. Gravity tends to impress upon all bodies upon which it acts *freely*, equal velocities, and thus A, B, and C, if left to themselves, would fall through A D, B E, C F in the first second, these being all equal. Now, according to the proposition of the parallelogram of forces, we may consider these motions as each originated by two forces instead of one—the first pair being such as to produce a motion of A G and A K respectively in the first second, A G being a continuation of A O, and K G, a rectangle, having the diameter A D. So the other forces could also be decomposed, giving two forces which may be represented by B H, B L, and C M, C N, (L H and M N being rectangles.) Now, of the two forces in each case, that which acts along A G is destroyed by the resistance of O, and A K alone is left free to remove A. Similarly B H and C M are destroyed, and B L and C N alone left to move the points B and C. Hence A, B, and C, instead of moving through A D, B E, and C F, in the first second will move through A K, B L, and C N respectively. It will be evident at once that, as we approach the vertical O P, the motion will grow less and less, and at O P, in fact, all the motion would be prevented by the resistance of O. If we imagine now the points A, B, C, to be rigidly connected, some of them must slacken and some quicken their motion. The point A will be retarded by the point C, which does not move so rapidly of itself, and the point C again will be pushed onwards by the point A. The body, however, does move, and with a certain velocity. We can suppose a point S, which would move with the same velocity, accompanying the body stroke for stroke, and if this point S were free, rotating along with the body and the point immediately beneath it upon the body marked, it would be found that the relative positions of S and of that point would not alter. If, again, any mass were attached there to the body, suppose a heavy ball compressed into a point, the motion of the body composed of A, B, C, &c., would go on just as before. The point S is called the centre of oscillation.



It is most readily found in any given case by the employment of the methods of the calculus; we shall not attempt to give these here. It is always farther from the point of suspension than the centre of gravity is, and always in the straight line joining these two points.

It is necessary to find it, in order to arrive at an exact means of reducing a compound pendulum to a simple one, to which latter many readily understood laws apply.

The centre of oscillation of a given body is found by letting the body come to rest, when we can draw a vertical line in which the centre of gravity must be found, and in which the centre of oscillation, therefore, also lies. To find, then, its length from the centre of suspension, we divide the *moment of inertia* (the sum of the products of the mass of each particle of the body by the square of its distance from the point of suspension) by the product of the mass of the body into the length of the line from the centre of gravity, to the point of suspension.

The centre of oscillation of a straight line or a cylinder suspended at one end, will be distant from that end by $\frac{2}{3}$ of its length, while the centre of gravity is distant $\frac{1}{2}$ of its length. The distance in an isosceles triangle of the point of suspension (the vertex) and the centre of oscillation will be $\frac{3}{4}$ of the length of the perpendicular from the vertex upon the base. The distance of the centre of gravity from that point will be $\frac{2}{3}$ of the length of that line.

Centre of Percussion is that point of a moving body at which the impetus of the body is supposed to be concentrated. If we imagine the body perfectly rigid and non-elastic, then when the centre of percussion strikes against an immovable obstacle, repose should be produced. If the body strike at any other point but the centre of percussion, there would be a sort of rotatory motion round the fixed point. The impetus of some of the points would, in fact, not be destroyed, and that of others would be not only destroyed, but an impetus in the other direction imparted.

If, again, the body move round an axis, as in the case of a body suspended from a point, and strike against the centre of percussion, there will be no strain upon the axis or the point of suspension. If it strike against any other point there will be a certain strain.

If, for example, a ball swinging round from a fixed point strike against an immovable point at its lower part, the velocity of the upper will not thereby be destroyed, but it will move onwards as far as it can, until the rigidity of the body and its connection with the point stopt prevent it, and then it will pull at the point of suspension in order to continue its motion.

There is a different centre of percussion for each of these two cases. When the body is moving freely, and gravity therefore acting freely in whatever direction the body may be moving in

centre of percussion is the same as the centre ;ravity. This supposes that there is no rota- ; motion combined with the rectilinear motion he body. In that case it is not acting freely. there be none, then whatever force acts upon h particle of the body acts uniformly like that :ravity, and acts in a constant direction. Hence, ording to the parallelogram of forces, the force ıpounded of gravity and the impelling forces l act uniformly also. If the body move nd a point or an axis of suspension, the centre percussion becomes the same point with the tre of oscillation, and is to be found in the ıe way.

t is necessary only to remark, that a ring body will strike the hardest blow upon opposing obstacle at the centre of percussion. that point there is no interior strain on the ticles of the body, and none upon the point or s of suspension, in which, part of the value of :moving force would be consumed. The whole e acts upon the obstacle. Thus, since the tre of oscillation of any straight uniform stick t ⅔ of its length from the point of suspension, hardest blow with a stick, may be given by ing an obstacle with it at that point.

Centre of Pressure. A fluid enclosed in a el exerts a certain amount of pressure against sides. It is manifest that this will be less ny point than at one lower down, because the ıle weight of the body immediately between points presses on the side of the vessel at the er in addition to the weight at the upper. The ciple of the equality of pressures, essential in DROSTATICS, (*q. v.*) at once establishes this. have therefore the simple problem of parallel es to deal with. So many forces acting against vertical side of a vessel in horizontal directions, required to know their resultant—if there be capable of representing all of them—and its ıt of application. It is evident that, as the es increase as we go downwards, the point of ication will be nearer the bottom than the and, therefore, always lower than the centre ravity. The value of the resultant forces will he sum of all the pressures. It is not ne- ary, of course, that the sides of the vessel be zontal.

he point is called the *centre of pressure* or *centre.* In the case of a vessel with a paral- gram for one side, the centre of pressure will t the distance of one-third of the height from bottom. In the case of a triangular vessel, se base is at the bottom, it is one-fourth of height only.

s the centre of pressure is the point of appli- on of the resultant of all the pressures, a force lied in the opposite direction, and of the same unt, would neutralize these. This will sug- ; a practical method of determining centres of ssure.

Centrifugal Force. That by which a body olving about a centre has a tendency to re- cede from it. It is simply the force opposite to the tension of the string or the attraction towards the centre. See CENTRAL FORCES.

Centripetal Force. That by which a body revolving about a centre has a tendency to move towards it. Thus the tension of a string by which the revolving body is held, or the attrac- tion resident in the centre and drawing the re- volving body towards it, is the centripetal force. See CENTRAL FORCES.

Centrobaric. A word not much used now. It is intended to indicate the following principle: Every figure, generated by the motion of any line or surface, is equal to the product of the generating magnitude, by the length of path of the centre of gravity of the line or surface. This is merely a translation of the maxim that any body may be theoretically considered to have its mass concentrated in a heavy point, at its centre of gravity. In that case the figure generated by the body, would be measured by the length of the path of the centre of gravity, multiplied by the quantity contained in the body. In the original case, we have to consider, the different points of the body, as perhaps taking different paths. The centre of gravity is the point round which these differences *compensate* one another.

By aid of this principle, when the centre of gravity of a line or a surface is given, we can determine the content of the body or surface generated. When—as in a cylinder—the centre of gravity is readily inferred from the sym- metry, this method of finding the content is very convenient.

The theorem is mathematically demonstrâted by Guldinus, and goes by his name.

Cerberus. A small northern constellation near the hand of Hercules. It contains only four stars.

Ceres. One of the asteroids discerned first by Piazzi at Palermo, on January 1st, 1801. While looking for one of the stars of Lacaille's catalogue, he noticed a star beside the one he expected; and by next night this had changed its place, continuing to do so on the successive nights. He was interrupted, however, by sick- ness, and when he recovered, the planet had become invisible in consequence of its approach too near the sun. He communicated the discovery, therefore, and a number of observers having cal- culated the approximate orbit of the planet from his few observations, watched for its reappearance. De Zach, on the last day of that year, and Ol- bers, on the first day of the succeeding year, re-discovered the planet. It had been supposed to be a comet at the first. It looks like a star between the seventh and eighth magnitudes, and is therefore invisible to the naked eye. Its mag- nitude is less than that of our moon. For the elements, &c., see ASTEROIDS.

Cetus (*The Sea Monster.*) A constellation which received the names also of Pistrix and

Orphus. It is below Pisces and Aries, and is usually drawn with a fish's head, two paws in front, and a curled fish's tail. Its brightest star is called Menkar (α, Ceti), and is of the second magnitude. It is in the southern hemisphere.

Chains. For the principles of the equilibrium of Chains see ARCHES.

Chameleon. A southern constellation containing nine stars. It is situated on the colure of the equinoxes within the antarctic polar circle.

Chances, Doctrine of. A most important division alike of speculative and practical science. See PROBABILITIES.

Changes. See PERMUTATIONS.

Charge (*Electrical*). See BATTERY.

Charles's Wain, is a name given by some of the older astronomers to the constellation Ursa Major (*q. v.*) It was formerly called in England the "brood hen." Both the Romans and the Arabs called this constellation a wain or waggon.

Chart. A map of the sea, for the use of navigators, showing the relative positions of rocks, shoals, harbours, &c. See PROJECTION.

Chromatic. A series of sounds each one-half of a note distant from the other, is said to be arranged according to the *chromatic gamut* or *chromatic scale*. The name *chromatics* was applied by the ancients to that species of their music which divided each note into three lesser ones.

Chronograph. See CHRONOMETER.

Chronology. The science which treats of the divisions of *time*, arbitrary or otherwise. What we call arbitrary divisions, are such as date generally from some remarkable historical event. What we call non-arbitrary divisions are such as commence along with some marked phenomena in external nature. In fact, the one and the other are arbitrary; but the divisions of time into years, days, months, &c., being simultaneous with the period of complete evolution of a *recurring* phenomenon, are not of an arbitrary character. For the chief of these see BISSEXTILE, CALENDAR, YEAR, MONTH, CYCLE, &c. where the chief points of what is called *Mathematical Chronology* will be found. With historical chronology this work has, of course, nothing to do. See GRIFFIN'S *Cyclopædia of History*. See especially Ideler's *Mathematical and Technical Chronology*.

Chronometer: Clock: Chronograph. Instruments for the accurate measurement of *time*. They bear, to our command over this formal condition of all finite existence, the same relation as the telescope bears to our command of its other formal condition—*space*. The arrangements of the universe offer, as our most accessible unit of time, that grand and *invariable* apparent diurnal revolution of the heavens,—or, the *sidereal day*. The *index* or *hand* of a just chronometer should make one exact revolution during a sidereal day; and also indicate, by graduated parts of that revolution, the requisite subdivisions of that day. In practice, this exact conformity of the revolution of the index with the duration of the sidereal day is not demanded; it is enough if the amount of the clock's daily *retard* or *advance* be known—(the clock's *rate*); and, if the observer may feel assured that his instrument will not *capriciously change that rate:*—that the graduation of the dial-plate, answer to the required subdivision of the day, is of course an absolute necessity. It will conduce to distinctness, if we arrange our remarks on the instruments employed for the above purpose under different heads.

(1.) *The Clock, commonly so called, or the Timepiece with Pendulum.*—The moving power of the modern clock, as is well known, is the *weight:* a cylinder is turned slowly round by the descent of a weight; and this slow motion, multiplied, and rendered apparent by aid of hands or indices, marks the progress of that descent. But such progress, although tolerably uniform, is quite inadequate to represent the flow of time with an absolute uniformity; nor was effective aid obtained from the old application of a *fly-wheel*, caused to alternate like the balance-wheel of a watch, by an obvious but rude artifice. The clock with weights did not become an instrument of Science, until Huyghens proposed to apply, as the supreme regulator of its motion,—the *pendulum;* whose laws had long before been discovered by Galileo. We owe to the great Florentine the two propositions;—that, provided the arc of oscillation be a small one, *pendulums of the same length oscillate in the same invariable period of time;* and that *the periods of the oscillation of pendulums of different lengths are as the square roots of their lengths.* Presuming, on the ground of these incontestable propositions, that the artist may obtain a pendulum whose oscillations shall mark, let us say, exactly *one second of time ;* it is easy to follow the application of the fortunate idea of Huyghens. Imagine such a pendulum (not represented in the sketch) suspended from the pivot D, to which is also attached, by a short arm, the *anchor* or circular arc A B C. This anchor will necessarily oscillate, or move slightly, now to one side, now to the other, synchronously with the pendulum. (*See woodcut on next page.*) The wheel E—one of the main wheels of the clock—has that permanent tendency to move round which is impressed on all the machinery by the *weight*. When the pendulum is at rest however, the wheel cannot move, being then caught by the anchor; and it is only when the anchor is moved sideways that the wheel is momentarily set free. During each oscillation of the pendulum one tooth of the wheel escapes, so that the machinery of the clock, and of course its index, makes *one step forward*, or *one beat*, during one oscillation of the pendulum, or, as we have supposed, during *one second of time.* The mechanical adjustment of the wheel and anchor—termed the *escapement*—requires to be arranged

so that the machinery play well; but, whether it play easily or not, the simple attachment of the pendulum secures that the clock beat seconds. Starting in this way, with provision for the uniform indication of the *smallest* element of time,

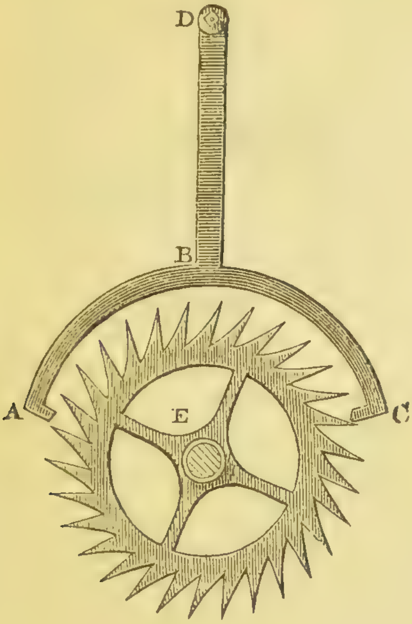


Fig. 1.

an instrument so constructed cannot fail in representing accurately all the subdivisions of a sidereal day, by the graduation of its dial; and the entire revolution of its index will correspond with the exact sidereal day, provided the pendulum be of the *precise length* of a second's pendulum. No artist would pretend that he had secured a pendulum of this necessary length by the first act of construction; but he provides means for slightly lengthening or shortening that part of the instrument, as the practice or *going* of the clock may show that it requires; and, by employing these means, the observer easily and speedily can bring his clock within a very small daily *rate* of retard or advance,—say one or two-tenths of a second. One requisition alone remains—Is the observer assured that the clock, thus completed, will *keep its rate*, or that he may trust its indications, in circumstances which prevent his examining and *determining that rate anew?* In other and more direct terms, Is the observer assured that the length of the pendulum, once adjusted, may not be liable to capricious fluctuations? Unfortunately, the reverse is the case. All bodies expand under the influence of increasing heat, and contract on the advent of cold; and the pendulum rod is not an exception. On every change of surrounding temperature, therefore, the clock must alter its rate; and, as the quantity of that change is not exactly determinable otherwise than by subsequent observation, the clock would thus cease to be a permanently reliable measurer of time. This defect might well have seemed fatal; but the ingenuity of the artist has overcome it, by an invaluable contrivance termed the *compensation pendulum.* The remedial artifice first suggested was the gridiron pendulum-rod—a composite rod of different metals, of different but known expansibilities. The suspension rod, G, it will be noticed, is attached—not to the ball, L—but to the cross rod, E E. Should G expand, the rod E E is of course carried *downwards.* Attached to E E are two rods, D D, whose tops are fixed to a cross rod C C, through the centre of which the rod G passes easily and *freely.* In case of expansion of the rods, D D, the cross bar, C C, is carried upwards. Fixed to C C, two bars, B B, stretch downwards, passing *freely* through the cross rod, E E, and carrying a third cross rod, A A, to which L is attached. These rods, B B, evidently expand *downwards.* The position of L, with regard to the suspension point of the pendulum, will in such a case be evidently affected, on an increase of temperature, by *three expansions*—two carrying it downwards, viz., those of the main rod, G, and of the rods B and B; and one carrying it upwards, viz., that of the rods D D. But if these three sets of rods—viz.,

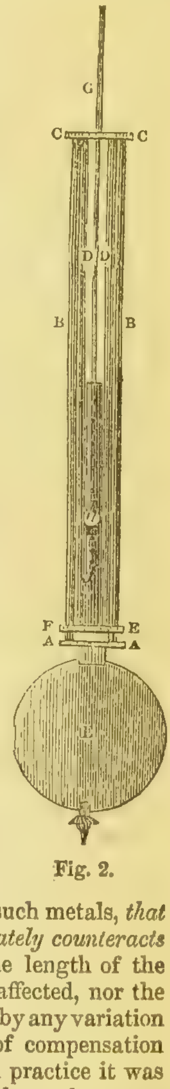


Fig. 2.

G, B B, and D D—can be made of such metals, *that the one expansion upwards accurately counteracts the two expansions downwards*, the length of the pendulum will evidently not be affected, nor the duration of its oscillations altered, by any variation of temperature. The principle of compensation now described is perfect; and in practice it was eminently successful. Nevertheless, the complexity of the gridiron pendulum, and certain defects inseparable from that complexity, have caused it give way before another arrangement, so simple that it may be understood without the aid of a diagram. Suppose that instead of the weight L, the rod G carries a cylindrical glass vessel, nearly filled with mercury. When the rod expands downwards, the same variation of heat will cause the mercury to expand upwards, or *ascend* in the glass vessel, so that the practical problem simply is, to give such dimensions to the cylinder, and to place within it such a quantity of mercury, that this upward expansion exactly compensate or counteract the dynamical effects of the lengthening of the rod. It is not too much to say, that the mercurial pendulum,

as now constructed, leaves nothing to be desired; and a Clock governed by it, constructed by the best makers, may be received,—under correction of observations repeated as often as convenient—as a reliable and absolute measurer of Time.

(2.) CHRONOMETERS, *commonly so called; portable instruments*; WATCHES.—The action of the Clock, as above described, depending on the undisturbed operation of gravity—alike in its moving cause the *Weight*, and its regulator the *Pendulum*—it is clearly requisite that the *repose* of the instrument be not disturbed. To do justice to a good Clock indeed, it is necessary to suspend it on a stone pillar, sheltered from the wind, but unconnected with the walls of any house, and not subjected to their tremor. But important purposes, alike in science and the arts, demand *portable chronometers*, faithful recorders of the lapse of time—capable, even while in action, of being conveyed from place to place;—instruments which must clearly depend on other agencies, alike for their *moving cause* and their *regulation*. The force of *gravity* is supplanted, in such chronometers, by the equally important and accessible mechanical force of *elasticity*. The moving power, or substitute for the Clock weight, is the force with which a strong spiral spring, *wound up* around an axle, uncoils itself; and, since the force of this uncoiling is not uniform, uniformity of motion is obtained from it by the tapering or pyramidal shape of the *fusee*—the name given to the wheel it turns. And similarly the office of the pendulum is performed by a *balance-wheel*, that oscillates regularly and isochronously, in obedience to the action and reaction of a spiral steel spring, delicate as a hair; one end of which is fastened upon its arm. The mechanism now described may be understood on a glance at the interior of a common watch. It is extremely fragile, in comparison with the comparatively solid and massive structure of the *Clock*; but, through the ingenuity and solicitude of the best artists, chronometers are produced that measure time with astonishing accuracy. The rate of these instruments is also liable to derangement through the influence of varying temperature; in consequence of its action on the balance-wheel. For instance, if A B A B be an arm, oscillating in obedience to any force whatever, around its centre, it will, through effect of a well-known principle of rotatory motion, oscillate slower if lengthened by expansion, and *vice versa*; so that, in the Chronometer, as in the Clock, increase of temperature will diminish the *rate*. Several modes of compensation have been proposed, but the following diagram illustrates the principle of them all. Let the arcs, B C, B C, on each of which is placed a weight D, form the circumference (an interrupted one) of the balance-wheel. Each arc is composed of two thin slips of different metals, lying side by side, the *outer slip* expanding somewhat more, under heat, than the *inner one*. When expansion takes place then, it is easy to see that the arcs B C must become *more curved*, and that the weights D will be drawn thereby nearer the centre of motion—an approximation, on the part of the circumference of the balance-wheel, which is meant accu-

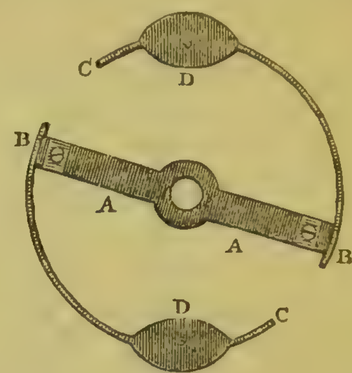


Fig. 3.

rately to compensate the recession of the motion of the arm, A B, through its being lengthened. It is due to our artists to state that the expected effect is here also to a considerable extent realized: but, the delicacy of the mechanism, should, if rightly estimated, render it in no wise wonderful that *compensation* is still the weak and fallible part of chronometers. Besides, the subject is of importance too profound, to permit that the inadequacy of prevalent methods be disguised. The emulation excited by the annual trials, and the publication of their results by the Observatory of Greenwich, has indeed succeeded in producing very large ameliorations; and, doubtless, when the highest care and skill are expended on them, instruments are constructed very nearly insensible to variations of temperature. But, unhappily, it is quite otherwise with the timepieces generally employed in our merchant service. The facts recently brought to light by Mr. Hartnup of Liverpool, surprise no one at all conversant with the actual state of these instruments. Five per cent. of them may very safely be pronounced utterly worthless — their rates varying with changes of temperature, so irregularly, that no *correction*, in the ordinary sense of the term, can be applied to them—the variations in question having no apparent law. But, even excluding these, it is the rarest thing possible to meet with an approximately good compensation. Generally speaking, for a change of temperature, from 40° to 60°, the mean average change of rate is *seven seconds a-day*: so that, however scrupulously his chronometer has been rated in harbour, a captain sailing from northern to southerly latitudes would, in eighteen days, mistake his reckoning of Greenwich time by nearly *two minutes*. Fortunately, the larger number of these instruments resume their old rate when the former temperature returns, so that their rate of variation is comparatively regular, and therefore calculable. Nevertheless, each chronometer *has its own special rate of variation*. It is incumbent, there-

fore, on every owner or master of a ship, to trust nothing, in existing circumstances, to a mere *harbour rate.* Before leaving port the chronometer should be tried under *the influence of different temperatures*, and the fact ascertained *whether its rate of variation has a law or not?* If it has not, the instrument is useless; if it has, the master ought to be in possession of that law, or of tables, that will enable him to correct for temperature. The loss of ships appears insufficient to remove the supineness of the merchant service, in this very simple but very important matter; and it would seem the duty of government to constrain attention to it. Nor is the precaution so necessary, beyond reach of any of our large seaports. Public observatories are now less uncommon; and the object sought for might always be obtained there. A mechanical remedy will doubtless gradually arrive, through still more improved modes of compensation.

(3.) The CHRONOGRAPH. An astronomical Clock beats *seconds.* But between one second and another some phenomenon may occur, and the observer requires to determine the fraction of the second at which it did occur. Hitherto he has done this by *estimation.* But, by an application something akin to the electric telegraph, a method is promised by which a second of time may be subdivided mechanically into hundredth parts, with almost perfect accuracy. Suppose a cylinder turning on its axis, and so governed in its rotation by the machinery of the Clock, that it will roll off, or deliver, say an inch length of a slip of paper in one second of time; the observer may, by a touch of his finger, cause a mark to be impressed on that inch of paper at the very moment of the occurrence of the phenomenon, and the position of that mark will indicate the exact instant of the occurrence. An apparatus of this sort has now been constructed at Greenwich. Mr. Airy has given a very detailed account of it in one of his recent annual reports. It seems complicated, but the ingenuity displayed throughout, is worthy of the mechanical genius of the Astronomer Royal. It answers perfectly, and will inaugurate a new era in observation, as far as the element of time is concerned.

Chronoscope. One of the very ingenious discoveries of the fertile Mr. Wheatstone. It is an instrument founded on the fact that luminous impressions on the eye *persist* for a certain time, or endure after the actual sensation is gone; and by which the discoverer, desires to prove the *instantaneousness* of certain luminous phenomena, such as the electric spark, or to measure their *duration*, however brief that may be. One mode suggested by Mr. Wheatstone is this:—The phenomenon is observed by reflexion in a mirror to which a rapid motion is communicated—a motion of such a nature, that supposing the luminous object *permanent*, its image would appear to describe a great circle. This accomplished, it is clear that if the phenomenon be *instantaneous* its image will appear as a *mere point* of the circle, and will have no shape. If, on the contrary, the phenomenon have an *appreciable duration*, the image of it will stretch out and form an arc of the circle, greater or less in proportion to that duration; and the size of the arc will constitute a measure of that duration. An electric spark viewed in this way shows no elongation, so that we cannot attach to it any duration. The discontinuity of certain *flames* and other luminous streaks, have, in this same way, been rendered manifest to the eye. See VISION, LIGHT, VELOCITY OF, &c.

Cinematics. The science which treats of motions, without concerning itself, as Mechanics do, with their cause, and the physical ideas that are thus introduced. Suppose, for instance, that a given body moves with a certain rotatory and a certain advancing motion, it is the province of cinematics to trace out the complete path which every individual particle of the body will describe. The velocity of motion, and the forces causing it, may be of any magnitude whatever. Cinematics treats the problem quite independently. Cinematics forms, properly, an introduction to mechanics, as involving the mathematical principles which are to be applied to its data of forces. The distinction is very useful, but it has not, until recently, been clearly brought forward. M. Laboulaye (1849) has published the first treatise on the subject.

Circle. A curvilineal figure, whose definition is, that every point in the curve, or, as the curve is termed in this case, in the *circumference*, is at the same distance from a fixed point named the *centre.* The properties of the *circle* are well known, and are investigated in every treatise on Geometry. Speaking more generally, a circle is a curve of the *second order*, belonging to the class of the *Conic Sections.*

Circle, *Astronomical or Spherical.* A mathematical circle passing round the Heavens or some part in them. If the circle shall bisect the celestial sphere, it is called a *Great Circle* of the Sphere: such are the Equator, the Ecliptic, the Circles of Right Ascension, &c., &c. If it does not bisect the Sphere, it is called a *Small Circle* of the Sphere: such are all Circles of Declination (excluding the Equator), the Arctic and Antarctic Circles, &c., &c. The relations of three intersecting Great Circles of the Sphere constitute the subject of SPHERICAL TRIGONOMETRY.

Circle, in *Instrumental or Practical Astronomy*, signifies a *Circular Instrument* employed in the measurement of Angles: beyond question the instrument on the construction of which Mechanical Art has lavished its choicest efforts. We shall purposely describe this most important Element in an Observatory at considerable length; giving our reader notice that the principles now explained shall be accounted as understood by him, in all reference to smaller instruments

belonging either partially or mainly to the Circular Class. It were useless to occupy time in explaining how the angle between distant objects may be measured by help of graduated circular arcs; neither shall we refer to the time when *sights*, and not *telescopes*, were the guide of the observer, in his endeavours to fix the direction of any celestial or remote body. Previous to the application of those optical powers, by whose aid we now magnify spaces either distant or near, the observer could make no exact determination unless by aid of an apparatus of large size, and proportionally unmanageable. Doubtless, the illustrious Tycho, who has left us the picture of his great Quadrant, occupying the *side of a house*, would have rejected it as a fable, that posterity, by use of a circle, easily carried in a boy's hand, would be able to detect far smaller angular quantities than were perceptible to the whole apparatus of Uraniburg! Even after the application of the Telescope, angles continued to be measured by the Astronomer as well as the ordinary Surveyor, by *Quadrants;* a class of instruments of which we give a rude representation in the margin. The mode of applying a quadrant of this

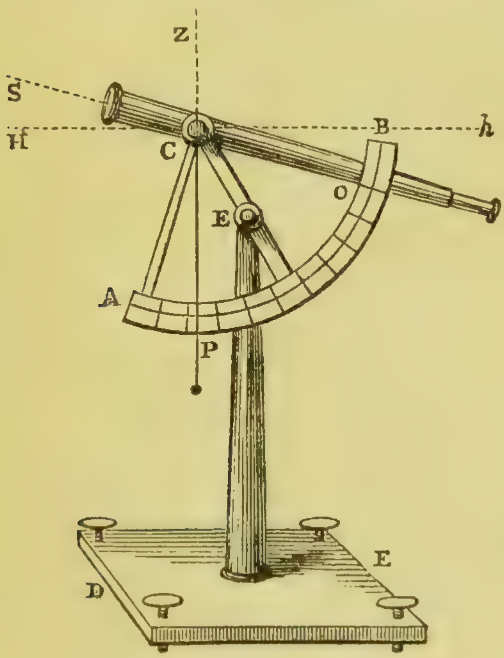


Fig. 1.

sort will be sufficiently obvious on cursory inspection of the diagram. It is of more importance that the reader apprehend why the use of the mere *quadrant* was abandoned, and recourse had to *entire circles* graduated all round their rim. It is abundantly clear that, however accurate and fine the graduation of the arc of the quadrant, the action of the instrument could not be depended on, unless its entire structure and working involved no serious error. But who could answer for the perfect *cylindricity* of the pivot on which the Telescope turns, or who could answer for the perfect *circularity* of the limb A B? No perfection of mere mechanical work could secure either of these requisites; so that the artist aimed, through a *change of form* of the apparatus, to insure that such error—apparently unavoidable—should be *compensated for.* And he accomplished this, by continuing the *quadrant* both ways, and converting the apparatus into a *circle.* The reader will now permit his thoughts to rest on such an instrument as below:—

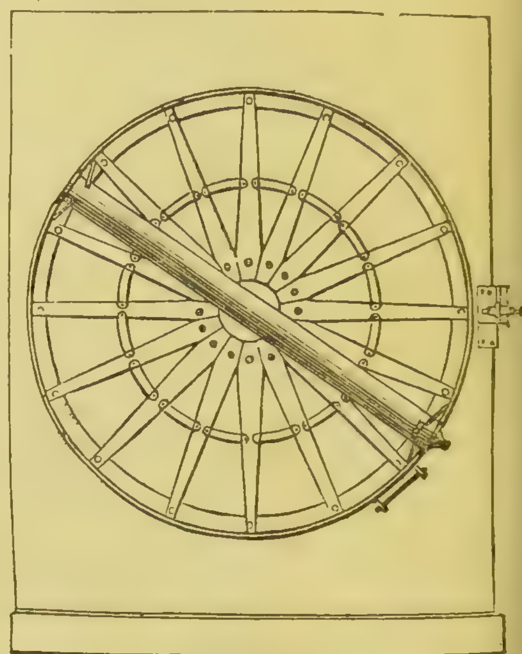


Fig. 2.

Imagine, for sake of distinctness, that the division of the instrument can be read at the two ends of the Telescope, which may be supposed moveable around its pivot. If the pivot be not a perfect cylinder, or if there exist an error of *eccentricity*, its effect will be to jolt the telescope as it moves, from the centre of the circle. But it is plain that as the jolt, in whatever direction, will just bring one end of the telescope as much nearer the line of division as it removes the other from it, the *average* or *mean* of the *two opposite readings* will, in effect, correspond *with the one reading, as it should be, were eccentricity null.* Nor does it require that these readings be made at the ends of the telescope; they simply require to be made at the two ends of a diameter. In circles of this sort, the telescope is not detached, but clamped to the instrument. The instrument turns round *en masse*, and the reading is effected by fixed microscopes, one of which is represented attached to the stone pillar. Two such microscopes then, correct for eccentricity. And supposing the divided line not quite circular, the other great source of error above specified; supposing it *elliptical* more or less,—the reader will discern without difficulty, if he takes the trouble to draw an ellipse, and suppose its circumference

equally subdivided, that a reading by *four opposite microscopes* must correct for error arising in any slight and unavoidable ellipticity. It has been the favourite plan of some artists to adapt four, eight; and three, six reading points to their circles. The ground of all compensation, however, is explained above. Ramsden was the first practically to comprehend the value of circular instruments, and two of his chief ones, placed at Palermo and Dublin, leave but little to be desired. Important modifications have been effected since his time, as will be seen below.—What we have further to say will be done best, if we separate these instruments into *classes*.

(1.) *The Mural Circle.*—The Mural Circle, the favourite achievement of the late Troughton, is represented in its leading features in the woodcut immediately preceding. The Telescope and Circle are closely attached, and they move round together on an axis sunk deep into the solid wall on whose face they lie. The division of the circle is on the side or edge of the limb of the circle and not on its face; so that the mode of reading is as the annexed woodcut shows. Troughton's mural circles

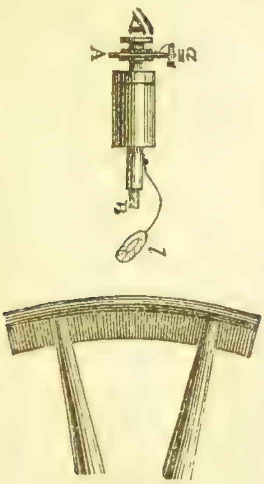


Fig. 3.

are usually from five to six feet in diameter. The one in the observatory at Cambridge exceeds these dimensions. They are meant to be so placed that their face, and the Telescope attached to it, sweep the meridian line. Their limb is divided usually to every five minutes of space; subdivision into seconds and tenths of seconds of space is effected by the Reading Microscope (*q.v.*) The mural circle is necessarily of great weight; but to prevent that weight from bearing too hard on the pivot, a system of counter-balances was put in play, whose nature will appear more distinctly in the cuts given of the Transit Circle. This important instrument has only one function, viz., to determine the declinations or zenith distances of stars. It cannot, with requisite accuracy, give their right ascensions; on which account it is rapidly giving way, after having done much excellent work, to that other structure which we shall next briefly describe.

(2.) *The Transit Circle.*—Until quite recently, it was necessary to the right use of a Transit Instrument (*q.v.*), that it be reversible; in other words, that the east end of its axis be turned to the west, and *vice versa;* and it is still of importance that such reversal be easily accomplished. Ramsden's circles were reversible, turning round on two pivots in the zenith and nadir; instead of being fixed to a wall like Troughton's; nevertheless, what they gained by their adaptibility to the office of Transit Instruments, they lost through the absence of permanent fixture *in the Meridian.* The desideratum was supplied by Reichenbach of Munich. The instruments constructed by him are as fitted to give correct *transits*

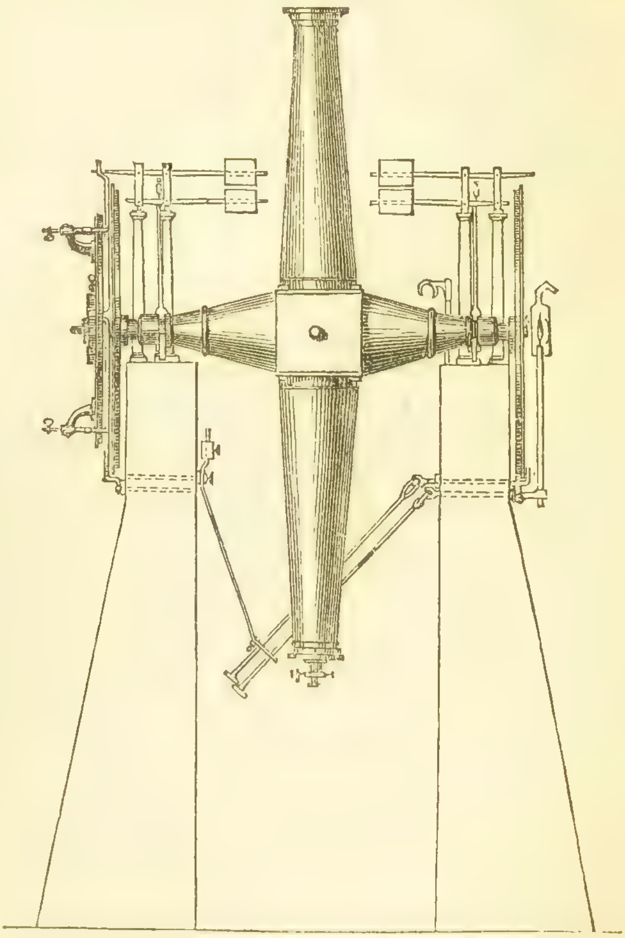


Fig. 4.

or *right ascensions*, as *declinations;* and the form he adopted, improved at Munich by his successors Ertel & Son, and by Repshold of Hamburgh, is rapidly becoming the *normal* one. The reader will readily understand the construction of the

Transit Circle on examining the two subjoined representations of the one placed by Ertel & Son in the Observatory of Glasgow. The first view of this instrument is a front one;—showing the telescope (see previous page) with all the attributes of a Transit Instrument, placed on two massive piers. When needful, this great telescope can be reversed with little trouble by aid of a subsidiary apparatus, represented in the cut, under article COLLIMATORS. By the Collimators, there described, the error of the telescope's collimation, as well as its steadfastness in the meridian, can at all times be directly determined. Attached firmly to the ends of the axis of the transit telescope, are two perfectly similar circles of three French feet in diameter. One of these is meant to read minutes of space by aid of a vernier; its divisions are in silver, and *to its rim* the *clamp* is attached. The other circle is *perfectly free;* it is divided on a thin line of gold embedded within the metal of the rim, to *two seconds of space*, and to this line, four reading microscopes, capable of reaching *tenths of seconds*, are applied. The great mechanical fault of the construction, as now explained, is the place to

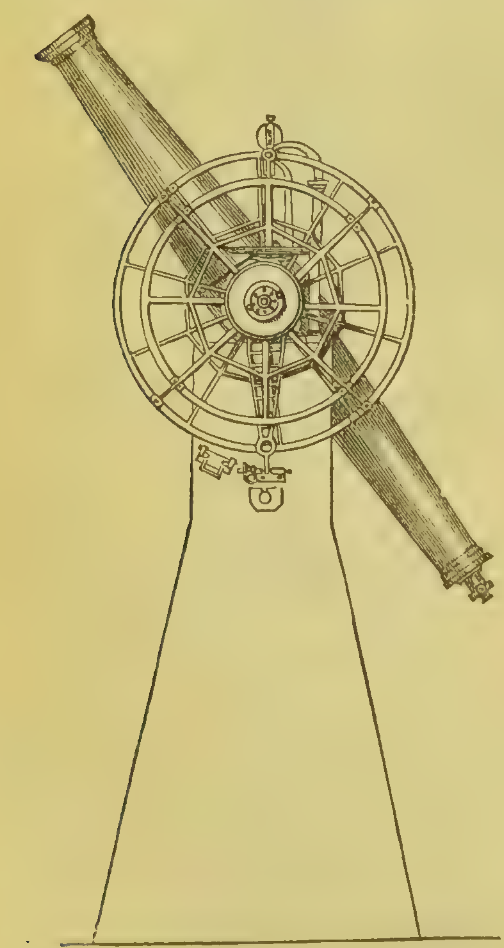


Fig. 5.

which the *clamp* is applied. The writer of the present notice, can very sensitively aver that the clamp of a heavy instrument ought never to be applied to the circumference of a comparatively slight circle. One characteristic of these foreign instruments is, the *mode of fixing the reading microscopes.* In the mural circle these were attached to the wall; not so with Reichenbach, Ertel, or Repshold. The foregoing side picture of the Glasgow Transit Circle may render the construction sufficiently plain. The view presented is that of the side of the Transit Circle on which the fine divisions lie. The inspection of these, and of their subdivisions, is accomplished by four microscopes fixed in the circumference of a smaller circle, represented in the cut, and named the *Alidade.* The axis of the transit telescope passes through the axis of the alidade; in fact, the latter coincides with the former. It may seem that instability is thus necessarily communicated to the positions of the reading microscopes, which assuredly ought to be absolutely fixed. The alidade, of course, does not turn with the telescope and its attached circles; although its fixity may be disturbed by effect of the eccentricity of the axis of the telescope which is revolving within it. Two effects may be impressed on the alidade by this eccentricity. It may, notwithstanding its clamp, be turned slightly round, or it may be subjected to a mere jolting motion, up and down or sideways. The latter is of course corrected by the readings of the opposite microscopes; and the former is corrected for, in every individual observation, by observation of a level attached to the alidade, which indicates and measures the minutest amount of displacement in *rotation.* The peculiarity of this apparently unstable mode of fixing the reading microscopes is this:—*They cannot change their place without leaving a record of that change on the level.* We deem it preferable to their establishment on stone pillars, for the same reasons that in the Barometer, a scale with known, although sensible rate of variation, is accounted better than one whose rate of variation is not known, and is definite although inconsiderable. There is no defect in the *theory* of these continental instruments. They are quite effective to fulfil their promise of enabling *one observer* to take at once the two co-ordinates of a star. The points in which they seem to fail—if any failure may justly be attached to them—is *stability.* The reproach of comparative fragility, or instability, has been frequently urged against continental workshops, as to all optical instruments. It is pleasant, therefore, to have to record, that the task of re-constructing a *transit circle*, has been not only undertaken but accomplished by the present eminent Astronomer-Royal, Mr. Airy. In complement of his unparalleled services to our great National Observatory, he has achieved the erection of a Transit Circle, consolidating the best conceptions of Reichenbach, Ertel, and Repshold; and superadding a degree of *firmness* neither contemplated nor attained by these

minent Artists. Mr. Airy's original conception (urged by his peculiar *dynamical* genius) was this:—To demand from Engineers, on behalf of Astronomical Instruments, that assurance of stability as well as accuracy, which they so readily supply elsewhere. The Astronomer-Royal must succeed. His new Transit Circle has very obvious advantages. The circles are closer to the centre of the Telescope; and there is no such thing as *clamping* at any circumference.

The foregoing brief account of Circular Instruments has left much unexplained. The reader must apply to larger treatises. But one point requires notice here. To read graduation aright the *zero* or beginning point of the order of degrees, must be fixed. The points usually endeavoured to be fixed, are the *horizontal* or the *zenith* points. The horizontal point is determinable in two ways. *First;* suppose a star is observed first directly, and secondly, by its image reflected in a basin of mercury; the line between, must be the horizontal line;—a mode of determining the line or point very common, if not universal, in good Observatories until recently. *Secondly;* take a *horizontal Collimator*, or a telescope whose horizontality can be assured by a *level*, and placed as to the main telescope in the position of the subsidiary instruments in cut under article COLLIMATOR. The horizontal point may be determined thus, with all accuracy attributable to the *level.* But a more accurate process has superseded these; the process of observing the *nadir point*, or the point of 180°. As in the subjoined woodcut, the telescope is so placed over a trough of mercury, that by aid of a peculiar eye-piece, the observer can discern at once the threads of the eye-piece directly, and the reflection or image thrown back from the mercurial level. When these coincide, below the eye, with the actual system of spider's threads, the Telescope must point to the Nadir.

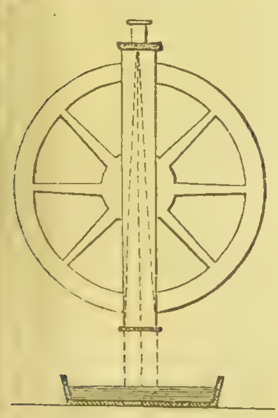


Fig. 6.

(3.) *Circle Repeating.*—It was at one time considered necessary that to obviate errors of *division*, every angle should be measured on different parts of the divided circle; and mechanical means were provided for effecting this. Circles of this sort were named *Repeating Circles;* but they are now in comparative disuse.

(4.) *Circle Reflecting.*—A very important Nautical Instrument. See SEXTANT.

(5.) There are innumerable other forms of Circular Instruments—*Great Circles, Universal Instruments, Prism Circles,* &c., &c., but the principles on which these may all be judged, have either been explained or referred to; and we can attempt nothing more in this Dictionary.

See DIVISION, TRANSIT INSTRUMENT, READING MICROSCOPE, SEXTANT, VERNIER, &c.

Circular Magnetic Polarization. In the year 1845, Faraday published his great discovery of the action of magnetism upon light, and opened up an entirely new and interesting branch of physical science. His fundamental experiment is as follows:—A ray of light is plane polarized, say by reflection from a surface of glass. The state of the ray during the various conditions of the experiment is tested by a Nicol's prism. In the course of the polarized ray is placed a bar of *heavy glass*, or silico-borate of lead, which is one of the best substances for exhibiting the phenomena. The Nicol's prism shows, that the state of the light is not essentially affected by the transmission through the heavy glass. The ray is still polarized, and in the same plane as formerly. But a very different result is obtained in these circumstances, by the mere introduction of powerful magnetic forces. In the neighbourhood of the bar of glass, two powerful opposite electro-magnetic poles are placed near each other, and in such a position that the direction of the ray through the glass is very near the poles, and parallel to the straight line which joins them. Supposing now that the light passes through the bar of glass, and that the poles are not yet magnetized by the electric current. Let the Nicol's prism be placed in the position of complete extinction, so that none of the light is transmitted through it. If, in these circumstances, the force of the electro-magnet be developed by the passage of a current through its coils, the light immediately reappears through the prism, and this continues as long as the magnetic arrangement is sustained, and no longer. When the light has reappeared under the action of the magnet, it can be made to disappear by turning the Nicol's prism through a certain angle. The polarized light is essentially affected, therefore, by transmission through the *diamagnetic;* and the affection is found to be identical with that which we have described in CIRCULAR POLARIZATION, as produced under very different conditions. The light is still plane polarized, but the plane of polarization has deviated through a certain angle under the magnetic action. We may now give a brief statement of the known laws of this action of magnetism upon light.

We may, in the meantime, regard the silicated borate of lead as the transparent medium that manifests the action. In the fundamental experiment, as already described, the force employed was the electro-magnetic, so that the proper force of the electric current was modified. When the iron cores were withdrawn, the action of the current was still sensible, but very feeble, showing that the introduction of the magnetic body increases the intensity of the current's action

upon the ray, though it does not originate the action. Powerful effects are obtained by the employment of long helices without magnets, the transparent body being placed within the helix, and the ray being transmitted in a direction parallel to the axis. With regard to the direction of deviation of the plane of polarization, it is connected with the arrangement of the electric or magnetic forces, by a very simple law. If the ray is affected by the simple action of an electro-helix, within which the bar of heavy glass is placed, the ray rotates in the direction of the current. The action of the current, is to draw the plane of polarization with it, round the ray. If the action of the light be due to opposite electro-magnetic poles, we have only to conceive that the heavy glass is a magnetic body. According to Ampere's theory of magnetization, the surface of the heavy glass would be traversed, on this supposition, by electric currents in planes perpendicular to the lines of magnetic force. A plane polarized ray, transmitted through the glass within this *imaginary helix*, will rotate, as in the former case, in the direction of the current. If the electro-magnetic poles be reversed, the direction of rotation of the ray will therefore be reversed. It was stated, in the description of the fundamental experiment, that the direction of the ray through the glass was parallel to the line joining the poles. This condition is not absolutely necessary, but it is most favourable to the manifestation of the desired effect. Understanding by lines of magnetic force, those lines which are usually called magnetic curves, and which are represented in experiment by the arrangement of iron filings in the field of force, we observe generally that the rotation of a polarized ray, under magnetic action, depends essentially on the inclination of the ray to the lines of force in the transparent medium. If the ray is parallel to the lines of force, the effect is a maximum. As the ray deviates from this direction the effect diminishes, until the ray is perpendicular to the lines of force, when there is no result. The peculiar action that is now under consideration has been manifested by every transparent medium examined, except the gases and the most of the crystals. Faraday's heavy glass appears to be the best medium yet discovered. Pure silicates of lead have been found, indeed, which are twice as powerful, but they have the disadvantage of tarnishing rapidly in air. Every liquid that has been tried manifests the action to some extent. Among crystals, fluor spar, rock salt, and rock crystal, appear to exert a feebly sensible action, but the crystals as a class give no result. Many of these transparent media possess a natural rotatory power. In the case of such bodies, the magnetic is found to interfere with the atomic action, only in the way of simple super-position of effects, increasing the ultimate rotation of the ray when the direction of the current is favourable, and diminishing it in the same degree when the direction is adverse. The amount of rotation of the ray, depends upon several elements. It depends, as we have seen, upon the nature of the transparent diamagnetic medium employed; it depends also on the quantity of the medium employed, or rather upon the ray-length in the medium, varying directly as this length, if other things are equal; it depends also upon the intensity of the electro-magnetic force, varying approximately, according to Faraday's statement, in the direct ratio of the intensity of the current. We learn, from the experiments of Bertin, that the amount of rotation of the ray is connected by a very simple law with the distance of the diamagnetic from the magnetic poles. When only one magnetic pole is employed as the circularly polarizing agent, the angle of rotation of the ray appears to decrease in geometrical progression, if the distance of the diamagnetic from the pole increases in arithmetical progression. When two helices are employed with opposite poles adjacent, their actions, estimated by the above law, are simply superposed. Two magnetic poles could not be expected to act in this way of mere superposition of separate effects, because of their powerful inductive action upon each other. The magnetic rotatory power appears, as already stated, to be a specific property of bodies. It is worthy of notice that this power depends, not only on the chemical and physical nature of the body itself, but upon the variable circumstances of pressure and temperature. Matteucci made the interesting discovery that a diamagnetic, submitted to pressure in a direction perpendicular to the ray, has its rotatory power modified, and this to such an extent—for example in crown glass—that the rotation of the ray under magnetic action may be entirely prevented. He discovered also, in heavy glass and flint glass, an increase of rotatory power by an elevation of temperature. With the same electro-magnetic forces, he found his specimen of heavy glass twice as powerful at the temperature of boiling oil as at common temperatures. Such are some of the more important facts which have been discovered in regard to the relations of magnetism and light. We shall only notice, in conclusion, an essential distinction between the Atomic and the Magnetic circularly polarizing actions—a distinction that may have occurred to the reader already. The former action does not depend upon the direction of the ray through the medium; it affects the ray with the same screw-motion, and to the same extent, for all directions, if the ray-length in the medium be constant. The latter action affects the ray most powerfully when passing through the medium in a particular direction, and not at all when in a perpendicular direction; and, for opposite directions of the ray, it does not affect the ray with the same screw-motion, but with the same absolute rotation. Hence, for example, if a ray be transmitted through a circularly polarizing medium, and be retransmitted by reflection

at the posterior surface, it will emerge in its original state, or with a double deviation, according as the circularly polarizing action is Atomic or Magnetic. On the theory of the Magnetic Rotatory Action little can be said. It is next to certain that the action is due to a peculiar arrangement of the molecules of the diamagnetic, but the nature of this arrangement is quite unknown. Faraday has suggested a state of tension, or a *tendency to currents*, in the diamagnetic, as the proximate cause of the phenomena. The history of a great discovery must be interesting in all cases, but more especially so, if the researches which led to it were founded upon a really philosophical and judicious expectation. In this view Faraday's discovery claims double admiration. He informs us that he had long been persuaded, in common with many other philosophers, that the various forms under which the forces of matter are made manifest have a common origin, and are in a manner convertible into each other. This persuasion extended to the powers of light, and led to many laborious but ineffectual researches, having for their object the discovery of the relations between electricity and light. Some of these investigations appear in the *Philosophical Transactions* as early as 1834. The fruitlessness of these researches could not remove the well-grounded persuasion referred to. He therefore resumed the inquiry "in a most strict and searching manner," and, after many ineffectual trials, succeeded at last, as we have seen. We may hope, after this, that the general conviction upon which he rested, in regard to the closeness of connection among the various natural forces, may yet lead philosophers to make similar exertions in other directions, and with similar or even greater results.

Circular Numbers. In arithmetic, numbers, all whose powers end in the same figure as they themselves do. Thus, any number ending in 0, 1, 5, 6, is a circular number.

Circular Polarization of Light. This is a subject that is full of interest, both to the experimental and the theoretical student of physical science. The facts connected with it constitute an important part of our present knowledge in optics; and the theory of these facts, which is due to the genius of Fresnel, is one of the finest examples of a clear, simple, and complete explanation of a varied and complex class of physical phenomena. The very difficulties that encompass the subject, in the present state of science, give an additional interest to it: we refer especially to the difficulties connected with the *production* of circular polarization.

Light may be circularly polarized by a very simple process. Fresnel's Rhomb, as it is commonly called, is a parallelipiped of glass, whose faces are inclined to each other at certain angles, dependent on the refractive power of the glass. If a beam of plane polarized light be incident upon one of the bases of the Rhomb perpendicularly, so as to emerge at the opposite base after two internal reflections, the result is found to depend essentially upon the inclination of the plane of primitive polarization to the plane of internal reflection. When this angle is 0°, thus 0° or 90°, the emergent beam is plane polarized, as was the incident; when the angle has any other value, the beam is elliptically polarized; and particularly, when the angle is 45°, the emergent light is found to have undergone that peculiar change which is called Circular Polarization. Light, in the last of these conditions, has peculiar properties. First of all, it is destitute of every trace of plane polarization. When subjected to the action of any analyzer, such as a Nichol's Prism, it conducts itself precisely as common light. In this respect it differs most eminently from plane polarized light, which is extinguished in one position of the analyzer. It differs also from elliptically polarized light, the latter giving a beam of varying intensity for the different positions of the analyzer, though never extinguished. The property now mentioned might lead us to suppose that the action of the Rhomb, in circular polarization, is simply a depolarizing action, which reduces the plane polarized beam to the state of common light. We observe, therefore, that the properties of light in the state of Circular Polarization, distinguish it as clearly from common as from plane polarized light. One instance may be given in proof of this. If two beams, one of common light and the other circularly polarized, be subjected to the action of a Fresnel's Rhomb, the results are different in the two cases. In the first the emergent beam has no property, as far as we know, distinguishing it from the incident, or from common light in general; in the other case, when the incident beam is circularly polarized, the emergent is plane polarized. The state of circular polarization is therefore different from that of common light, since the two states are differently modified by one and the same action. We cannot enter into the details of this subject, but there is one point that claims our special notice, from the importance of its connections. It has been already stated that the action of the Rhomb upon a beam of plane polarized light depends on the inclination of the plane of original polarization to that of internal reflection. Let A denote this angle. We may speak of all its possible values as comprehended between 0 and 90°, including values properly negative, from 0 to —90°. If A has any other value than 0 or 90°, the light is, as we have stated, elliptically polarized, and if A=45°, the elliptic polarization becomes circular. Let a second Rhomb be now placed, so as to receive the beam emergent from the first, and let its position be similar to that of the first; then the light emergent from the second will be plane polarized in every case—that is, for all the values of the angle A; so that, by two internal reflections under proper conditions, we convert a plane polarized beam into one elliptically polar-

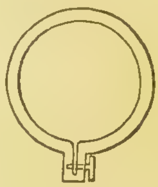
ized; and, by two additional reflections in the same plane, we reproduce the state of plane polarization. The finally emergent light, though plane polarized, is not in precisely the same condition as the original beam. The planes of polarization of the two beams are found to be different; they are inclined equally to the plane of internal reflection upon opposite sides of it; so that their mutual inclination is 2 A. By means of four internal reflections we can, therefore, make the plane of polarization deviate through any desired angle. In other words, a plane polarized ray may be made, by such means, to turn through any angle round its own axis. In connection with the preceding statements, we have now to refer to a very remarkable class of facts. If a homogeneous ray of plane polarized light be transmitted perpendicularly through a plate of rock crystal, whose faces are perpendicular to its axis, the ray after transmission is still plane polarized, but the plane of polarization has deviated through a certain angle. The result of transmission through the rock crystal is precisely the same as that of four internal reflections described above. This is the reason why the peculiar action of the rock crystal is usually described in connection with the subject of Circular Polarization. The laws of this action are simple, though the nature of it is not known. First, with regard to the direction of deviation. We may speak of the ray as propagated through the plate with a screw-motion; it is made to revolve, in fact, round its own axis while it is being transmitted through the crystal. In every particular crystal of quartz, the motion of the polarized ray is constantly that of a right-handed screw or of a left-handed, whatever be the position of the original plane of polarization, and whatever be the face of the plate upon which the ray is incident. In some specimens of rock crystal the rotation is to the right, in others to the left. This difference has been clearly connected by Haüy with a distinction in crystalline structure.

With regard, again, to the amount of the deviation, this depends upon the wave-length or the refrangibility of the homogeneous light transmitted, and upon the thickness of the plate. The angle of deviation varies in the direct proportion of the last of these elements for all plates or sets of plates derived from the same crystal. The most refrangible rays are deviated to the greatest extent in every case. Thus, in an experiment of Biot's, the deviations produced by the same plate $\frac{1}{25}$ of an inch in thickness, were 44° for the extreme violet of the spectrum, about 17° for the extreme red, and intermediate values for the rays of intermediate refrangibility. When a beam of white light has been plane polarized, and transmitted through a plate of quartz, the variously refrangible rays of which it is composed rotate unequally, as the last statement would lead us to expect. In such a case, a Nicol's prism will not extinguish the emergent light in any position, but will give coloured images. Quartz is the only known solid that exerts the circularly-polarizing influence upon light transmitted through it without internal reflection. But there are many liquids that possess this property, such as oil of turpentine, oil of citron, and other essential oils, tartaric acid, syrup of sugar. What especially distinguishes this class of actions from that of rock crystal, already described, is the constancy of the results for all directions of the ray through the liquid. The rotation of the plane of polarization is as evident in the case of liquids as with a plate of quartz, but the rotatory power of liquids is much more feeble. Thus, concentrated syrup of sugar, the strongest of liquids in this respect, produces a deviation only $\frac{1}{30}$ of that due to a plate of quartz of the same thickness. We notice among liquids a distinction similar to that observed between different specimens of quartz, in regard to the direction of deviation. Some liquids, such as syrup of sugar and tartaric acid, make the plane of polarization turn from left to right; and others, such as gum arabic and essence of laurel, in the contrary direction. When liquids which possess the rotatory power are diluted or mixed in any proportions, there is a perfect preservation and superposition of effects. The same quantity of liquid, when traversed by a ray of polarized light, produces the same amount of rotation in all circumstances of dilution and mixture. We except from this rule the cases of mixture which are accompanied by chemical action. The peculiar action of transparent media that we have been considering has been very fully investigated by Biot. He has given to it the name of Circular Atomic Polarization.—The Theory of Circular Polarization may be now briefly referred to. According to the generally received form of the Wave Theory of Light, the molecular vibrations of a plane polarized ray are rectilineal, and constantly perpendicular to the plane of polarization. In a circularly polarized ray the vibrations are supposed to be circular, the planes of the circles being constantly perpendicular to the direction of the ray. If we suppose, then, that the particles in the course of a ray are arranged in succession in the form of a helix or screw-thread, and that the screw revolves round its own axis, without advancing, we have a representation of the movement of waves in a circularly polarized ray, according to the theory. There will be evidently two kinds of circularly polarized light, according as the screw in the illustration is right-handed or left-handed. The various properties of circularly polarized light are beautifully explained by this theory. We shall only indicate the explanation of the two distinguishing properties mentioned at the outset. The first is, the absence of all trace of plane polarization, though the circularly polarized beam is obtained from a plane polarized one by a very simple action—that of two internal reflections. To explain this property, we observe

that the action of an analyzer, such as Nicol's Prism, is to obstruct the passage only of those vibrations which are perpendicular to a particular plane, this plane holding a constant position in the analyzer, and revolving with it round the ray. Now, it is a property of the circular vibration that, if it be resolved into two rectilineal vibrations at right angles to each other, the amplitudes of the components are independent of their directions. A circularly polarized beam, when analyzed, will therefore lose the same quantity of light in all positions of the analyzer—that is—will present no sign of plane polarization. The second property mentioned was the convertibility of a circularly polarized beam into a plane polarized one, and *vice versa*, by two internal reflections at proper angles, in a transparent medium. In explanation of this property, it is proved very simply that a circular vibration is converted into a rectilineal one, if either of its rectangular components be accelerated or retarded by a quarter of a complete vibration; and that a rectilineal vibration is changed into a circular one of the same amplitude, by a similar acceleration of one of its equal rectangular components. We have only to assume, therefore, that in internal reflection there is an acceleration of those vibrations (say) that are in the plane of incidence, while those at right angles to this plane are not accelerated; and further, that the amount of acceleration due to two internal reflections at the angles of incidence proper to Fresnel's Rhomb, is a quarter of an undulation. This assumption is made, accordingly, as the basis of the theory of Fresnel's Rhomb, and more generally as a fundamental principle, in explanation of the mutual convertibility of plane and circularly polarized light by means of internal reflections. The assumption has certainly much in its favour. While upon the theory, we should not omit reference to the phenomena of Circular Atomic Polarization, which have been already described. The nature of the molecular actions that give rise to this class of phenomena is entirely unexplained. We may have a clear view of the difficulty, by supposing the incident beam of plane polarized light to be decomposed into two opposite circularly polarized beams; the action of the medium is then to be regarded simply as giving rise to an acceleration of one of the latter beams relatively to the other. Fresnel has proved, in the case of quartz, that two opposite circularly polarized rays traverse the medium with different velocities; but the difficulty is, especially in the case of liquids, to conceive an action, capable of producing such a result. See QUARTZ.

Clamp: The name of a portion of an instrument, whose function is that of arresting the instrument's motion. Applied to a railway carriage it is called a *break*.

The excellency of the clamp resides in two qualities. It must keep the parts of the instrument which do not require to be moved perfectly still; and permit at the same time a perfectly unembarrassed motion to those parts which require to be moved. The clamp itself is sufficiently simple; consisting generally of two plates, connected by a screw, which can bring the plates to catch upon any object, and hold it close. The less of the object they so catch—if they hold it securely—the better; because the motion of the other parts will be so much the more free. A tangent screw is usually connected with the end of the clamp, and with the moveable parts; and is employed to produce very fine and slow motions.



Sometimes, instead of the clamp with two plates, a circular clamp is made use of; it is employed to hold a *tube*. It consists of a ring nearly fitting the tube, having a descending end as in the figure. The ring is made of elastic material, and when the screw represented does not keep the slit quite close there is a sufficient opening out of the ring to let any body readily free. If the slit were the whole length of the mass, instead of being terminated as in the figure, it would not be easy to preserve such a clamp in good working order.

The best account of clamps, with a very full description of the various constructions, will be found in *Pearson's Practical Astronomy*, vol. ii. pp. 272-277.

Cleavage: A phenomenon presented by many of the slates and older stratified rocks which physical science has recently brought within its domain. The phenomenon is this:—Every one knows that a bed of stratified rock can generally be split with a certain ease in the direction of the strata; the housemaid who lays a block of coal properly on the fire bears unconscious testimony to this fact. But such old beds also split with comparative ease in some other direction, having no definite connection with the strata. This split is a cross split, and calls to mind the fact that crystals, such as Iceland spar, also split easily in two directions. The phenomenon in the case of the crystal attaches to the nature of crystallization; but this new direction of split, or this "plane of cleavage," which often runs uniformly through vast mountain masses, cannot be explained by the ordinary processes of crystallization: nor until quite recently did any rational theory or physical cause seem to the geologist to be attainable. Professor Sedgwick, indeed, had a considerable time ago, ventured on the conjecture that probably polar forces of some sort were developed amid the molecules of a stratified mass, after its stratification; and that—inasmuch as a bar of iron, after the lapse of years, shows a tendency to become crystalline—so may the solid rocks tend to a similar state;—whence these planes of cleavage. The idea was an imposing one, and has since been swollen to gigantic di-

mensions by the Norwegian Keilhau, in whose opinion even the crystalline granite masses are only stratified rocks metamorphosed by a tendency of such a kind, operating during immeasurable ages. Now that we have reached it, however, the true cause turns out vastly more simple. We owe the discovery and exposition, to Dr. Tyndall, of whose exceedingly interesting memoir Sir John Herschel has worthily said, that since reading, a great many years ago, Wells's little tract on *Dew*, he had not met with anything that seemed to bring so many loose-lying phenomena under a general and very simple principle. It would be wrong, however, to omit mention of Mr. Sorby. This very acute and promising geologist had clearly approached the truth; but unfortunately he appeared to consider that a subordinate portion of the phenomenon plays a principal part. Dr. Tyndall's theorem is this:—*Rocks distinguished by planes of cleavage, have been subject to enormous pressure or squeezing in a direction at right angles to those planes.* He shows by irrefragable evidence, *first*, that such pressure has existed, and, *secondly*, that it suffices for explanation of the phenomenon.—(1.) The fact that pressure of the kind supposed, actually occurred, is manifested by the contents of the rocks with cleavage. These rocks, for instance, contain organic remains—the relics of shells, trilobites, &c. Now those relics are universally squeezed up in the direction of the foregoing plane of pressure, that is, in a plane at right angles to the plane of cleavage. But the condition of inorganic constituents of the rocks give corresponding and equally emphatic indications. Most persons will recollect those yellow marks that so often appear in school slates. Those and similar ones, sometimes of considerable dimensions, found in the rocks, appear to have been originally deposits of very fine mud, in globular or almost globular cavities. Now the section of those masses in the direction at right angles to the direction of pressure, remains circular, but in the plane of pressure the section is elliptical, *i. e.*, the globular masses have been pressed out into ellipsoids. In the same way—as Mr. Sorby has so well shown must be the case—the flat sides of all particles of mica, talc, &c., that are found in these rocks, lie in accordance with the cleavage planes, and therefore at right angles to the direction of pressure.—(2.) But while the reality of ancient pressure has thus been demonstrated, can that alone account for the existence of planes of cleavage? Rigorously defined lines of cleavage are merely lines of comparative weakness in the rock. Now the illustrations just adduced, show that all through the rocks subjected to such pressure there must be a plane of weakness situated exactly as above defined. It is clear that every compressible irregularity within the structure of the rock,—every discontinuous portion—every air-bubble, or water cavity, must be flattened and spread out in the direction at right angles to the direction of pressure. Dr. Tyndall takes the homely illustration of "puff paste." It is thoroughly to the point; for no cook can make "puff paste" well, unless, by pressure, the portions that will afterwards melt, are flattened and formed into thin cakes lying perpendicularly to the direction of the pressure. And thus, in the present, as in multitudes of other cases, the most puzzling and largest classes of phenomena, turn out, after all, nothing more mysterious, than operations unnoticed because familiar—passing hourly before our eyes! Dr. Tyndall refers to an illustrative experiment that must be felt as singularly conclusive. Taking a piece of wax, and using every means to make it as homogeneous as possible, he subjected it to pressure before his audience at the Royal Institution, and it came from the pressure with well marked planes of cleavage. And he used his theory most successfully to explain the exfoliating of railway bars and other analogous phenomena.—For instance, break a piece of ordinary iron, and you will discern that its structure is granular. If this mass be beat or pressed the granules are elongated; they yield and become plates. Hence exfoliation; the plates come off in leaves. In other words, the iron has got a *cleavage.*—Geology is thus not merely freed from an enigma; her inquirers may further conclude from the presence of slaty cleavage, that in former ages an enormous pressure, definite as to direction, must there have been at work.

Clepsydra or Water Clock. An instrument in which the efflux of water through a small orifice is taken as the measure of time. Two species of clepsydrae have been employed,—one wherein the fluid is allowed simply to pass through the orifice,—another where the level is constantly kept at the same height by the introduction of new water. The latter plan secures that the efflux of water be uniform. In the former case, it is clear that there is not the same weight of fluid pressing on that which issues at the orifice—when one-half (for example) has passed—as before. In consequence the velocity diminishes. If the water be pure, and the orifice very small, and quite clean, the law of efflux will be, that in $\frac{1}{m}$th part of the time of the whole efflux, the amount of the water that shall have passed is $\frac{1}{m}\left(2-\frac{1}{m}\right)$

Thus, in $\frac{1}{3}$ of the time, $\frac{5}{9}$ of the water will have passed; in $\frac{1}{2}$ of the time, $\frac{3}{4}$; in $\frac{2}{3}$, $\frac{8}{9}$ths; and so on.

It follows that the second method is the best for reckoning time. If we can keep the level constant, and at the same time not disturb the water, we shall evidently have a uniform flow of fluid, which may be measured by being received into a uniformly graduated tube.

The clepsydra is susceptible of very consider-

ble mechanical refinement; and may become n instrument of great delicacy. It is supposed › have been in use among the Chaldeans. Scipio Jasica introduced it into Rome. Tycho Brahe mployed it in his Observatory.

Climate. A word which, in its ordinary cceptation, signifies the general material relaions of any country or district of the globe—be nese beneficent or maleficent—to living beings vithin that region, or, more correctly perhaps, o *Man*. *Climate*, in this sense, comprehends, s Humboldt has said, "all those modifications f the atmosphere by which our organs are ffected—such as temperature, humidity, variaions of barometric pressure, its tranquillity or ubjection to foreign winds, its purity or admixure with gaseous exhalations, and its ordinary ransparency—that clearness of sky so important, hrough its influence, not only on the radiation f heat from the soil, the development of organic issue and the ripening of fruits, but also on the utflow of the moral sentiments in the different ones." To expiscate and declare the laws of he constitution of climate, under this wide cceptation of it, and to indicate the revolutions which actual climates have undergone, forms the bject of one of the most extensive of the departnents of existing physical research,—one that nust build on all the physical sciences as a oundation, and draw also from history and thnology,—a department properly termed *Climatology*.—To enable one to constitute or delinete the sphere of a distinct department of science, ne requires to obtain, not a vague or general, ut an exact definition of it; but this is precisely he thing that can never be obtained until a cience, being nearly complete, has surveyed its wn sphere and marked out its boundaries. At resent, certainly no more definite initiatory lefinition can be given, than the one suggested by he foregoing extract, which is the essence of all he writings of Humboldt on this subject, viz: hat *climates* ought to be considered identical or orresponding, which, in whatever latitudes or egions they may be found, offer the same or early the same conditions for the material welleing of Man.—Had our globe been a perfect phere, and with a symmetrical distribution of and and water, the boundaries of climates would have been marked out by parallels of Latitude. But it is not so. And since differences of *elevaion*; those contrasts which distinguish rigid natter from fluid, viz., the opacity, density, and cohesion of the particles of the former, n opposition to the transparency or permeaility to light, and the mobility of the particles of fluids; the unequal distribution of vegetaion, of still and running water, of winds and atmospheric currents; the diversity of the slopes or exposures of a country, as well as other minor circumstances;—all, may place countries of the same latitude under extremely various climatological conditions, it is very evident that mere *geographical* place is but one element—and the simplest one—towards the exact determination of a *climate*. As a first step in the discrimination of terrestrial climates, Humboldt proposed that *parallels of Heat*, as well as of Latitude, should be drawn across the globe; viz., *isothermal* lines, or lines of equal mean heat during the whole year; *isotheral* lines, or lines of equal summer heat; and *isochimenal* lines, or lines of equal heat in winter. The idea thrown out by this venerable physicist has not lain dormant. And M. Dové of Berlin has recently enriched climatology by traces of lines indicating the equal mean temperature of the several months. The study of those lines may be said to constitute the basis of all actual climatology: and the following are a few general results. The progress of *isothermal* lines across the two continents, shows that the climates of Europe are of a lower mean temperature, than those, either of central Asia or of America; and that, in general, the northern hemisphere receives more heat from the sun than the southern. This difference begins to show itself in parallel of Latitude 20°, and goes on increasing towards either pole. Between lat. 0° and 60°, the difference is not less than 16° of Fahrenheit.—In the same hemisphere and under the same Latitude, the annual heat diminishes rapidly from west to east, in the interior of continents, while it follows the inverse course in countries near the sea coast. Between the two continents in the same hemisphere,—between the climates of the east and those of the west,—an opposition or contrast exists, resulting doubtless from the opposing influences of *continental*, and *liquid* or *diaphanous* masses. The influence now alluded to, is still more clearly marked, in the distribution of heat through the seasons. In America the summers are burning, the winters rigorous, and the intermediate seasons very variable, compared with those in Asia and Europe; and this contrast holds also for Europe and Asia. The *isochimenal* lines from west to east, in Europe and Asia, assimilate countries apart by 9° or 10° of latitude; and in the opposite way, the *isotheral* lines, bring together, localities separated geographically by at least ten degrees of latitude.—Such being the state of inquiry as to *Climatology*, and the mode in which alone it can be conducted,—this article closes. But there are other two important inquiries, viz.: I. As to the distribution of Climates as they exist; and, II. As to Climates as they have been.

Climates, *Present Distribution of.* The force principally determining the distribution of climates over the surface of the globe is, of course, the obvious geographical one. Geographical climate ought, indeed, to be considered the *genus*, whilst varieties under it are mere *species*. Each hemisphere may unquestionably be divided into vast zones, that in the main have a certain homogeneity in this respect. The cir-

cumstances modifying the effect of solar radiation, however numerous, have after all only a restricted action; they give rise to local dissimilarities, *individualizing*, so to speak, each special region, and affecting the regularity with which the solar influence diffuses itself from equator to pole: nevertheless, observations show that for every ten degrees of augmenting latitude, there are on the whole diminishing *means*, both of summer and winter heat. Placing one's self in the centre of such a geographical zone, and neglecting the divergences arising out of mere locality, one cannot fail to discern the action of grand cosmical and climatological influences,—for instance, the same types of vegetation and animal life, and the same conditions of health or sickness as regards Man. Under the line and near the pole, these phenomena reach their maximum of contrast: at an equal distance from these extreme points, the effects alluded to balance and sustain each other—each passing into its opposite and *vice versa*. It is thus, not unauthorized by nature, that a distribution, now very old, is still received, viz., that the globe can be parcelled out into *Regions of Cold Climates*, *Regions of Hot Climates*, and *Temperate* or *Intermediate Regions*. Of these three, we shall speak in some detail; merely noting at outset two definite facts or laws respecting irregularity of climate, or their *difference in* SPECIES. *First*. This series of *hot*, *temperate*, and *cold* climates, which one finds on passing from *Equator* to *Pole*, is reproduced in every geographical region, in connection with variation of *altitude* or *elevation* above the level of the sea. Mountains like the Himalaya, Chimborazo, Lebanon, St. Bernard, &c., present these different climates, superimposed, like the storeys of an old Edinburgh house: nay, the effect of elevation has been computed; and in the main it may be said that an elevation of two hundred feet, deteriorates a climate, as much as a displacement of the locality would do, from equator to pole, of one degree of Latitude. There are thus *climateric zones of* ALTITUDE, as well as *climateric zones of* LATITUDE; nor are the former less difficult to define exactly than the latter, depending as they do on soil, vegetation, exposure, &c. Nevertheless, *general* laws exist. (See those graphic maps by Humboldt, Berghaus, and Johnston.)—*Secondly*. Within each of these zones there are two secondary divisions, depending on the number and extent of the vicissitudes of the atmosphere. (1.) Regions in the neighbourhood of large masses of water, those namely on the sea-coast, or which are bathed by large Rivers, enjoy an atmosphere comparatively free from great changes; *i. e.*, its temperature varies comparatively little, from morning till evening, from day to day, from month to month. On this account—mainly because of the neighbourhood of the sea—the equatorial zone from 0° to 15° lat., has a mean temperature no higher than 80°, except in the case of exceptional localities like Senegal. For the same reason, plants may be cultivated on the coasts of England and Ireland, that cannot be found in the interior of continents of much lower Latitudes. The *lines* of corresponding vegetation, delineated on the fine maps already referred to, afford abundant illustration of the truth now spoken of. All such regions are peculiarly favourable to *horticulture*; even when the heats of summer are scarcely adequate to ripen the fruit, the cold is not intense enough to kill the tree in winter. (2.) In contrast with the foregoing are *continental climates*; distinguished by the multiplicity and extent of their meteorological variations. Just because they are far from the equalizing influence of the ocean, differences of temperature, of the moisture of the air, of barometric pressure, are strongly marked, between morning and night, from day to day, and month to month. These continental climates, for instance, our *Canadian* one, have, not unduly, been termed *excessive* climates, or rather *climates of excesses*.—We shall now characterize in as few words as possible, the *three* regular geographical climates.

(1.) *Hot* or *Torrid Climates*.—The sphere of this class of climates extends from the Equator to 30° or 35° of north and south latitude. It comprehends nearly the whole of Africa; all that great division of Asia lying to the south of a parallel from Syria to Tonquin, and passing through Lahore and Boutan; and the large portion of America, between California and the southern boundary of La Plata. The perpendicular radiation of the sun, accumulates, in equatorial climates, the maximum of heat; the mean of the year in the shade ranging from 80° to 85°, while that of summer reaches 90°. The tropical regions of the old world are considerably warmer than those of the new; and in either case the heat diminishes as latitude increases, very slowly indeed compared with its rate of diminution in the temperate zones. Transitions of temperature during the day are rare also under the torrid zone, and of slight comparative amount, seldom surpassing from 14° to 16°; the frequency and extent of these, are less in the new than in the old world; near the sea, than more inland; and generally in westerly than in easterly regions. In no part of the world is the diminution of heat through *elevation*, more felt than in the equatorial mountains; and it is not without astonishment that an European finds himself passing in a few hours from burning plains covered with the banana and the cocoa, to sterile regions buried under perpetual snows. Speaking generally, the heat of the equinoctial plains keeps between 65° and 100°; but between day and night their temperature sometimes varies by 36°, owing to the intensity of terrestrial radiation under skies so exquisitely serene. Barometric oscillations are much better marked in the tropics than in our temperate zones. The periods of the diurnal oscillation are *4h.* 13*m.* and 9*h.*

3m. A.M. and 4h. 8m. and 10h. 23m. P.M., the difference between the maximum of the forenoon and the minimum of afternoon being very large. See ATMOSPHERE. Between the seasons of rain and those of arid heats, the intermediate periods are distinguished by extreme atmospheric perturbations. Autumn and spring, in the equatorial zone, resemble, in their meteorological versatility, the equinoctial periods of all countries; but they are of very short duration. The peculiar winds that influence the torrid zone further affect its climate: viz., the land and sea breezes, the monsoons, the trade-winds, the hurricanes of the Antilles, the tornadoes of the African coast, and the typhoons of the Eastern seas. See WINDS. This is not the place to speak of the effects of these tropic climates on vegetation and life.

(2.) *Frigid or Arctic Climates.*—These climates extend from lat. 55°, to the pole, and comprehend, in the north, Denmark, Sweden, Norway, Finland, Northern and Middle Russia, Siberia, Lapland, Iceland, Greenland, Kamtschatka, Spitzbergen, &c. The coldest point in the globe—not yet determined in the southern hemisphere—lies—in the north—at about ten degrees from the pole, north of Behring's Straits, and about east longitude 170°. The temperature of this point is = —10°; that of the pole is about +3°. Between latitudes 64° and 75° the mean temperature of spring is +3°; that of autumn about +10°; that of winter —22°; and that of summer +36°: results calculated by Foster, from the daily observations of Ross, Franklin, Parry, and Back. In these climates, spring announces itself by thin and flaky snow-storms, followed by plentiful rain and by westerly and southernly winds. Soon after, the ice breaks up, and masses of it detach themselves from the coasts; winds and currents tear them away, and a *debacle* takes place, in the midst of dense vapours that darken the atmosphere. The temperature rises from May to July, during which period there are few storms. The actual summer does not overpass this date. Its mean temperature is only 36°, and its extreme heat only reaches 60°; the causes which enfeeble the usual benefits of summer, being the obliquity of the solar rays, the coldness of the winds blowing from the pole, and incessant evaporation. During the month of August, after some rain, the earliest snows fall; the thermometer descends rapidly; and in November the accumulations of ice in straits and passes, again oppose an insurmountable obstacle to navigation. Then begins winter, a season very rigorous without doubt, but which meteorologists have usually painted dark in excess. Between latitudes 70° and 78° the mean temperature of the year is not lower than between +17° and +19°, although the extreme of cold reaches —70°. These terrible colds are felt in January and February, an epoch at which the polar winter is at its depth; then over sea and land, snow stretches far and wide; glacial vapour disengages itself and mingles with the air; the sun, concealed below the horizon, reveals his existence by a mere twilight whose calorific effect is *nil*. The Aurora, by its magical illumination, somewhat supplies, indeed, the place of our luminary; but its brilliancy, unaccompanied by heat, in no wise mitigates the sharpness of the winter. Diurnal variations of the thermometer in these hyperborean climates do not amount to much; but the annual variations are very great. The changes of the barometer are quite in contrast with those affecting this instrument near the equator: beyond latitude 60°, the *periodical* variations disappear; while the *general* variations augment with the latitude. With the exception of the Aurora, there are no traces of electric phenomena in high latitudes. Winds blow there usually from north-east or south-west, suddenly veering from the one point to its opposite. In some localities, especially Spitzbergen, the southern winds are colder in winter than the northern ones. By the impulsion which the winds communicate to the air, they increase the sensation of cold—rapidly renewing the air in contact with our bodies; and by their irregularities, they frequently produce tempests that spread widely around. Rain is, of course, comparatively little known in the circumpolar zone; it falls in the form of snow or hail, highly crystalline in winter, somewhat softened and moistened in spring. The hail of such regions is simply frozen rain; and is not due to electric action, nor accompanied by electric phenomena. The general character of polar climates is necessarily determined by the duration and intensity of their winter. Beginning in October, it may be said to prolong itself into May; so that the other seasons run through their course in a few weeks: here summer is neutralized by nocturnal frosts and the lowness of its thermometric mean; spring and autumn, remarkable for a humidity that augments the persistent coldness of the air. Under the glacial zone, vegetation can scarcely be said to exist. Everything in the animal and vegetable world presents a dull, feeble, and monotonous aspect.

(3.) *Temperate or Intermediate Climates.*—These fortunate climates range from 30° or 35° to 55° north and south latitude. Almost the whole of Europe with its islands belongs to them; in Asia they embrace the fair and vast countries, stretching from the Euxine and the Mediterranean, to the empire of Japan and the great ocean on the south and east; the southern part of Africa is under their influence; and in America they include California, parts of Mexico and Canada, the United States, Chili, and Patagonia. The leading characteristics of the temperate zone are these. 1. The seasons are well marked; cold and heat alternate annually, each reaching its apogee, through a gradual transition from the other. Meteorological observations made in 26 stations, in the temperate zone, differing

in height and exposure, furnish as the annual mean temperature of winter, 26°; of summer, 68°; of spring, 62°; and of autumn, 53°. 2. Although distinct, the seasons are very variable; while within equatorial and circumpolar regions, they are distinguished by a remarkable stability or evenness of temperature. 3. Those oscillations of temperature which are little perceptible from one day to another, in the other zones, are strongly felt in temperate climates from morning to evening, from week to week, month to month, and season to season. It is a rare thing that the thermometer remains at the same degree for five or six days at a time: within that interval it may indicate vicissitudes of 18°, 27°, or even 36°; the scale of the entire variations is not less than 54° or even 72°, while at the equator, thermometric changes do not exceed 14° or 16°. The annual periods of chief versatility occur about the equinoxes. Near the vernal equinox, the mass of the atmosphere is agitated by gales blowing from all parts of the horizon, while both thermometer and barometer undergo sudden changes. The early portion of spring thus participates in the qualities of winter. Summer has three phases: the *first*, cold as the spring; the *second*, distinguished by a sustained elevation of temperature and dryness and purity of the sky; the *third* generally bringing with it electric disturbances, and leading in the autumnal equinox, marked like its predecessor by alternations of calm and storm, rain, fogs, and dew. In winter the atmosphere becomes steady and serene; rains succeed and alternate with snow; the sun is low in the sky, and the nights are long.—Such the features of the centre of the temperate zones; towards either extremity, they merge, of course, nearer to the climates of the equatorial or polar regions. It is in this zone that the influence of *locality* is the most sensible. According to the exposure of a place—according as masses of mountains shelter it—according to the neighbourhood of lakes, morasses, or sandy plains—its climate may be infinitely diversified; it may offer conditions totally different from those of surrounding localities, and rather analogous to what is found in countries more southern or northerly.

Climates, *Ancient.* The climates of various places of the earth were, at one time, quite different from what they now are,—probably of the whole earth. For instance, in coal fields in arctic regions, we have relics of what are now purely tropical plants. It is clearly enough indicated by geological research, that throughout those immense pre-historic, or rather, pre-Adamitic periods, there has been a succession of varying climates over the greater part of the terrestrial globe; and that the main feature of this change has been a diminution of heat, since early geological epochs. Various causes have been assigned for this remarkable change, to examine which fully, does not belong to a volume entering so slightly into geological and cosmogonic discussion. It is necessary, however, to allude to them. —1. The gradual alteration or *degradation* of the climates of the earth, has been attributed to the gradual cooling of the earth, or to the recession of what is called the central molten part of the earth's mass. It is not proved, however, that there is any such central molten mass, or that there has been this gradual cooling. Physical considerations are far from sustaining such an hypothesis: further, it is not consistent with certain astronomical phenomena, viz., the precession of the equinoxes: neither is it established that, from old epochs, there has been any, general, *gradual* degradation of climate. —2. The hypothesis has been started, that the axis of the earth's rotation may have altered;—the present polar regions having been, through effect of such alteration, formerly equatorial ones, and *vice versa.* This hypothesis is not only gratuitous, but inconsistent with every known law. *The axis of the earth's rotation has never altered.* —3. An explanation of a different kind has been proposed by Sir Charles Lyell. He attributes climate mainly to the distribution of land and water over the surface of the earth. And, since continents and oceans have often interchanged places, he conceives that regions, now arctic, may, on this ground, have been torrid. The cause is a *vera causa*, but not a *sufficient* one. It involves the fallacy of explaining differences of *genera*, by the causes of difference of *species.* No conceivable distribution of land and water could so far abrogate the efficiency of the Sun as to render possible, tropical plants in Melville's Island.—The difficulties involved in the problem are great, and certainly they are not overcome as yet.

Clouds. Respecting the formation and constitution of Clouds, much still remains to be investigated. In regard of constitution, the *Fog* and *Cloud* are identical: nor do they differ in anything save this; the *Fog* is a *Cloud* resting on the surface of the earth, while the *Cloud* is a *Fog* floating in the Atmosphere. Intending to advert especially to Inquiries concerning the origin of these *visible vapours*, under article Fog, we shall merely state here, that the formation in question is essentially distinguished from that of Dew, by the circumstance that while Dew falls or is precipitated, because the surface on which it falls is *colder* than the Atmosphere, Fogs rise and are diffused, because the surfaces from which they rise are *warmer* than the atmosphere. The formation or source of the former is the aqueous atmosphere itself; the source of the latter the moisture in the ground. — The *structure* of the particles or molecules of fog and cloud has been pretty closely examined; and the prevalent opinion is that they are not solid drops, but thin vesicles or air-bells, of the character of soap-bubbles; the collapse of which yields rain. The diameter of these vesicles varies *generally* with the monthly temperature;

reaching its *minimum* of about ·0006 of an inch in August, and its *maximum* of about ·0015 in December: the variation, however, is not regular, as another apparent minimum seems to occur in May. This vesicular constitution of the particles of clouds, might promise a solution of the puzzling question, why they float and even ascend; nor have conjectures been wanting that they may be filled with some light gas, after the manner of balloons. Chemical analysis has disproved all such hypotheses: and there is no need of them; for, on the application of known causes to the question, the difficulty will be found very nearly to disappear. In the *first* place, the velocity of the fall of such a vesicular particle, through the air, cannot be very great; just because, owing to the extreme tenuity of the aqueous envelope, the specific gravity of the particle can scarcely be appreciably greater than that of air. *Secondly*, a cloud that seems motionless, is often really *falling*. It must be recollected that the moment these vesicles come into contact with dry air, they dissolve and disappear; so that to a descending cloud, such a change as the following may frequently occur,—its lower part may continually be dissolving, while the upper is being continually increased by the addition of new vesicles. And, *thirdly*, an atmospheric force exists, directly opposed to the fall of clouds, viz., *ascending currents*. During fine weather the vesicle falls with a much less velocity than that of the ordinary ascending current; which therefore draws it upwards along with it. On this account *cumuli* are more elevated at midday than in the morning; towards evening, on the contrary, as the current becomes weaker the clouds descend and dissolve when they reach the warmer region of the atmosphere:—hence that remarkable transparency of the sky which often follows even a cloudy day. Horizontal currents also oppose the fall of clouds. — Referring again, as to the *genesis* of these vesicles, to article FOG, we shall proceed with an account of the Natural History of Clouds, and of their main functions.

(1.) *Clouds, Forms of.*—A classification of Clouds according to their chief forms, was first proposed by the meteorologist Howard; and the one he gave, retains its place. He distinguished these masses into three main forms, the *cirrus*, the *cumulus*, and the *stratus*; to which he attached as subdivisions — the *cirro-cumulus*, *cirro-stratus*, *cumulo-stratus* and *nimbus*. The *cirrus* (the sailor's *cat's tail*) is composed of thin filaments, the association of which resembles sometimes a brush, at other times masses of woolly hair, and again, slender net-work. The prevalence of the *cirrus* constitutes the *mackerel sky*. The *cumulus* or summer cloud (the sailor's *ball of cotton*) appears as a large massive hemisphere resting on a horizontal base. Such hemispheres are often piled on each other, forming those great and gorgeous clouds that sometimes accumulate in the horizon, and present the aspect of remote gigantic mountains crowned with snow. The *stratus* is a horizontal band, forming generally at sunset and disappearing at sunrise. Howard's subdivisions explain themselves; nor is any remark necessary here, except that the *cumulo-stratus* often assumes at the horizon a black or bluish tint; then passing into the *nimbus* or rain-cloud. The *nimbus* is distinguished by its uniform gray tint and its fringed edges; the clouds of which it is composed, are mingled so as to be indistinguishable. Of these various shapes of clouds, the *cirrus* and the *cumulus* are in all respects the most striking and important. After a period of continued fair weather, when the barometer begins to fall slowly, *cirri* are seen to appear, sometimes under the form of slender filaments, and on other occasions, as parallel bands arranged in obedience to some cause yet unknown, from South to North, or from South-west to North-east. Of all clouds the *cirri* are the most elevated, they are so high, that from the summits of the loftiest mountains their appearance is the same as when we look at them from the plains. *Halos* and *parhelia* are formed among the cirri exclusively; nor, if they are examined carefully by aid of a blackened mirror, does one almost ever fail to find amongst them, distinct traces of halos. But as these phenomena are due to the refraction of light by frozen particles, it would therefore seem likely that *cirri* are themselves composed of snowy flakes, floating in the atmosphere at a great height. The appearance of these clouds frequently precedes a change of weather. In summer, they announce rain; in winter, frost or snow. Even when the vane is turned towards the North, the *cirri* are often carried along by South or South-west winds, which soon descend and are felt at the surface. On the occurrence of that descent, the *cirri* become more and more dense, because the air is moister; they then pass into the cirro-stratus, and in a short time rain is precipitated. These same circumstances sometimes determine the formation of a light and wholly vesicular cirro-cumulus. This is the cloud which on passing before the sun or moon, causes these luminaries to be surrounded by *coronæ*. The cirro-cumulus foretells heat. While the foregoing clouds are the produce in our northern zones of the South wind, the *Cumulus* owes its existence to ascending currents: its elevation is not nearly so great as that of the cirrus. *Cumuli* are characteristic of the finest days of summer; they appear in the horizon shortly after the sun has risen; their number and size increase till the hour of greatest heat; after which they diminish; and in the evening, the sky is again serene. They are formed as already hinted when ascending currents draw the vapours into the higher regions of the atmosphere; regions in which the cold air is rapidly saturated. If the current increases in force, the vapours and clouds become more elevated; and as they penetrate in this way into colder and colder regions, it occurs that the sky

—although fine in the morning—becomes quite clouded by twelve o'clock. Saussure has endeavoured to account for their rounded form by this mode of formation: it is undoubted that when one liquid traverses another, the former must, because of the reaction of the ambient medium and the mutual resistance of its parts, take on a cylindrical form with a circular section, or one composed of arcs of circles. Although *cumuli* generally disappear in the evening, they sometimes do not. In the latter case, they become, on the contrary, more numerous, their borders are less brilliant, their tint deeper, and they pass into the *cumulo-stratus*, especially if a stratum of *cirrus* happens to exist below them. This arises, because the higher and middle regions of the air are near the point of saturation; and rains and storms may then be expected.—See RAIN and HYGROMETRY.

(2.) *Clouds, Distribution and general Functions of.*—It were needless to endeavour to present any general view of the distribution of clouds over the whole surface of the Earth, inasmuch as the clearness or cloudiness of the sky is so largely influenced in every particular case by the nature of the soil—its power of absorption and radiation, its dryness or humidity. One grand fact, however, must be adverted to. Around the equator, and stretching a few degrees on each side of it, there is a vast *cloud-ring*, not precisely steadfast, but passing sometimes to the north, sometimes to the south—always existing, however, and nearly permanent as to its breadth. Almost all good voyagers have noticed it. The following extract from a log-book, by an American Captain, describes it as graphically as any. On entering the parallel 4° North Lat., Captain Sinclair writes:—"This is certainly one of the most unpleasant regions in our globe. A dense, close atmosphere, except for a few hours after a thunder-storm, during which time torrents of rain fell, when the air became a little refreshed; but a hot glaring sun soon heats it again, and, but for your awnings, and the little air put in circulation by the continued flapping of the ship's sails, it would be almost insufferable. No person who has not crossed this region can form an adequate idea of its unpleasant effects. You feel a degree of lassitude unconquerable, which nothing can dispel. After crossing the line I found that I had now surmounted all difficulties, and that the breeze continued to freshen and draw round to S.S.E., bringing with it a clear sky and most heavenly temperature—renovating and refreshing beyond description. Nothing was now to be seen but cheerful countenances, exchanged, as if by enchantment, for that sleepy sluggishness which had borne us all down for the last two weeks." The ring-cloud is coincident with the region or zone of Equatorial Calms; and something of the same sort distinguishes the calm-zones of the tropics of Cancer and Capricorn. Nor is it difficult to detect the physical cause of that connection. The vapours that ascend in such masses, under the direct heat of the Sun, from the surface of the Earth in the Equatorial regions, condense into clouds in the upper atmospheric strata; and, as they are not swept away by winds, they hang over that belt of calms, and shield the Earth below it. Evaporation is thus to a large extent stopped, the Earth's surface protected from the torrid beams of the Sun, and the Equatorial zone rendered habitable. Precipitation, too, becomes not only possible, but plentiful; and underneath that cloud belt, we have the Equatorial Rains. From 5° South Lat. to 15° North Lat., the beneficial action of this cloud belt prevails; and its existence farther vastly modifies the meteorological habitudes of the more northern and more southern zones. But for the effects of these circles of calms, we should have had a really torrid or burnt-up equatorial zone on the one hand, and in the temperate regions perpetual precipitation or rains. Over all the Earth, too, we can trace effects of the clouds; not so striking, perhaps, but altogether similar. To view them as the mere sources of rain were wholly inadequate. They act still more remarkably in tempering the solar heat, and checking nocturnal radiation. A few cloudless days in summer, suffice to inform us how insupportable, anywhere out of the Polar Circles, were the direct heat of the Sun; and, even in summer, perpetual nocturnal radiation, under an unclouded sky, would change and deteriorate all our climates. Absence of cloud in it, and of course radiation unchecked, is one of those many considerations which induce us to suspect that our satellite, the Moon, has not yet reached those physical conditions which are consistent with the presence of organic life.

(3.) *Clouds, Electricity of.*—The question concerning the Electricity of Clouds, as well as that concerning the general Electricity of the Atmosphere, is still under keen debate. Of the conflicting opinions of Lamé, Becquerel, and Peltier, we shall here give those of Becquerel; meaning to refer pretty fully to the researches of Peltier under article FOG. When the sky is not clear, the electrometer sometimes indicates the presence of positive Electricity in the Atmosphere, sometimes that of negative; nay, if these indications be obtained by means of a kite, it is found that the electricity manifested, frequently changes its character, as the kite passes from one cloud to another. For instance, Peltier tells us that at one time, by means of a kite, he found the atmosphere *positive*, through the first zone of 60 yards; above that, of the thickness of 25 yards, lay a *negative* zone; while beyond that space, *positive* indications reappeared. It may be taken as universally true, indeed, that the clouds are in an electric state, only of less tension, than when storms are the consequence. And, because of this electric state, they are variously affected by the mountain masses that rise up amidst them.

To this cause, in all probability, must be referred ·ffects observed among the Andes by Boussin-gault, viz., parasitic clouds of immense extent ıttaching themselves, so strongly to the *middle* portions of those vast trachytic domes, that the wind could not separate them,—lightning darting rom this mass of vapours, and rain, mixed with ıail, pouring down from them on the plains be-ow. Let us reflect, however, on the causes of hese different electric states. (1.) The *positive* ·lectrization of clouds can be explained without lifficulty. If a cloud is formed amidst a tranquil ıtmosphere, in its natural or positive electric tate; it is clear that the same condition of elec-ricity will characterize each of these vesicular ;lobules,—the aqueous envelope of which must be egarded as a good conductor. When the elec-ricity is feeble, and the vesicles considerably part, nothing remarkable will follow, and the loud will not pass into the condition of a storm-loud; it will merely appear somewhat more ighly electrified than the surrounding air, be-ause it is a better conductor. If, however, the loud is dense, and its globules close on each ther, it may be regarded as a *continuous con-uctor;* and all the electricity of its interior, will, ccording to the laws of electric distribution, col-ect itself on the cloud's surface, where it will be etained by the pressure of the ambient air. A torm-cloud, thus formed, ought manifestly to ontain as much electricity as the whole mass of ir that furnished its vesicles; so that, however eeble the general atmospheric tension at the ime, it is easy to see that, as the cloud occupies large space, the electricity at its surface may cquire an enormous tension, and be prepared to ake part in the stupendous action of a thunder-torm. Clouds formed in the ordinary state of he atmosphere must therefore be positively elec-rified, and will continue so if they remain at est. Driven onwards by the wind, they lose heir charge during their course, and cease to be torm-clouds. (2.) It is not so easy to appre-end the origin of the *negative* electricity of louds. The most probable cause is this:—ising directly from the ground, these Clouds ansport upwards the *negative electricity of the arth.* It is well known, for instance, that the queous mist arising at cascades is *negatively* ectrified; as in the same way, the spray at the lges of any torrent. Now Saussure proved, by ımerous experiments, that mists rising from the ound about sunrise, give precisely the same indi-ations; and he concluded, generally, that all ıpours formed on the globe's surface carry up a rt—greater or less—of the negative electricity hich the Earth usually possesses. It is not impro-ble, also, that clouds become charged negatively rough effect of *influence* or *induction.* The Earth elf, for instance, sometimes becomes positively arged in this way, by agency of a cloud strongly ectrified negatively; and there is no reason to lieve that a dense cloud, of powerful positive tension, may not act similarly by distant ac-tion on another cloud, either feebly electrified or not electrified at all, and in communication with the Earth. The positive electricity of the latter cloud would, in such a case, be driven off into the earth, and a negative charge developed in that portion of it nearest the former. Should communication with the Earth now be cut off, this cloud would continue in a negative state. The same kind of action, might bring about simi-lar results, as follows:—The Earth always ex-ercising a negative influence, and the Atmosphere a positive influence, on Clouds, any one of these floating near the Earth must be much more ne-gatively inclined in its upper than in its inferior portions; but an increase of temperature might dissipate its lower strata, and in this case a cloud electrified purely negatively would remain. —From this mode of the formation, alike of posi-tive and negative clouds, it may be inferred, as a general conclusion, that the condensation of vapours is always accompanied by an evolution of electricity, and that rain should, in all cases, manifest signs of it. (3.) Storm-clouds are found at all *elevations.* Rocks forming the culminating points of the loftiest mountains frequently bear marks of fusion and vitrification that can only be attributed to lightning. On one of the rocky pinnacles of Toluca, which project 15,000 feet above the level of the sea, Humboldt records that he noticed such effects; and Arago informs us, that clouds of high tension have been discovered, 26,650 feet above the low lands of the temperate zone. It is clear that they attain the highest elevations, since electric indications so frequently appear among the *cirri,* which, as we have seen, essentially belong to the regions of perpetual snow. Generally speaking, however, these clouds are comparatively low; the situation from which thunder proceeds, sinking to the distance of 5,000, or even 3,000 feet, from the plain. See THUNDER-STORM.

(4.) *Clouds, Colour of.*—The varied and eva-nescent colouring of the clouds—depending on the relation between light and those vesicles—has not yet been fully explained or even closely investigated. A few special remarks must suffice here. It is pretty evident that in all ordinary circumstances the *red* rays pass through those vesicles with greatest facility, and therefore in greatest abundance. Hence the facts, that when the air is filled with such molecules, the part of the sky below the sun is usually of a reddish hue—and that after sunset the region of twilight is of a deep carmine colour. In winter in our latitudes, this redness often prevails throughout the whole day; and in summer, when *cirri* are floating about, the same occurs several hours before the culmination of the sun. When, on the contrary, the sky, during day, is deep blue, the twilight presents a gorgeous yellow, or gol-den tint. The red colouring of the clouds is connected with a phenomenon often seen in moun-

tainous regions, called the rose-tint of the Alps. This occurs chiefly when *cumuli* or *cirro-cumuli* are floating in the west: the bare surfaces of the rocks then resemble masses of incandescent iron. The cause is, that reflected red rays are reaching the eye in greatest number, and these red rays strike on the rocks from which they are reflected, after passing through the atmosphere. See WEATHER.—Peltier would have us believe that clouds charged with negative electricity prefer the shades of slate-gray; while white, red, and orange coloured clouds are positively electrified—positions which as yet are certainly not established. But the whole subject remains in a crude state.—The reader is referred to instructive papers by Professor J. D. Forbes, in the *London and Edinburgh Philosophical Journal.*

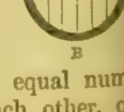
Coffer-dam. A contrivance employed in the construction of bridges. It consists merely in a double row of piles filled up between with sand and earth, and surrounding the place where the piers are to be built. It is intended to prevent the disturbance of the work by the water.

Cohesion. All perceptible bodies are supposed to be made up of a very large number of smaller bodies or atoms, placed side by side. It is evident, however, at first sight, that though this supposition accounts for a body's mere existence, it does not account for the various phenomena resulting when force is applied to it. Suppose a piece of stone, of irregular shape, composed of a very large number of these atoms; when the force of gravity acts upon them, those not at the ground, or vertically above the particles at the ground, would fall, if the stone were made up of a mere aggregation of particles. Take a handful of sand and attempt to shape it like the stone. It is found to be impossible. Yet in this case, we have the atoms present as before, and they are placed near one another as before. Moisten the sand with some water, and the body may now be moulded. Why? Gravity will not now disintegrate the mass. For what reason?—Wherever no motion is produced, and when a certain force acts upon a body, one conclusion is inevitable, that a force equal in amount to the first, must be acting on the body in the opposite direction. Gravity produces no effect in this case, and there must therefore be a force acting opposite to gravity, holding up the particles of the body. If we apply a set of forces equal to those of gravity in the case of the dry sand, and in the contrary direction, we shall find that there is a similar equilibrium; and we infer from this a force equal to gravity, and in the line of its direction. Similar forces will be found acting in every direction, and they may bear any relation, according as the body differs, to the force of gravity. If the body be at any part broader than its base, then the forces we speak of must be more powerful than Gravity. It follows then that we have in certain bodies, forces binding their constituent particles one to another, preventing separation, and resisting separating forces applied in any direction. These forces are termed forces of *cohesion.*—In the case of which we have spoken, these forces acted between two different bodies. In that case their specific name is *adhesion.* See ADHESION. It is a kind of cohesion. We have no physical ground for supposing any difference in origin or nature, between the two kinds of forces. *Cohesion*, when employed as a specific name, is taken to signify the force which binds the particles of the same substance, one to the other. Thus, in the case of the stone, the forces which resist its disintegration, acting between the same kind of particles, are termed cohesive forces. —There are three principal conditions, under which bodies are found to exist. These are the solid, liquid, and gaseous. These states depend upon the relative amounts of the force of cohesion and the force of heat. The latter acts as a power repelling the particles of matter, one from the other. When, therefore, this force becomes greater in any case than the cohesive force, the body in question melts, and then volatilizes. In the solid state, the excess of the cohesive over the repulsive forces, is sufficient to counterbalance strains tending to disintegrate the body. In the liquid state, the cohesive and repulsive are considered equal; and hence the force of gravity is unbalanced, and liquids always sink to the lowest possible level. In the case of gases again, the force of heat is supposed to be greater than that of cohesion, and the particles of the body fly off, separating from one another with considerable repulsive force.—About the nature of the cohesive force, and the mode of its operation, nothing at all positive is known. One thing only appears, that it acts at insensible distances, and produces no effect in the majority of cases, until the particles upon which it acts, are brought within such distances. Probably there is a positive repulsive force acting before this, to which, when its results are most evident, we give the name of Elasticity. The whole subject is involved in much obscurity. See STRENGTH OF MATERIALS.

Cold is merely the absence or negation of heat. Disputes were for a long time prevalent as to which had a real existence, cold or heat, if not perhaps both. The acceptance of the theory, that heat is not matter, finally set the question at rest.

Collimation, *Line of.* A very important technical term, connected with the use of Instruments, in Practical Astronomy and Geodesy. In the field of view of a Telescope, there is generally a system of spider threads, placed, or meant to be placed, exactly in the focus of the object-glass and eye-piece. Viewed through the eye-piece, they present an appearance like that annexed, viz., a central vertical line, A B, with an equal number of others equidistant from each other, on

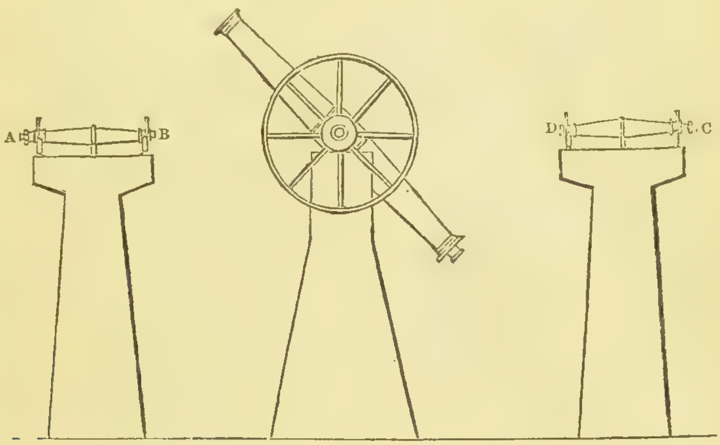
A

C  D

B

ch side of it; and a central horizontal one, C D. ow, if line A B be exactly in the vertical *middle axis* of the telescope, the instrument is said be collimated vertically; and, in the same way, C D be in the horizontal *middle* or *axis* of the :ld of view, the instrument is said to be correct to *horizontal collimation*. But as neither the ıe nor the other adjustment is ever perfect, there e, in every telescope—when one aims at the last :actitude, an *Error* of *Collimation*, which must be lowed for in every observation. See CORRECTIONS. The direction and amount of this error are best detected by the use of the subsidiary instruments named COLLIMATÓRS.

Collimators, *Vertical and Horizontal.* Subsidiary telescopes, by whose aid, the vertical and horizontal collimation errors of great Astronomical Instruments, are now universally determined. Their nature and importance will appear by aid of the annexed cut, which gives a side view of the interior of the transit room in the Observatory of Glasgow. The central figure is



:e large Transit circle of Ertel, already deribed; and one main object of the subsidiary struments A B and C D, is to assure that its great lescope be vertically and horizontally collimated. he collimators are two telescopes of considerable ze, placed horizontally on the tops of adjacent llars, in the meridian line. It will be seen that ıey look into each other—*down each other's throats* -as the phrase goes, and that the main telescope ın look into both. Suppose, in the first place, ıe main telescope removed or lifted up; each ›llimator can then see from its eye-piece, not ıly its own system of spider threads, but also ıe system or diaphragm in the focus of the :her. It is manifestly easy in such a case to ›rrect both vertical and horizontal collimation, :t us say, of telescope A B; for if its diaphragm e brought into coincidence with the diaphragm f C D, a semi-revolution made in its wires, ought ot, if there be no collimate error, to disturb that ›incidence. If the coincidence does not hold fter this semi-revolution, there is error in the osition of the diaphragm of A B; and it must be ıoved, by the appropriate screws, until it shall e found to retain its place in reference to the iaphragm of C D, in both positions. Exactly in ıe same manner the diaphragm of C D may be ›rrected. This accomplished, and the central ertical wires or threads of the two diaphragms rought into absolute contact, the main telescope replaced, and the observer looks through it, in ıccession, on the two diaphragms of the collima›rs. On looking down A B, the central vertical ıread of the main telescope may be found either to coincide with the central vertical wire of A B, or to be at a certain distance, let us say, *west* of it. If, on turning the telescope round, so that it looks down C D, its central thread be found, in the former case, coincident with the central thread of C D, or, in the latter case, at the same distance, *east* of it, there is no error of vertical collimation. But if these conditions are not fulfilled, an error exists, and must be either corrected mechanically by the screws or measured and allowed for. The Horizontal Collimation error of telescope may, by aid of these subsidiary ones, be also corrected; although this requires a reversal of the main Instrument, by the aid of the lifting machine.—To the Collimators, delicate Levels are attached, so that their deviation from absolute horizontality can readily be detected. These Collimators also enable the observer to dispense with the old and inconvenient arrangement of a remote *meridian mark*. Captain Kater was one of the earliest to propose the use of Collimators. One of his devices was, a vertical one —or a small telescope kept vertical, by resting, on a float in mercury. This is now dispensed with, chiefly through the extensive use of two horizontal Collimators, and of Bohnenberger's method of fixing the nadir point by reflection. See CIRCLE.

Collodion. A solution of the recently discovered substance, gun cotton, in ether, is so named. It is easily prepared, by immersing a greater or less quantity, according to the viscidity of the required solution, of common gun cotton in a mixture of one part of alcohol and two of sulphuric ether. From four to twelve

grains of cotton to the ounce of this mixture will give sufficient range of viscidity for most purposes. Collodion has chiefly, hitherto, been used in Medicine and Surgery, and in Photography. It forms an excellent covering and protection for wounds, more especially for parts from which the skin has been removed. It is in use in the dissecting-room as a protection, in the case of punctured or wounded hands, from the virus of the putrifying body. It also acts well in allaying pain in certain kinds of burns and scalds, and is useful as a styptic in arresting bleeding. In all these cases it acts, by the rapid evaporation of the solvent, leaving a continuous coating of cotton as a thin but air-tight covering, more closely and gently applied than could be effected by almost any other means. Its photographic uses are described under the head of—

Collodion Iodised. The iodide of silver, as is now well known, possesses the remarkable property of being rapidly decomposed by light, and, when arranged as a surface on which the image in the camera obscura is thrown, becomes the most valuable of all photographic agents. According to the method invented by Daguerre, and known by his name, the iodide of silver is formed at once as a thin dry film on the surface of a silver plate, by its exposure to the vapour of iodine. On account of the expense of silver plates, the very perishable and delicate nature of the picture, and the objectionable glare of the metal, it becomes desirable to obtain the sensitive coating on some other than a metallic surface. This Mr. Talbot and others have admirably accomplished on paper in the Calotype, for an account of which the reader is referred to the article CALOTYPE. There is, however, much difficulty in getting paper sufficiently uniform in its texture to produce negatives perfect enough, from which to print the pictures by the second part of the process. For this reason Sir John Herschel, several years ago, tried to spread the coating of iodide of silver on glass, but his success was only very limited, on account of the difficulty of getting a suitable medium wherewith to attach the coating to the glass, so as to enable it to be subjected to the processes of development and fixing. This was subsequently done, with great success, by the use of the albumen of eggs, and the results were most admirable. Certain difficulties of manipulation prevented the albumen process from becoming very general, more particularly as the substitution of collodion in its stead at once offered greater facilities—facilities, indeed, so great and important, as to constitute the basis for an extension of the photographic art to a degree formerly unthought of.

The purpose of this, necessarily short, article will be best served by first indicating a simple and effective mode of preparing the collodion—then of iodising it—subsequently relating the steps of executing the plate, developing the picture, and fixing; after which, some of the causes which have been found, by the writer, to be the most common sources of failure in this delicate though simple operation, will be detailed. To prepare the cotton, take of strong sulphuric acid 60 parts, by measure, of nitric acid, 30 parts, of fuming nitric acid, 15 parts. Mix together, and immerse in it 12 grains of Swedish filter paper, or of fine cotton, to every measured quarter of an ounce of sulphuric acid in the mixture. Allow the paper or cotton to remain in the acid twenty minutes; then remove the cotton or paper by means of a glass rod, and wash well in five or six waters till all acid taste is removed; dry it in a heat little raised above that of the air. Care is to be taken that the fumes from the acids be avoided by the operator, as they irritate the lungs. —Let now one measure of alcohol be mixed with five of sulphuric ether, and add to each ounce four grains of the prepared cotton or paper, which, if it has been properly treated, ought to dissolve completely in a few minutes. It is now to be iodised as follows:—A saturated solution of iodide of potassium, in alcohol, is to be made and filtered, and added to the collodion in the proportion of two fluid drachms to each ounce of the collodion. The mixture may be used immediately; or, what is perhaps preferable, it may be allowed to stand for a day, to enable the sediment to subside; but it should not be forgotten that, day by day, it loses somewhat of its sensitiveness, and that, in the course of two or three weeks, it becomes nearly useless.—Supposing now that the glass plates have been cut to the size of the recess in the camera slide, they are to be rubbed over with and allowed to soak in a mixture of one part of nitric acid and six of water, and afterwards well washed in plain water, and dried with two or three successive bits of cotton, or still better, of linen cloth, until, when well breathed on, they show no signs of streaks or marks. The dust having been blown from the surface of the glass, the corner is to be held between the finger and thumb of the left hand, while the right holds the bottle and pours freely on the upper level surface a sufficient quantity of the iodised collodion. The plate is to be gently inclined from side to side, till the whole surface is covered, after which all that will run off is to be poured back from one corner into the bottle, taking care immediately to vary the direction of the vertical lines on the plate, to prevent the ridgy streaks which are apt to happen from the running collodion. From the very volatile nature of the ether, this process of coating the plate ought not to occupy more than a few seconds.

A few seconds more having elapsed, to allow of the partial drying of the film of exquisitely fine iodised paper, which has thus been spread on the plate, it is now to be immersed in the silver bath. For this part of the processes, as well as those which follow the exposure in the camera, darkness—except the somewhat distant light of a single candle—is necessary. The silver bath is

solution, of the strength of thirty grains of crystallized nitrate of silver, to an ounce of water (distilled water being preferable), as much of the solution being used as to fill a gutta percha or crockery ware tray, somewhat bigger than the plate, to a depth of an eighth of an inch. One edge of the plate is made to rest on the bottom of the tray at one end, while the other edge rests on a small gutta percha hook held in the hand. It is then, steadily and without stopping or hesitation, allowed to descend on the surface of the silver solution in the bath, where it is to be left half a minute, when it is to be steadily lifted to its edge again for a few seconds, to allow the evaporation of the ether; after which it is again lowered down for another half minute or so and this repeated till the coating of iodide of silver appears flat, and perfectly free from all oily looking markings. It is now to be lifted from the bath, the drops of silver solution hanging to the border wiped away by blotting-paper, and it is ready for the camera slide. The time of exposure will depend on the strength of the light, and on the exposed aperture, and the length of focus of the lens. Experience alone can enable the operator to judge of this, periods of from one second to five or ten minutes being required for different circumstances.

The surface of the plate, when removed from the slide, shows no appearance of a picture; only an incipient reduction of the iodide to the state of metallic silver has taken place, and this must be further continued, by the application of what is called the developing or reducing solution. Two grains of crystallized pyro-gallic acid, dissolved in an ounce of water and ten drops of the silver bath added before using—forms an excellent developer. It is to be poured on, and, by a motion of the plate, quickly spread over the surface, then, almost immediately, the highest lights of the picture will appear as dark marks, and gradually the other parts will show themselves. When they are judged to have sufficiently come out, the plate is to be held under a stream of water and well washed, after which it may be taken to the light and fixed, by pouring over it a solution of cyanide of potassium, of the strength of twelve grains to an ounce of water. This quickly dissolves the unreduced coating of iodide of silver, and leaves only the pure silver forming the lights of the picture. The plate is then to be quickly and thoroughly washed in a stream of water, allowed to dry, and afterwards protected by passing over it a varnish, composed of amber dissolved in chloroform, or a saturated solution of gum damer in naphtha. Another developing solution is composed of eight grains of sulphate of iron dissolved in an ounce of water, and two drops of nitric acid added, and half a drachm of acetic acid, and half a drachm of alcohol. The use of the acetic acid and the alcohol being chiefly to enable the iron solution to flow freely over the surface of the plate, and thus to prevent unequal actions giving rise to stains, a greater quantity can be added according to the necessities of the case.

If it is required that these pictures should be intense enough to print from—that is, if the lights must be sufficiently opaque to protect a piece of sensitive paper placed behind them from the light, so as to give a print of the picture—the exposure in the camera must be considerably prolonged; also, it is generally necessary to thicken the deposits on the plate, which may be done by first making a thoroughly saturated solution of corrosive sublimate in strong muriatic acid, mixing it with seven times its bulk of water, and pouring it over the plate while still moist after the washing from the cyanide solution, allowing the corrosive sublimate to remain on it till a dead whiteness spreads over the lights, then washing under a very gentle and small stream of water made to spread over the surface. Great care is now necessary, as the film is rendered exceedingly tender and liable to be torn. When it is thoroughly washed, a solution of five grains of iodide of potassium to an ounce of water, is to be poured out in sufficient quantity to cover it, or a saturated solution of hydrosulphuret of ammonia, which greatly increases the opacity of the film, and in the high lights renders it perfectly black, so as to answer the purposes of a negative in printing. It is now to be carefully washed, dried, and varnished, before being used for printing. The process of printing is the same as detailed in the article CALOTYPE. The usual causes of failure in the Collodion process are as follow:—Imperfectly cleaned glasses; hurrying the plate out of the silver bath before the greasy-looking marks have disappeared; want of speed and completeness in spreading on the developing solution; allowing the silver solution, which has run down and accumulated on the lower edge of the plate during the exposure in the camera, to run back on the plate before the developing solution has spread over it, thus preventing the equable development. If the collodion be pure and moderately new—the silver bath slightly acid, by the addition of a fraction of a drop of nitric acid, rather than alkaline—this beautiful process is most certain in its results.

Colours. This article is set apart for consideration of one of the most obscure and perplexing subjects in theoretical or speculative Optics, viz., the *colour of natural bodies.* But two remarks require, for the sake of distinctness, to be premised.—(1.) Decomposition of the solar ray by refraction, shows the white beam resolved into seven different simple beams or primary colours, viz.: the *violet*, *indigo*, *blue*, *green*, *yellow*, *orange*, and *red* rays. The reader will find all proposed analyses of the solar beam, discussed under SPECTRUM. Further, *light* and *colour* have for their efficient cause the vibrations more or less frequent—although

always inconceivably rapid—of an extremely subtle and extremely elastic fluid or ether; an ether whose density is evanescent, and its elasticity infinitely great. The different prismatic colours, regarded apart, result from undulations of the ethereal fluid, of different *lengths.* And colours are simple or composite, according as they originate in the superposition of identical or different undulatory movements. Compare LIGHT and UNDULATIONS. — (2.) A distinction must be made among what we term the colours of external objects : the phenomena of permanent colours which form the subject of this article, must not be confounded with colours that are accidental and variable. Of the latter class of colours, there are three chief sources, viz., *refraction* and *dispersion*, as in the case of the rainbow, the spectrum, cut diamond, &c.; *diffraction*, which bestows on the feathers of birds their exquisite shades, on the agate its brilliant reflection, and extracts from finely striated surfaces their magnificent net-work ; and *interference*, to which are owing, in an infinite variety of ordinary circumstances, the production of the colours of *thin plates.* (Compare these articles.) The *permanent colour* of a body, on the other hand, seems to belong to the body itself. It is wholly independent of its shape, and appears as inseparable from its nature, as its density or any other essential physical attribute.—The doctrine set forth by Newton regarding the cause of the colour of natural bodies, is extremely artificial and defective. He considered it a case of what is now called *interference;* ranging it under the category of *thin plates* (*q.v.*): a theory which, under all its phases, as elaborately developed by M. Biot, has received a masterly refutation at the hands of Sir David Brewster. This latter philosopher, however, influenced probably by his early partialities towards the now almost abandoned doctrine of *emission*, (see LIGHT) appeared inclined to refer the phenomena in question to *absorption*, the designation of a department of optics which Sir David's long and fruitful labours have marvellously enriched. The notion that *absorption* is the cause of the permanent colour of bodies is still the common one; and, *in extenso*, it is as follows:—"A ray of light, which we have seen, is rather a *sheaf of rays* falling upon a body, —that body, because of its inherent qualities, takes in, or *absorbs*, a portion of the sheaf, and sends away or reflects the rest of it. But as the reflected portion alone meets the eye, and as that portion must be coloured, the eye attributes to the body the colour appertaining to that reflected portion." Since this proposed explanation assumes an occult cause or power, on the part of each body, to accept a part of the incident sheaf of rays, and to reject or reflect the other part,— it is clear that no supporter of it, is entitled to object to any other explanation, on the mere ground that peculiar and yet not-understood action on the part of the coloured body, is assumed in it likewise.—The subject can be rightly entered on, in one way only. *Is the light which gives us the sensation of the colour of bodies*, REFLECTED LIGHT? And as all surfaces reflect light in so far, *can we separate the light they reflect, from the light which induces us to say that they are of such and such a colour?* Until these and similar questions are definitely answered, the doctrine of absorption can only be held as a re-statement, in difficult words, of the fact, that natural bodies are coloured. The classic memoir on one portion of this inquiry will always be that one of the remarkable Œrsted. Having shown first, that all bodies have, in a certain sense, the property of mirrors, he proves that their mirror-action has nothing whatever to do, in producing the sensation of colour which any body transmits. Whatever the nature of the surface, the *images* produced by it, and which are due to reflection, *have no colour:* or if a shade of colour tinges these images, that is owing entirely to the immediate proximity of the coloured body,—just as, in a room with green hangings, the complexion is affected. The light to which colour is owing, Œrsted considers due to, what he termed a decomposing reflection —something quite different from anything depending on more or less imperfect mirror-action. We are conducted onward an important step, by the researches of Benedict Prevost. This ingenious inquirer appears to establish, by no recondite process, that the phenomena of the colour of bodies is not due to any *reflection* of light, but to a *radiation*, so to speak, on the part of the bodies themselves: in other words, a substance on which light falls, does not accept one part of the sheaf, and reject the rest; it reflects the whole according to its ability, but the incidence of the ray originates a power in the body itself to become the source and centre of peculiar and therefore coloured undulations. Regular reflection could not produce, in regard to colour, any phenomenon not analogous to the *echo* in sound, which is heard only at a determinate point. But besides the echo, there is a *resonnance* of the body itself, which is thrown into vibration by communication of motion—a resonnance heard on all sides of it. The permanent colours of bodies are, according to Prevost, the result of a *luminous resonnance.* Neither Prevost nor Œrsted, however, was able to bestow on these opinions that definiteness in which all satisfactory inquiry should terminate. *Numeri omnia regunt.* This was reserved for Arago. Partly by himself, partly by our own celebrated physicist, the laws of the *polarization of light* (see POLARIZATION) had long been established; and especially that essential difference pointed out, between *light polarized by* REFLECTION and *light polarized by* REFRACTION. Now Arago had for some time recognized, that between light arising from incandescent solids, and from gases, there is a wide separation. Light reaching us from incandescent solids is

..arized by *refraction*, proving that it emanates ..n the interior beds or strata of the solid body; ..ile that reaching us from inflamed gases is .. polarized at all. On this ground, Arago ..ablished physically the correctness of Sir ..lliam Herschell's idea, that the light of the .. originates in a brilliant gaseous photosphere: .. the same use of his polariscope has evolved ..ults not less remarkable concerning the origin ..the colours of natural bodies. If a sheaf of ..ite rays falls on a polished white surface, the ..face thus illuminated, sends to the eye, light ..two entirely different conditions. Looked at ..der a certain angle, the light comes as if from ..*mirror;* seen from any other point, the white ..ht is dull, and as if dispersed. Now what is ..s dull and dispersed white light? Is it part ..the ordinary beam, broken into *shivers* by the ..ltitude of small irregular reflecting points on .. surface of the body, or is its origin different? .. course, the main point to be ascertained was ..s—does that dull white light come from the ..*face*, or from some interior portion of the ..ite body? Now, when these two *lights* are ..alyzed by the polariscope, they are found to .. oppositely polarized,—the former polarized .. *reflection*, the latter by *refraction*, and there- ..e *certainly arising from the interior of the ..oured body.* But what is the origin of this ..racted mass of light? Is it portion of the ..ident ray, which, after penetrating to a cer- ..n distance within the mass of the body, then ..erges; or, *has it been evolved or generated ..thin the body itself?* The latter is the fact; ..d Arago thus demonstrates it. On illuminat- .. a disc of white or opal glass, by a beam ..larized *in one special plane*, he found that the ..dy thus illuminated sent to his polariscope ..ht polarized *in the opposite plane.* The light, ..erefore, which emanates from coloured bodies .. not part of the light that falls upon them, but ..*mething generated within the bodies themselves.* ..n ingenious experiment, lately reported by .. Botzenhart of Vienna, confirms these con- ..usions of Arago. By aid of the dichroscopic ..ns of Haidinger, Botzenhart separated the two ..nages of the body—the one arising from ordi- ..ry reflection; the other being due to colour— ..e white image is always polarized in the plane .. the incidence of the ray; the coloured image .. the plane at right angles to it. Botzenhart, .. though in so far anticipated by Arago, infers ..e following propositions. (1.) The white light ..hich illuminates bodies *is reflected* WHITE. ..) The coloured light that reaches us from ..tural bodies does not come from their surfaces, ..t from their interior—*having undergone an ..tual refraction.* The second of these proposi- ..ons had been previously established by Arago. ..he *critical facts* thus satisfactorily established, .. remains to ascertain whether and how far ..ey can be brought within the range of any .. the accepted theories of light? Protected by a certain reservation, we shall endeavour to express the amount of this apparent coincidence. The true author of the modern *theory* of colours is that great and comprehensive genius—Euler. It may be summarily expressed as follows. Since it is certain that any noise will make the chords of a harpsichord vibrate and emit a clear note; so the same thing may occur with reference to the objects of SIGHT. Coloured objects are like the strings of a harpsichord, and their different colours resemble those sounds, sometimes *grave*, sometimes *acute*. The light to which bodies are exposed is analogous to an external sound affecting a harp; and exactly as this sound acts upon the strings of the harp the light of an illuminated body may be supposed to act on the luminiferous medium *within* another body, and cause it to send forth *its own*, or *peculiar vibration.* As already confessed, an occult or not understood cause is presumed by this explanation, viz.: that the luminiferous ether within different bodies is in various conditions of tension,—conditions that render it most susceptible of a special kind of vibration within each separate body. But the force of the objection holds against every theory of natural colour; and this one has the advantage of quadrating with the exacter inquiries of Œrsted, Prevost, and Arago. Still further: while *light* falls on a body and puts its ethereal molecules, or rather the ethereal medium within it, into vibration, the incident ray likewise is subjected to a sort of reaction on the part of the body which manifests itself under singular modifications. The primitive ray even when *white*, is not only, as if metamorphosed into a coloured ray; but this coloured ray becomes *discontinuous*—streaked by *dark bands*, at the points of its maximum and minimum intensities. The facts of this singular change were brought to light chiefly by Sir David Brewster, and will be noticed at length under FRAUENHOFER'S LINES. But it is to M. Ermann and Baron von Wrede that we owe the explanation of these, and their easy deduction from the foregoing theory of colours. The student who would go to the root of this subject is especially referred to a paper by the latter eminent inquirer, reproduced in *Taylor's Scientific Memoirs.* He will find there how all those dark bands, every shade of colour, and the most complete transparency or opacity, may be supposed to spring from one source—a special description of *interference*, or of retardation undergone by certain series of the undulations, propagated by bodies, when they originate the appearances of which we have now been treating.

Colours, ACCIDENTAL OR SUBJECTIVE. An extensive class of relations in which the observer stands to the phenomena of colour, because of peculiarities in, or re-action of, the visual organ. On several of these, remarks will be found under DALTONISM, EYE, IRRADIATION. At present we shall notice the phenomena of acci-

dental colours properly so called, or those purely subjective sensations of colour, which the contemplation of actually coloured bodies often brings along with it. There are two chief classes of these. (1.) *Sensations* FOLLOWING *on the steady contemplation of coloured objects.* Place a coloured object on a black ground, and look at it attentively. Its colour soon begins to fade gradually. Then *turn* the eye suddenly to a sheet of white paper: immediately *an* image of the object will appear, of *the colour complementary to that of the original object.* In other words the contemplation of a *red* object will give rise to a *green* image; a *white* object has a *black* image; a *yellow* or *blue* one, a *violet* or *orange* image; and *vice versa.* All such images remain visible for some time; both their intensity and duration depending on the length of time during which the eye was fixed on the object itself. The exact laws of the phenomenon are as follow:—As soon as the retina ceases to be excited directly by the coloured object, there occurs—*first*, the persistence for a very short time, of the primitive impression; *secondly*, the apparition of the accidental image; and *thirdly*, successive appearances and disappearances, more or less numerous, of the accidental image; and, in certain cases, alternating apparitions of the primitive impression and of the accidental image. (2.) The second class of accidental or subjective colours may be termed the *simultaneous* class. The appearances just referred to consist of images *succeeding* the contemplation of coloured objects; but experience proves that, even *during* that contemplation, there is another curious order of phenomena, also manifesting complementary colours. It was remarked by Buffon, for instance, that if one looks long on a coloured object placed on a white ground, colours rapidly develop themselves around the edges of the object, of the same shade as the accidental image already spoken of. Rumford showed, too, that if a shadow is produced within coloured light, the colour of the shadow is complementary to that of the light; that is to say, if a sheet of paper be illuminated by a *green* light, a body illuminated by white light, and interposed between the paper and the source of the green light, will cast on the paper a *red* shadow. Other curious similar phenomena have been noticed by various observers. The general law of these simultaneous subjective colours is as follows:—When we look, directly or indirectly, at a coloured space, there is developed around the edges of that space a considerable breadth of the complementary colour, of greater or less intensity; which intensity, however, gradually diminishes as its distance from the object diminishes. (Black and white, in all these cases, rank as complementary colours.) And if two coloured spaces or objects are near each other, they seem to have a *reciprocal action;* regard being had to the size and brightness of each. Minute details may thus be summed up: At a distance from the edge of the coloured object we find—*first*, a slight prolongation of the actual impression; *secondly*, beyond this prolongation, extending to a considerable distance, the development of the accidental colour; and *thirdly*, in certain circumstances, beyond the space occupied by the accidental colour, a new development of the actual colour of the object.—The explanation generally adopted, regarding the first class of phenomena, is that of M. Scherffer. It presumes that the continued action of rays of a certain colour, on the retina, enfeebles its sensibility to these rays; so that, on the eye being directed to a white surface, the part of the retina affected, receives an impression from the other rays only, or from the complementary part of the white beam. M. Plateau, in an exceedingly interesting memoir, has attempted to refer both classes to the same physiological principle. According to him, the *persistence* of the primitive impression, as well as the *accidental colours* of the *first class*, constitute the transition of a portion of the retina from an excited to its normal state, the transition being considered in reference to the *time* required to accomplish it; while *irradiation*, and accidental colours of the *second class*, constitute or mark the same transition, as taking place in *space*—the transition between the actually excited portion of the retina and that which is in repose.

COLOURS, *Complementary.* — If two colours make up white when they are mixed, the one is said to be *complementary* to the other. Black and white, light and darkness, are also held as mutually complementary.

Columba Noachi. A southern constellation,—one of those named in modern times. It has no stars above the third magnitude.

Colures. Two great circles of the Sphere, or rather, two pairs of semicircles. If a great circle be supposed to pass through the pole of the Earth and the pole of the Ecliptic, it will pass also through the points at which the Sun has the greatest North and South Declination: the halves of this great circle—the one passing through the tropic of Cancer, the other through the tropic of Capricorn—are the *Solstitial Colures.* The other great circle passes through the equinoctial points; and its *halves* are the equinoctial colures.

Coma Berenices (*Hair of Berenice*), a constellation in the northern hemisphere. It is not one of the more ancient, possessing no large stars. Tycho first gave it a distinct place in the sky.

Combinations. See PERMUTATIONS and PROBABILITIES.

Combustion. See HEAT.

Comets. An extraordinary class of cosmical meteors, some of which are at present connected with our planetary system; the greater number being known to us only because they traverse that portion of space within which our system lies. A Comet, as it usually appears, consists

a nucleus more or less bright, environed by nebulous atmosphere or *chevelure*, and at-nded by a still fainter nebulosity or *tail*, etching out linearly, often through an im-ense distance. The nucleus or the *head* of the met, notwithstanding its aspect of comparative idity, is itself altogether nebulous; for the escope dissipates every trace of a solid mass, id clusters of small stars, which even a cloud ould have obscured, have been seen through heart of it. The shapes of these singular teors do not, however, always conform to the egoing type. At times they appear with eral tails; in some cases there is no tail pro-ly so called, only the nucleus is not quite nmetrically placed within the chevelure; and other instances they present a mere nebulosity, h no trace of a nucleus. It is unnecessary to nark how strongly these bodies are contrasted th the organized planets of our system. Nei-r is it astonishing that their unwonted aspects l unexpected apparition, caused them to upy, for many centuries, the place of *portents.* ience has now dissipated all such delusions; l, although much concerning the physical stitution of Comets continues unknown, we eady have recognized their subjection to cer-n great and definite Laws of the Universe; l can welcome them, besides, as instruments ely to enable us to attack successfully certain mentous cosmical problems that could not be ched without them. We shall arrange what have to say in detail regarding Comets under ee distinct heads:—

(1.) *Comets, Motions, Periods, and Numbers of.* It is now thoroughly established that, erratic ugh these singular bodies appear, they are, all their motions—in the neighbourhood of our lar System, at least—governed by that same of gravitation which controls the planets they revolve around the Sun. Newton's nal discovery—that a body obeying this law ght move either in an ellipse of whatever entricity, or in any other conic section—doubt-s awakened in the mind of his devoted fol-ver, Dr. Halley, that *à priori* suspicion, which ulted in the famous prediction, that the Comet, htly bearing his name, would be found to re-ve around the Sun, in an ellipse, in a period 75 or 76 years. Apart from his conviction of *reality* of that principle of Order, which his strious friend and teacher had detected amid great, *actual*, and the still greater, *possible*, iety of planetary motions, Halley would rcely have ventured to infer—even from the king correspondence of earlier records with phenomena he saw—that the capricious eor under his own inspection, and those which l arrested former observers, were one and the ie: certainly he would not have hazarded prophecy that, after the lapse of another 75 76 years, this Comet would return. The r in which this body appeared to Halley and his contemporaries was 1682. Flamsteed con-joined to his other eminent services, that of care-fully determining the Comet's places; and it was the remarkable correspondence of *parabolic* elements, as afforded by these places, with the similar elements of the meteors that distinguished the years 1531 and 1607, which led Halley to the conclusion that "the Comets of those three years—viz., 1531, 1607, and 1682—are one and the same Comet, that had made three revolutions in its elliptical orbit." Taking certain disturbing effects, on the part of the planet Jupiter, into ac-count, he concluded that the Comet's next return to its perihelion might be delayed until the be-ginning of 1759; and, in advising the astro-nomers of that day carefully to look for such re-appearance, he expressed a hope, which cer-tainly has not been disappointed, that, in event of its return, posterity would not refuse to ac-knowledge that its periodicity had first been discovered by an Englishman. As the year 1759 approached, astronomers became more and more alive to the consummate importance of Halley's prediction; and those very laborious computations, rendered necessary by the compa-ratively advanced state of physical astronomy, were undertaken and brought to a brilliant con-clusion by the celebrated Clairaut, assisted by Lalande and Madame Lepaute. The nature of the indispensable computations will be best understood, with the orbit of the Comet before us. This is represented in the annexed diagram. The contrast between this cometic orbit and the paths of the planets, need scarcely be pointed out. The paths of the latter are far apart from each other; and, notwithstanding their eccentricities, they never intersect; nor do the bo-dies moving in them, come at any time very near each other: hence, the reciprocal influences of these orbs are comparatively small—im-pressing only slight changes on the shapes of their or-bits, or their periods. But the path of Halley's Comet is such that, in the course of a revolution, it may, through great proximity to some planet or planets, be seriously affected as to its course and period; so that an exact estimate of the planetary perturbations, is essential to any just deter-

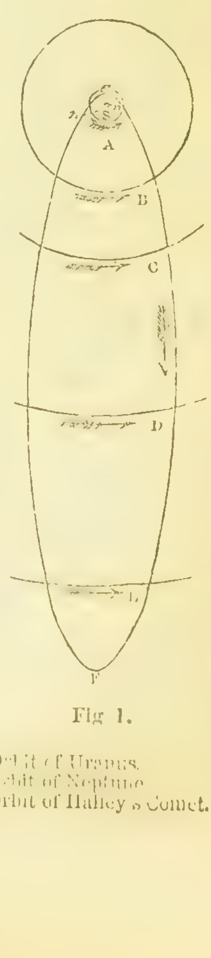


Fig 1.

A. Orbit of Earth. D. Orbit of Uranus.
B. Orbit of Jupiter. E. Orbit of Neptune.
C. Orbit of Saturn. F. Orbit of Halley's Comet.

mination of the date of its return. In Clairaut's time, unfortunately, these perturbations could not be minutely estimated, since the masses of Jupiter and Saturn were not exactly known, nor had Uranus or Neptune been discovered. Clairaut did not pretend to an ultimate exactitude; for, when hazarding his first statement — that the Comet should be found at its perihelion on 13th April, 1759—he confessed that he might be in error a full month. *The Comet came to its perihelion on 12th March.* This approximation was wonderful enough; more than adequate to establish the reality of the sway of gravitation over these wandering meteors. But when another period of 75 years had completed its course, Science had to reach still greater triumphs. Halley's Comet reappeared at the close of 1835. The masses of Saturn and Jupiter were now determined, and the influence of Uranus could be taken into account. The investigation was of course renewed. It was best performed by Rosenberger of Halle; and the occurrence agreed with the prediction, within *five days*—surely a trifling discrepancy considering the long period of that mist's revolution! Since then, Neptune has been revealed; and when this comet again returns, it is not likely that the error of calculation may exceed as many hours! It is indeed a matter of no slight gratification to the Human Intellect, that amid all external obstructions, and its proper imperfections, it can yet ascend so closely to the seat of Nature's most intricate laws! The actual dimensions of the orbit of Halley's Comet, as determined by the late Hermann Westphalen, are as follow:—

Least distance from the Sun,	55,900,000 miles.
Greatest distance, ...	3,370,300,000 —
Major axis of Orbit, ...	3,426,200,000 —
Minor axis of Orbit, ...	826,900,000 —

As shown in the diagram, this comet recedes from

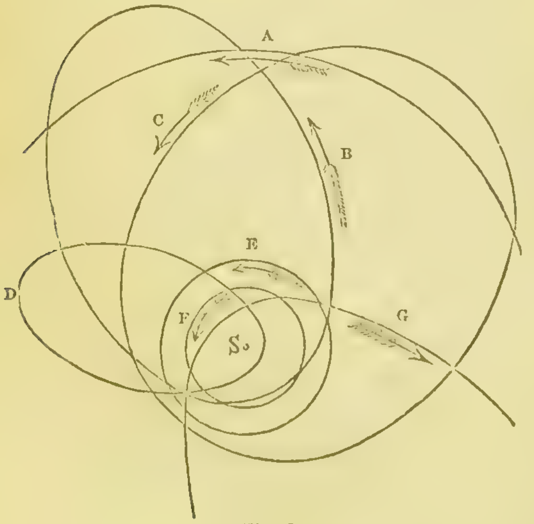


Fig. 2.

A. Orbit of Jupiter. E. Orbit of Mars.
C. Comet of $6\frac{3}{4}$ years. F. Orbit of the Earth.
B. Comet of $7\frac{1}{2}$ years. G. Halley's Comet.
D. Comet of short period.

the sun to a distance considerably exceeding that of Neptune. At the remote parts of its orbit it is always far removed from the plane of the Ecliptic on the South Side; and, alone of all known *periodical* comets, the direction of its motion is in the contrary direction to that of the planetary system, or against the order of the *Signs.* We have purposely dwelt at some length on the History of the Comet of Halley: while, standing as a type of the habitudes of all Comets, it holds a position intermediate between a number of Comets of shorter period, and those whose periods are so long, that rational doubt may be expressed whether they are really connected with our system or not. The subjoined diagram represents the position of the orbits of other three—viz., the Comet of 3·3 years, or the Comet of Encke; Biela's Comet, or that of $6\frac{3}{4}$ years; and Faye's Comet, whose period is $7\frac{1}{2}$ years. Besides these we have the Comet of De Vico, with a period of $5\frac{1}{2}$ years; the Comet of Brorsen, with a period slightly larger; the Comet of D'Arrest, whose period is scarcely $6\frac{1}{2}$ years; as well as six others, the probable periods of four of which are within six years,—those of the other two, being *twelve or fifteen.* Four Comets are known, with probable periods corresponding to that of Halley's; and the following results of computation may perhaps be accepted as wide approximations, viz.:—

Years of the Comet's appearance.	Probable period.
1763, ...	7,334 years.
1769, ...	2,090 —
1807, ...	1,714 —
1811 (the great comet of),	3,065 —
1811 (second comet of), ...	876 —
1822 (fourth comet of), ...	5,444 —
1825 (great comet of), ...	4,386 —
1840 (fourth comet of), ...	344 —
1844, ...	102,050 —
1846 (De Vico's), ...	2,720 —

There is one other Comet, apparently periodical, of too important a character to permit us to omit notice of it. One of the greatest meteors of this sort, mentioned in history, appeared in the middle of 1262; and has been spoken of by all historians in terms of wonder and astonishment. In 1556 a prodigy of the same character, and with similar elements—in as far as wide observations enable us to judge—spread terror alike over Europe and Asia. Considerable probability exists that it had appeared several times previously, after similar intervals. Some modern inquirers, especially Mr. Hind, have occupied themselves earnestly with the question of its orbit; and the latter Astronomer, after great research and pains, has ventured to say that between 1856 and 1860 we may look for its return—its mean period being about 300 years. If this prediction be verified, it will be only second in interest to what befel in the case of Dr. Halley.—Of course it must not be imagined that the larger periods mentioned above, are determined with absolute accuracy; nay, it is even unlikely that the greater number of these bodies

at visit our system, have re-entering orbits at all. ır more probably, their paths are hyperbolic; ıd the meteors themselves merely bent around .r Sun, whose attraction they have encountered, they sweep through the cosmical spaces. ıose of them that really move in Ellipses, and erefore in one respect may be said to belong to .r system, are not improbably also cosmical eteors, whose paths have been converted into llipses by the influence of the Sun and the .anets. Lexell's Comet, for instance, from ing a very erratic meteor, was thus obliged for brief time to revolve through a short ellipsis; ıt the perturbing action of Jupiter again threw off towards remote abysses. It is rendered still rther probable that the Comets do not properly long to our planetary scheme, by the fact that ey are not subject to any of the constituent ws of that scheme. For instance, they are not nfined to the plane, in the neighbourhood of hich the planetary orbits are all found; *i.e.*, eir places in the sky have no connection with .e *Zodiac;* their motions are as frequently *trograde* as *direct;* and, as we have seen, the ongation of their orbits causes them to intersect e paths of all the regular attendants on our ın.—We can form no conception of their numrs. Probability alone is the ground of any dgment on the question; and certainly we canɔt place the inferior limit, lower than many illions. The likelihood is that they are countss as the stars themselves; and must be regarded scattered through the interstellar spaces, where ey perform functions that are at present utterly ıknown.

(2.) *Comets, certain Physical Features of.*—We e not yet in a condition to define the physical ructure of comets. They furnish, and probably ill long furnish, in this point of view, the greatt stimulus to curiosity. But a few remarks ay be safely adventured on.—*First*, the relation the Comets to *Light* cannot be considered ttled by the observations of Arago. Testing ıe light sent by them by means of his *polarcope*, he discovered that it is polarized by *reɛction;* and he hence concluded that they are not lf-luminous, but that—like the planets in this spect—they shine by reflecting solar light. rago did not show conclusively that *all* the ght issuing from Comets is polarized by reflecɔn; and it can scarcely be denied that what essel saw, regarding Halley's Comet in 1835, tablishes a strong probability that these bodies ıve a certain faculty of evolving a *proper* light their own. Further observation is needful for .e settlement of this delicate question.—*Secondly:* -As to the molecular constitution, or inteɔr consistency of Comets, it cannot be preımed to be distinguished by much *cohesion.* either can the particles of such a body be supɔsed to be held together by effect of their mutual avitation; for that would not really amount to uch more than mere juxtaposition in space. We must look, therefore, at each individual cometic molecule, in the light of an independent projectile, describing its own orbit around the Sun. This extreme looseness of texture was strikingly manifested by the recent separation of Biela's Comet into two distinct bodies or nuclei, revolving around each other; and it is perhaps the cause of another peculiarity of these bodies—viz., their remarkable contraction in size when they reach *perihelion.* If a number of bodies describe parabolas or very eccentric ellipses, having the same axes, and around the same central orb in their common focus, the intervals separating them will be *minima* at the perihelion; in other words, the parabolas will nearly touch or osculate each other at that region; and the molecules will again mutually recede as the group departs from the Sun. *Compression*, therefore, at perihelion, and *expansion* afterwards, are among the mechanical necessities of such a system. It scarcely requires to be stated that the *density* of Comets is quite trifling. Although they have passed quite near to the smaller bodies of our planetary system, they have never impressed any palpable or measurable *perturbation.* But, *Thirdly*—Notwithstanding the amount of light thus thrown on such molecular expansions and contractions, the whole subject of the tails of these bodies is enveloped in mystery. These extraordinary appendages are perhaps often hollow, and symptoms of rotatory motion around their longer axes have been detected; still, why is it that they are always directed right away from the Sun; and what conceivable force can retain their more distant portions in *any* orbit, when at perihelion, they are brandished around our Luminary with a velocity so unparalleled? It is useless at present to waste time in conjecture on a subject, demanding, beyond everything, the attention of the precise observer. One remarkable contribution to positive knowledge must, however, be mentioned—viz., Bessel's memoir on the phenomena, attending what seemed the process of *the formation* of the tail of Halley's Comet in 1835. This distinguished person had rare opportunities to notice these phenomena; and they led him to the conviction that our central orb develops a *Polarity*, or exercises a *Polar Force* over these diffuse meteors. What that polar force is, remains of course unknown: but, considering it akin to *Magnetism*, it will readily be seen that it renders comprehensible the disturbance of the Comet's form, its position towards the Sun, and the fact that the motions of the body are not disturbed by the development of the tail. The whole of the memoir in question is worthy of more attention than it has hitherto received.

(3.) *Comets, as Instruments of Discovery.*—These bodies, we have said, are yet likely to lead in, brilliant courses, of otherwise inaccessible discovery. For instance, if the speculation, just alluded to, shall turn out well founded, they reveal a new character of the Sun, or at all events greatly

extend our knowledge of that portion of his efficiency to which we owe all the magnetic phenomena of the Earth. In the next place, wandering, as Comets do, through all portions of the interplanetary spaces, they must doubtless unfold, in future times, the contents of these spaces;—a train of discovery that may be deemed already inaugurated by the more than presumption, obtained from the retardation of Encke's Comet, in favour of the opinion that a *resisting medium* exists. And lastly, they stand to us as specimens—the most palpable we can obtain—of those other curious formations, such as the Zodiacal Light, which belong to our planetary scheme as indefeasibly as the Planets themselves. What is already established, has, of course, dispelled all delusions as to the effects of the shocks of Comets in producing geological catastrophes in the Earth. Their feeble or rather inappreciable density, deprives them of the power to produce any dynamical effects; although it is possible that *collision*—as it has been termed—by introducing extraneous substances into our terrestrial atmosphere, might influence the fates of the organic races, to whose development and welfare that Atmosphere is an essential ministrant.

Commensurable. Two quantities of the same kind, or two numbers, are said to be commensurable when a third quantity or number is contained in each a certain number of times. In reasoning regarding *incommensurables*, we require to show that, what we expect regarding them, holds good, almost perfectly, with regard to two commensurables on each side of them; and approximates more and more nearly to the complete expression of the truth, the nearer these close in on the incommensurable in question.

Compass. Consists fundamentally of a magnetized needle turning freely. As this turns towards a position, constant within certain limits, wherever it may be, it is evidently possible, by means of it to ascertain definitely the directions and changes of direction of any motion, referring their lines to this constant direction. It is supposed commonly to have been invented about 1302, by Flavio Gioja. It was certainly known in some parts of Europe before 1180. (*Encycl. Metrop.*) Its use to sailors, who must lose all the ordinary marks of direction, is abundantly plain.

Points of the Compass are the thirty-two principal points of division on the rim of the circular card below the horizontal plane in which the needle is permitted to move freely. They are called otherwise *Rhumbs*. They are marked at the extremities of equal arcs of the circumference, and refer, according as they are nearer one or other, to the two chief lines North (towards the pole star, or North point of horizon,) and South, and East and West, (to the right and left as we look towards the North). They are as follows:—

North.	East.	South.	West.
N. by E.	E. by S.	S. by W.	W. by N.
N.N.E.	E.S.E.	S S.W.	W.N.W.
N.E. by N.	S.E. by E.	S.W. by S.	N.W. by W.
N.E.	S.E	S.W.	N.W.
N.E. by E.	S.E. by S.	S.W. by W.	N.W. by N.
E.N E.	S.S.E.	W.S.W.	N.N.W.
E. by N.	S. by E.	W. by S.	N. by W.

The student may readily construct the circle and mark the points in order. Each division contains $\frac{360}{32}^\circ$ of the circumference, or represents 11° 15 of angular space. Each is further subdivided into quarters

Compass, Mariner's.—The common construction is of extreme simplicity. There is merely a circular box containing a paper card, on which the points of the compass are marked, above the middle of which a magnetic needle is set, free to rotate round its centre. It turns always *nearly* to the north, (see MAGNETISM and VARIATION,) and the amount of deviation is pretty accurately estimable.

As in all other instruments of extreme importance, this idea has been so elaborated that the chances and limits of error, from defective suspension and the like, are excessively slight. The most important irregularity that is yet beyond the complete control of practical men, arises from the effect of the iron and magnetizable matter present in ships. Many vessels are now, of course, entirely constructed of iron. In the various positions of the ship, they become magnets, acting conjointly with the earth on the needle, and exerting greater or less deviating effect, as the direction of their action coincides more or less closely with the constant direction of terrestrial magnestism. See COMPOSITION OF FORCES.

Azimuth Compass has the circumference of the card more accurately subdivided into exact degrees, minutes, and seconds, and to the box is fitted an index with two sights—that is, upright pieces of brass set diametrically opposite one another, with slits down the middle, through which the sun or a star may be viewed. The position into which the index of sights must be turned to see it, will evidently indicate on the card the azimuth of the star. As in all similar instruments, telescopes accurately adjusted are used instead of sights when the observation is to be exact. The use of the instrument is chiefly to note the actual magnetic azimuth, from which—as we know the azimuth calculated from the north and south line, we find the *variation* of the needle, with the amount of its changes of variation.

Compass, Deviation of, is the angle by which the compass is deflected from the magnetic north by the action of the iron in a ship.—The large quantity of iron now used in the construction and equipment of steamers, iron sailing vessels, and sometimes of wooden sailing vessels, produces a deviation of the compass from the magnetic north, which interferes seriously with

navigation of such vessels. We shall en- ivour to explain the causes, the laws, and the thods proposed for correcting the deviation produced. All iron, as regards magnetism, s between the two extreme states, denominated soft iron" and "hard iron." Soft iron is iron iich becomes instantly magnetic by induction ien exposed to the influence of any magnetized dy, and instantly loses its magnetism when influence is removed. Its magnetism is med "induced" magnetism. Hard iron is n which does not, in ordinary circumstances, come magnetic by induction, and which, when ce magnetized, retains its magnetism when the luencing body is removed. Its magnetism is med "permanent" magnetism.—Neither ex- me exists in nature, but much of the iron in vessels approaches each extreme. Much is in intermediate state, the magnetism of which s been called "subpermanent," or "retentive." ch iron, when once magnetized, retains its gnetism with little loss for a long time, unless posed to blows or strains, changing its mag- ism slowly and gradually when exposed to a v inducing cause, as for instance, when the vessel anges her magnetic latitude.—In investigating action of iron on the compass, it is conveni- to suppose, in the first instance, that all the n is "hard" or "soft," and then make allow- ce for the difference of the supposition from truth. We shall first consider the effect of permanent magnetism of hard iron on the npass, and we shall suppose the ship to re- in upright—the effects of heeling not having n sufficiently investigated to be capable of ng treated of satisfactorily here.—A piece of d iron magnetized, in other words, a magnet, ced in any position in a ship, attracts the north l of the compass, in a direction in the ship ich does not alter as the ship swings round. y number of such magnets, in other words, all hard iron of the ship acts similarly, and gives to a single resultant, attracting the north l of the compass in a direction in the ship, ich does not change as the ship is swung nd. When this resultant is in the direction of magnetic needle, it produces no deviation. As ship swings, the needle follows the resultant, deviation, or rather the sine of its deviation, reasing as the sine of the horizontal angle tween the needle and the resultant.—In what lows, we shall suppose the deviation to be so all that the sine of the deviation varies as the viation, and that the deviation produced by the disturbing forces, is the sum of the devia- ns they would produce separately. This sim- fies the problem, and is sufficiently correct in se cases in which alone great nicety is pos- le.—The force which opposes the deviation or directive force of the needle, being propor- nal to the horizontal magnetic force of the th, the deviation produced by the hard iron is portional to

$$\frac{\text{Hor. force of hard iron}}{\text{Hor. force of earth}} \times \text{sin angle between}$$

resultant and needle.—Calling the horizontal force of the hard iron R, and resolving it into two,—viz., P directed towards the ship's head, Q to the starboard side; and calling the horizontal force of the earth H, and the angle which the ship's head is to the east of the deviated needle ζ', it will easily be seen that the deviation produced by the hard iron is proportional to

$$\frac{P}{H}\sin\zeta' + \frac{Q}{H}\cos\zeta'$$

The effect of the soft iron is less simple.—In estimating it we make the supposition, which is not far from the truth, that induced magnetism is proportional to the inducing force, and that therefore the effect of any number of inducing causes is the sum of the separate effects. The ordinary cause in this case is the earth's magnetism acting in the line of the dip; which inducing force we may resolve into its two components, the vertical and horizontal forces, and consider their effects separately.—The vertical force induces in the soft iron a magnetic state which does not change as the ship swings, and therefore produces a deviation following the same law as that caused by the hard iron. The deviation so produced, is therefore directly proportional to the vertical force, and being as before inversely proportional to the horizontal force, it varies as the ratio of the vertical to the horizontal force, *i. e.*, it is proportional to the tangent of the dip. Calling the dip θ, and treating this force as we treated the permanent magnetism, we may represent the deviation caused by the inducing force of the vertical part of the earth's magnetism by the formula

$$B'\tan\theta\sin\zeta' + C'\tan\theta\cos\zeta'.$$

Combining the two, we get for the deviation caused by the permanent magnetism, and the vertical part of the induced magnetism

$$\left(B'\tan\theta + \frac{P}{H}\right)\sin\zeta' + \left(C'\tan\theta + \frac{Q}{H}\right)\cos\zeta'.$$

The effect of the horizontal force is most clearly seen by considering its effects on masses of iron of a simple shape,—viz., thin iron rods placed horizontally. When such a rod is at right angles to the magnetic meridian, it is not magnetic. In any other position, its south end attracts the north end of the needle; its north end repels it. Such a rod placed in the line of the keel, and either before or abaft the compass, will produce no deviation when the ship's head is either N. or S., because though it attracts the needle, it does so in the direction in which the needle is point-

ing; so it will produce no deviation when the ship's head is E. or W., because the rod is then not magnetic. When the ship's head is N.E. or S.W., it produces a maximum easterly deviation; when N.W. or S.E., a maximum westerly. It therefore causes a deviation proportional to the sine of 2 ζ'. Here the force producing the deviation, and the force overcome in producing, are both the horizontal force of the earth's magnetism. The deviation produced, is therefore independent of this force, and may be represented by D · sin 2 ζ', where D depends solely on the distribution of the soft iron in the ship. A similar bar placed athwartship, either to the starboard or port side of the compass, will produce a deviation $-$ D sin 2 ζ'; so a horizontal thin rod, whose direction passes through the compass, and makes an angle of 45° with the line of the keel, will cause a deviation E cos 2 ζ'; and two similar bars similarly placed, whose direction passes through the centre of the compass, but which are at right angles to each other, will counteract each other's effects.—There is a remarkable arrangement of such bars, which remains to be mentioned,—viz., two horizontal rods, at right angles to each other, in the same plane with the compass, tangents at one end to a circle concentric with the compass. If these rods both lie to the right from the respective points of contact (looking from the centre of the compass), they produce a constant easterly deviation; if both to the left, a constant westerly deviation. This deviation may be represented by a constant term A, which will be positive in the first case, negative in the second.—These arrangements combined will therefore produce a deviation

$$\mathrm{A} + \mathrm{D} \sin 2\zeta' + \mathrm{E} \cos 2\zeta'.$$

It may be shown that, whatever be the arrangement of the soft iron, the deviation produced will follow the same law, and be expressed by the same formula.—The whole deviation produced by the hard and soft iron may therefore be expressed by

$$\delta = \mathrm{A} + \left(\mathrm{B}' \tan\theta + \frac{\mathrm{P}}{\mathrm{H}} \right) \sin \zeta' + \left(\mathrm{C}' \tan\theta + \frac{\mathrm{Q}}{\mathrm{H}} \right) \cos \zeta' + \mathrm{D} \,.\, \sin 2\zeta' + \mathrm{E} \cos 2\zeta'$$

and this formula (which may be written) $\delta = \mathrm{A} + \mathrm{B} \sin \zeta' + \mathrm{C} \cos \zeta' + \mathrm{D} \sin 2\zeta' + \mathrm{E} \cos 2\zeta'$ will be found in all cases to represent the deviation with sufficient accuracy.—We will now return upon our steps by considering the causes and effects of the terms separately. A expresses a constant deviation whatever be the direction of the ship's head. It cannot arise from permanent magnetism. It may arise from the induced magnetism of soft iron arranged in the way described above, and will in fact exist whenever any elongated horizontal mass of iron is placed near the level of the compass, with its ends at unequal distances from the compass. Thus it has been observed to have a considerable value in gunboats, in which a gun carriage and the fly-wheel of the pump are placed in such position relative to the compass; but it may also have an apparent, not a real value, caused by an index error in the compass on board, from an error of the shore compass from comparison with which the deviation is deduced, or from any mistake as to the amount of variation when the deviation is deduced from the observations of heavenly bodies at sea. In ordinary cases the value of A is so small, that, for the purposes of navigation, it may be safely neglected. D and E cannot arise from permanent magnetism, but only from the magnetism of soft iron. Positive values of D from such masses before or abaft the compass—negative values from such masses on the starboard or port side—values of E from masses of soft iron in the intermediate angles. As might be expected, the value of E is generally very small, and it may always be safely neglected. D varies from 1° or 2° in wooden steamers to 5° or 6° in iron steamers. By theory it ought not, and in practice it is found not to change its value in a change of latitude. This part of the deviation, has been called by Mr. Airy the quadrantal deviation, and it is convenient so to distinguish it. It is almost always positive, showing that it arises from masses of iron before and abaft the compass, and indicating that it may be corrected by a bar of soft iron, or an elongated box of iron chain, placed athwartship on the starboard or port side of the needle.—The remaining terms are by far the most important, and the most difficult to deal with. This part of the deviation has been termed the polar-magnet deviation by Mr. Airy, but is more conveniently termed the "semicircular" deviation, as it consists of two parts, one caused by the permanent magnetism of the hard iron, which is more strictly polar magnetism, the other by the induced magnetism of the soft iron. These two parts change, but in different proportions, with a change of magnetic latitude. The first varies inversely as the horizontal force. The second varies as the tangent of the dip.—Their changes in different magnetic latitudes may be thus described:—At a magnetic pole of the earth, when the dip is 90°, and the horizontal force zero, each part becomes infinite. This indicates that there is then no directive force. For some distance from the magnetic pole the two parts change nearly at the same rate, and therefore the whole varies nearly as the tangent of the dip. But as we approach the magnetic equator, the part which arises from the soft iron diminishes the most rapidly. It becomes zero at the equator; and in south magnetic latitudes has the same value as in corresponding

north magnetic latitudes, but the opposite sign. The part which arises from the hard iron does not become zero at the magnetic equator, but becomes a minimum at that line, nearly coincident with the magnetic equator, at which the horizontal force is a maximum, and in south magnetic latitudes it has the same sign, and nearly the same value, as in northern.—If our hypothesis, that all the iron is perfectly hard or perfectly soft, were strictly true, it would be possible by observations made in any two magnetic latitudes to determine the values of the two parts separately. But in fact this is impossible. The subpermanent or retentive magnetism causes the changes in the magnetism of a ship to depend not only on the place at which the ship is, but on the places in which she has been for some preceding days or weeks; her magnetism being thus in arrear of its theoretical amounts, to an extent which there appear to be no means of estimating.—It appears to have been established by Dr. Scoresby, that a change takes place in a newly launched iron vessel, even without a change of magnetic latitude; but it seems probable, that, after a short time, and a few voyages, no further change of any importance takes place without a change of magnetic latitude.—We shall now say a few words as to the phenomena observed in different parts of the same ship, and the methods of correction which have been proposed. In general, in north magnetic latitudes the north point of the compass is attracted towards, and follows, the ship's head. This arises from the larger mass of iron before and below the compass. As the upper end of every vertical mass of iron attracts, and the lower end repels the north end of the needle, the funnel, the lower end of which is generally before the standard compass, generally diminishes the attraction, and acts as a corrector. An iron stern post is still more effective, and frequently more than counteracts the effect of all the iron in front of it, causing the north point of the compass to follow the stern of the ship. In such a case we may generally find a place in the ship, before the stern-post, where the semicircular deviation is zero. But placing the compass in such a position is of less advantage than might be supposed, as a change would probably take place on a change of latitude.—Whatever be the amount of the semicircular deviation, it may be easily corrected at any given time and place, either by a bar of soft iron, as proposed long ago by Capt. Flinders, or by a magnet, or two magnets, as proposed more recently by the present Astronomer Royal. The methods of making these corrections are too well known to require description here. These corrections may be useful when the vessel does not change her magnetic latitude materially, as in the case of vessels plying between ports in the United Kingdom, or even in Europe, or plying between England and the United States, but are worse than useless when the vessel changes her magnetic latitude considerably, more especially when from a northern she goes to a southern latitude. In fact, whenever the semicircular deviation is reduced to zero by the addition of either soft iron or a magnet, it can only be by the induced magnetism of the soft iron (including the soft iron corrector if applied), compensating the permanent magnetism of the hard iron (including the correcting magnets if applied).—The consequence is, that, in a ship, whose compass has been so corrected, going to an equal south magnetic latitude, the induced magnetism of the soft iron changing its sign, doubles instead of compensating the magnetism of the hard iron. The danger of all such corrections is so well understood in the Royal Navy, that no such correctors are ever made use of in her Majesty's ships, but they still continue to be made use of in the mercantile navy, not only in vessels plying between different ports nearly in the same magnetic latitude, but even in vessels going to southern latitudes. As an instance of the inefficacy and danger of such corrections, it may be mentioned that the Fiery Cross iron steamer had her binnacle compasses corrected by magnets in England, and in latitude 29·53 S., longitude 60·39 E., had a deviation of 85°.—How then are the deviations to be corrected? By fixing a compass, to be called the standard compass, in a place in the ship as far removed as possible from the disturbing influence of the ship's iron, directing the ship's course by this, and using the binnacle compass merely as a guide to the man at the helm. It has been proposed to fix such a compass at the mast-head, fitting the mast-head with brass work instead of iron work; and it is said that a compass may be so fixed, and then observed without inconvenience; and that when so fixed, the deviations are insensible. This method was successfully adopted in the "Royal Charter," as described by Dr. Scoresby. If not adopted, or not successful, the method adopted in the Royal Navy is to be preferred. An azimuth compass is fixed at a convenient place so high above the deck that bearings can be taken over the ship's bulwarks. Then the ship's head should be placed on a number of compass points, as nearly equidistant as possible, and the deviations on each point observed either by comparison with a compass, or by the bearing of a distant terrestrial object, or of a heavenly body. The methods of doing this are well known, and are described in a pamphlet published by the Admiralty, under the title, *Practical Rules for ascertaining or applying the Deviation of the Compass caused by the Iron in a Ship*. From these observations a table of compass-courses and corresponding correct magnetic-courses may be formed, by which (making a simple interpolation if necessary), from any compass-course, the corresponding correct magnetic-course may be found, or, conversely, from a correct magnetic-course, the corresponding compass-course.—This is suf-

ficient for ordinary purposes. When greater accuracy is required, then we have the two problems to solve,—viz., *first*, from deviations observed on a few points to compute the deviation on the intermediate points; and, *second*, from deviations observed on a large number of points to compute the most probable value of the deviations on each point. These may be solved mathematically by computing the co-efficients A B C D E from the observations, by the method of least squares; and when the observations are made on equidistant points, the calculations are very simple. The method of doing this is given in a "Supplement" to the "Practical Rules," by Archibald Smith, Esq., published by the Admiralty. Another method, which is the most convenient in ordinary cases, is the graphic method, proposed in different shapes, by Mr. J. R. Napier of Glasgow, and Captain Ryder, R.N.—In this method a straight central line of convenient length—say 18 inches—divided into 360 equal parts, represents the margin of the compass-card cut at the north part, and straightened and extended in the following way:—

N. E. S. W. N.

Through any compass-course taken on this line, and the corresponding correct magnetic-course, two straight lines are drawn intersecting in a point above the central line if the deviation be east, below if west.—Through every compass-course, on which the deviation has been observed, lines parallel to the first set are drawn, and through every corresponding correct magnetic-course, lines parallel to the second, giving a point of intersection for each observation. Then a curve is drawn, called the curve of deviations, passing as nearly as possible through all the points of intersection. We have then the following easily applied solution of the two following problems:—

PROBLEM I. From a compass-course to find the corresponding correct magnetic-course.

Rule.—On the central line take the compass-course. Move parallel to the first set of lines till you reach the curve, then parallel to the second set of lines till you get back to the central line. The point in the central line, at which you arrive, is the correct magnetic-course required.

PROBLEM II. From a given correct magnetic-course to find the corresponding compass-course.

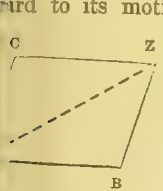
Rule.—On the central line take a given correct magnetic-course, move in a direction parallel to the second set of lines till you arrive at the curve, and then move in a direction parallel to the first set of lines till you get back to the central line. The point on the central line at which you arrive is the compass-course required. In Mr. Napier's plan, both sets of lines make an angle of 60° with the central line, and with each other, so that the lines form a set of equilateral triangles, and the scale on each line is the same as the scale on the central line.—This method is described with diagrams in the "Supplement" to the "Practical Rules" above mentioned, and is also published separately by the Admiralty, under the title, *A Graphic Method of Correcting the Deviation of a Ship's Compass.* In Captain Ryder's plan, the central line is the diagonal of a square, the other lines make angles of 45° with it, and are at right angles to each other, and to the sides of the square, which sides are divided into 360°. The top and bottom representing correct magnetic-courses, the sides compass-courses.—By this method the correct magnetic-course corresponding to a given compass course, or the compass-course corresponding to a given correct magnetic-course is found as if by a table of double entry. A description of this method, with a diagram, has been published by the Admiralty, as a supplement to the "Practical Rules." The two methods, it will be seen, are the same in principle. Mr. Napier's will perhaps be found more convenient in construction by the expert, Captain Ryder's more simple in use by the inexpert.—When the deviation is corrected either by a table or by the graphic method, it must be remembered that the same table or the same curve can only be used so long as the ship remains in the same magnetic latitude, and so long as there is no material change in the arrangement or condition of the iron of the ship. *Whenever any considerable change in the magnetic latitude of the ship takes place, a fresh table should be computed, and a fresh curve constructed from fresh observations.* It should be mentioned, that plans have been proposed for correcting the semicircular part of the deviation by adjustible magnets, which can be readjusted on a change in the deviation; but it seems doubtful how far it is prudent to entrust such delicate manipulation to unskilled hands. Since the first edition of this work, the journal of Dr. Scoresby's voyage to Australia in the "Royal Charter" has been published. This voyage was undertaken with the object of observing the change in the magnetism of an iron ship proceeding from a northern to a southern magnetic latitude. The result showed a change in the compasses corrected by fixed magnets of about one point on the arrival of the ship in Australia, and of two points on its return, and a consequent failure of this mode of correction, indicating a large change in the sub-permanent magnetism. The change did not indicate the existence of any large amount of transient induced magnetism.

Compensation. A method adopted in many cases to neutralize errors of which we cannot get rid. Thus, if we wish to obtain a residual phenomenon, free from some slight effect of irregularity, we introduce an opposite irregularity of nearly equal amount. The compensation balance of a watch, and the methods adopted for preventing the deviation of the ship's compass

‑n the magnetic meridian, are beautiful in- ‑nces of this;—the former tolerably complete, latter but very partially so as yet. The ‑pensation pendulum illustrates the principle ‑iciently. See CHRONOMETER.

Complement. In *Arithmetic*, the comple- ‑nt of a number is the difference between it ‑l the power of 10 immediately above it: thus, ‑ is the complement of 527. In *Geometry*, ‑ complement of an angle is the excess of a ‑nt angle over it.

Composition of Motion and Forces. ‑solution *of Motion and Forces.*—The very ‑portant mechanical and physical problem ‑ally designated as above, is equivalent to ‑:—If a Body in Motion becomes subjected ‑how to a second motion; or, if, to the Force ‑ressing the first motion, there be superadded ‑ffective on the Body, a second Force, naturally ‑sing a Second Motion—what will be the ‑ual Motion of the Body? Or what the Single ‑ce, in obedience to which, it may be supposed ‑nove?—I. The formal reply is this:—1. With ‑rd to its motion of TRANSLATION. If a



body, disposed by any force to move through the space A B, along the line A B, in a given time, should also be subjected to the operation of another force, tending to cause it move in the same ‑, through the space A C, along the line A C it actually move in that specified time through ‑line A Z—the *diagonal* of the *parallelogram* ‑ned on A B and A C, as its *sides*. And supposing ‑body A subjected to *three* forces, lying in differ- planes in space, it will move, in the unit of ‑, along and through the diagonal of the *paral- ‑iped*, formed by three lines representing, in ‑tion and amount, the three elementary forces. ‑n easy application of this fundamental theo- any number of Forces acting at a point may be ‑pounded and their influence represented by a ‑le Force; and, *vice versa*, any Single Force be conceived as the *Resultant* of any number ‑nstituent elementary Forces; and *resolved* into ‑ elements. 2. To the case of Motion of ROTA- ‑, or of Forces causing Motion of Rotation, a ‑ar principle applies. Technically, this part ‑e general problem consists in the *Composition ‑Resolution* of *Couples*. The nature of the solu- will be understood by a single illustration, if a body, A (see preceding figure), rotates west to east, around an axis whose direction ‑, and with a velocity represented by the line and if it be struck anew by a force, that of would cause it to rotate also from west to around an axis whose direction is A B; and a velocity represented by the line A B; then ‑ll actually rotate from west to east, around ‑xis whose direction is the diagonal A Z, and a velocity represented by A Z. In the same ‑er, rotations lying in different planes, and whether in the *same* or in *opposite* ways, may be *compounded*, and of course resolved at will, precisely as with Motions of *Translation.*—II. The inquiry, as to the grounds on which the principle just enunciated reposes—the proposition usually termed the *Parallelogram of Forces*—has been a favourite and fertile one, with persons occupying themselves concerning the Philosophy of the Sciences. We have no room for controversy; and shall merely record our conviction, that the principle formally expressed by the Parallelogram of Forces is not resolvable into any simpler one; and that the proposition itself, is therefore not *deducible*, in the strict sense of the term. It is an *Axiom*, commended by universal experience, that, *if a moving body be impressed with a new motion, or acted on by a new force, it is not thereby hindered from obeying to the full, alike in quantity and direction, the first motion to which we have supposed it subjected.* For instance, whether a ship is at rest or sailing smoothly, all interior motions—actions, let us say, on its deck—take place indifferently, and with the same precise results. Now, the Parallelogram of Forces is nothing more than the technical *rule*, to which this principle gives rise. It will be found, on exact analysis, that every effort to deduce the principle *a priori*, whether geometrically, or by aid of the algorithm of Functions, involves some assertion fully equivalent to the truth pretended to be demonstrated.

Compressibility. Bodies are supposed to be constituted of small indivisible atoms, existing at certain distances from each other, but kept together by attractive forces within the body, as solids; or by coercive forces from without, as liquids or gases. In all cases, except in the case of these indivisible atoms themselves, bodies are supposed to be made up of a number of these particles. All cases which come practically under our observation are included under this statement, for whether or not these atoms exist at all, they have never been found as magnitudes recognizable by our finest senses, assisted by our most perfect instruments. Every body, then, is composed of atoms at certain distances from one another.

From this statement we are naturally led to inquire if these distances can be augmented (see EXPANSION) or diminished. That property of bodies, in accordance with which they can be diminished is called their *compressibility*.

The compressibility of different bodies is different. The relative compressibility of various bodies is readily tabulated, according to the degree of compression obtained by the application of a given force. One thing, however, must be observed carefully. The compressibilities must be measured at one uniform temperature. There is no property of bodies more readily and completely altered than this is, by the influence of heat. Thus water at 212° becomes steam, and is then compressed according to the gaseous laws, in the inverse

proportion of the pressures applied, becoming $\frac{1}{2}$ less by the addition of one atmosphere of pressure, while water itself scarce becomes $\frac{1}{20000}$th less, as we shall immediately see. A heated solid generally takes up much more space than a cold one; and hence the body exists with its particles at greater distances one from the other in the first case than in the second. In this case also they are generally more compressible by outward force. The extraction of heat, indeed, without the application of outward force, produces in almost every case, a degree of compression and *vice versa.* Compression, in almost every instance, is accompanied by evolution of heat. The degree of compression, at given temperatures, varies in solids, according to their other physical properties. A like variation obtains for liquids. They were originally thought indeed to be incompressible. The celebrated experiment of the Florentine Academicians, in which they applied very great force to a ball of thick gold filled with water, in order to compress the water, and found themselves only able to make it ooze through the metallic surface, was held to be conclusive upon the point. But this was very soon doubted. What it did, in fact, prove, was, that it requires a less force to drive water through the pores of gold than it does to compress its bulk. It has been questioned whether it even established this, or whether the percolation of water was not due rather to cracks and fissures in the gold than to the severe pressure to which it was submitted; fissures so small, as not to be visible to the experimenter. Even accepting the other conclusion, we are not forced to believe water and other fluids incompressible. In fact we see that the diminution of heat does compress them, and knowing as we now know, the identity of the nature of heat and mechanical action, this would be sufficient to prove that they are compressible in certain circumstances. But without falling back on this resource, experiments made more carefully than the Florentine one, have shown very clearly that liquids are compressible. Water, for example, has been compressed by the addition of a pressure of 15 lbs. per square inch to the atmospheric pressure (addition of *one atmosphere* of pressure) to lose $\frac{1}{20000}$th of its bulk. It loses by the addition of two atmospheres proportionally more and so on. This law holds, within the limits under which we can safely experiment. We cannot procure pressures of a thousand atmospheres, or construct vessels capable of withstanding them. Pressures of 800 atmospheres have been applied. Other liquids follow the same law, each having a different coefficient of compression for the increase of an atmosphere in pressure.—With gases the case is different. The bulk of gases increases or diminishes in exactly inverse proportions to the pressures. Thus, if we add a pressure of one atmosphere to a gas, it will be reduced to one-half of its original bulk, if a pressure of four atmospheres, to one-fifth, and so on. This law holds for all gases within certain limits. Under violent pressures gases evince a tendency to assume the liquid form. When they reach pressures near those at which they actually pass from the gaseous state, the foregoing law becomes very little reliable. In some instances such passage to fluidity is the result of pressure comparatively slight; with others again, it has not been found possible to procure any indication of an approach to it, by our highest available pressures. Thus the following gases are condensed into liquids at the pressures noted:—

GASES.	
Sulphurous Acid Gas,.............	2 atmospheres.
Cyanogen,.........................	3 "
Ammonia,..........................	5 "
Sulphuretted Hydrogen,...........	15 "
Muriatic Acid,....................	24 "
Carbonic Acid,....................	36 "
Nitrous Oxide,....................	44 "

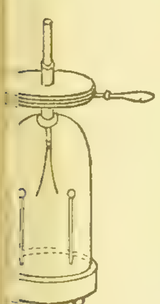
If, however, we keep in mind, in the case of each gas, its point of transformation; we may safely apply the gaseous law above stated to all pressures sensibly lower.

Concave. A surface is said to be *concave* where lines, drawn from point to point in it, fall between the surface and the spectator; and *convex*, where the surface comes between the spectator and these lines. By changing our position one surface may thus become to us successively concave and convex. They are merely relative terms. See LENS and MIRROR.

Concord, in Music. For an explanation of the meaning of this, and of the circumstances in which it obtains, see HARMONICS: for an explanation of the theory, see ACOUSTICS.

Condenser, Electric. The use of this highly valuable instrument is so far indicated by its name. The immediate result which is contemplated in every proper employment of the condenser, is the condensation of electric charges; or, in other terms, the increase of electric tension. It may be added, that the final results most generally aimed at, in the employment of the instrument, are electroscopic and electrometric determinations, in regard to very feeble charges or sources of electricity. And it is, indeed, the extreme importance of such determinations that renders the condenser so valuable as an instrument of research. Suppose, in illustration of these remarks, that a charge has been distributed over such an extent of surface that its existence cannot be detected by the employment of any simple electroscope, such as the electric pendulum. For determining the existence and the species of a charge so diffused, the only known method is to accumulate the charge, or some part of it, upon a much smaller extent of surface than it originally occupies. This accumulation is the work of the condenser. Then, by a simple change in the conditions of the apparatus, we detach the accumulated charge from the original

ace, and make it act on the electroscope. In her illustration, consider a much more im-ant case than the preceding. Every electric ce, with which we are acquainted, is incapable volving electricity beyond a certain tension, ch depends upon the nature of the source. If limiting tension, then, which corresponds to particular source be very feeble, the statical tricity evolved by the source can be detected by a process of accumulation, as in the ner case. The instrument and the process of umulation are the same in both cases. The denser draws in fact upon the source, as upon ery great conductor very feebly electrified. se examples indicate to some extent the im-ance of the functions of the condenser in tro-statical experiment. They indicate also as far as electroscopic purposes are concerned, work of the condenser is just to increase a n charge in a greater or less proportion ac-ding to the power of the instrument. Consider in what way these important results are cted. The condenser consists essentially of e plates in the form of discs with plane and shed surfaces. One of the discs is formed of lating matter. The other two are conductors. the adjoined figure there is a representation of the condenser in the form in which it is most commonly employed. The insulating plate does not appear in the figure, its place being supplied by thin shells of nonconducting varnish, which have been painted upon the surfaces of the conducting plates. As appears from the figure, the upper disc is furnished with an insulating handle, and the lower is



permanent connection with a gold leaf elec-cope. The working of the apparatus is y simple. The upper disc is placed upon lower. The former, which is called the iving plate, is then made to communicate n the source, while the latter, or the condens-plate, is put in communication with the ground. this disposition the given charge is placed in liar circumstances. A conducting path has all but opened up for it into the ground; it is most important to observe that the two aces which limit the interruption in this path very extensive and very near to each other. in these circumstances that the power of the lenser originates. By the disposition above-tioned the condenser is charged. An instant ime is generally sufficient for the completion he charge. To manifest the charges accu-ated in the plates, we suppress the communi-on between the condensing plate and the and, and that between the receiving plate and source. The discs are then separated, and y are both found to be charged, the upper with the electricity of the source, and the lower with the opposite electricity. In a good condenser the intensities of the charges on the two discs differ very slightly from each other, and are much greater than that of the original charge, so much greater, indeed, that they may give powerful effects with the electroscope even when the original charge gives none. That there is a considerable accumulation of electricity in the discs is a fact that may be directly proved by experiment; but we may easily account for this accumulation from known and simple principles. The electricity of the source is diffused, first of all, over the surface of the receiving plate. It then acts by induction upon the condensing plate through the thin shell of nonconducting matter that separates them. The latter plate, being in communication with the ground, receives therefore an opposite charge of electricity. This charge reacts inductively upon that of the receiving plate, and disguises a certain proportion of it, so that an additional quantity of electricity flows into the receiving plate from the source. This additional charge increases the induced charge upon the conducting plate, and the latter charge reacts therefore more powerfully upon the receiving plate. It is an essential part of the theory of the condenser, that this process goes on, until the free charge in the receiving plate is equal to that which it would have obtained in the absence of the condensing plate. We are now in a condition to give a mathematical statement of the theory of the instrument. Let P and Q be the total accumulated charges in the receiving and condensing plates respectively. The circumstances of the charges P and Q are different. The charge Q is entirely disguised, so that if the condensing plate be touched by a conductor it parts with none of its electricity. On the other hand, the charge P consists of a disguised portion and a free portion. But the same principle may be applied to both charges, and it is this—that the total charge induced in either plate has a constant proportion to the total inducing charge in the other plate. This principle is founded on experiment, and must be acknowledged as at least closely approximating to the truth. If, therefore, Q be equal to MP, the disguised part of P will be equal to MQ or to M^2P, for the disguised part of P is the total charge induced by Q, and must be, by the above principle, in the same proportion to Q as Q is to P. The disguised part of the charge P being then M^2P, the free charge upon the receiving plate must be $P-M^2P$ or $(1-M^2)P$; and, by a principle already stated, this is the total charge that the receiving plate would have obtained from the source in the absence of the condensing plate. If M be very nearly equal to 1, which it is in a delicate condenser, it is evident that the condensed charge P is excessively great, compared with the free charge or the natural charge $(1-M^2)P$. In general, the ratio of the former

charge to the latter, or the fraction $\frac{1}{1-M^2}$, is very properly taken as the measure of the condensing power of the instrument. With regard to the charge Q, which is accumulated in the condensing plate, it is equal to M P; and, since M differs very slightly from unity, this charge differs very little from P. This fact excuses the disposition represented in the figure, in which the electroscope is connected with the condensing rather than the receiving plate. The disposition is required for convenience of manipulation, and it is made, as we see, at little or no sacrifice of sensibility in the instrument. After these remarks, it is evident that the best condenser is that in which M most nearly equals 1; in other terms, it is that in which Q nearly equals P, or in which the charge induced in the one disc is most nearly equal to the total inducing charge in the other disc. The entire question as to the efficiency of the condenser may therefore be reduced to this: What are the circumstances which affect the value of M, and how do they affect it? There are three elements upon which M chiefly depends—the diameters of the discs, the thickness of the insulating plate, and the matter of which the latter plate is composed. When the diameters of the discs are increased, and when the thickness of the insulating plate is diminished, M approximates to 1; but there are practical limits to this approximation. The discs cannot be properly handled, nor even properly fitted to each other's surfaces, when their diameters are too great. Practically, the most convenient magnitude of the diameters is between six inches and a foot. Again, when the thickness of the insulating plate is too far diminished, a discharge between the discs may have place through the plate. With delicate instruments, such as that represented in the figure, which are employed for the examination of very feeble charges, the thickness of the insulating shells of varnish on the surfaces of the discs is sometimes diminished to less than $\frac{1}{100}$ of an inch. The value of M in such a case must differ from 1 by an excessively small quantity. The third element which was mentioned as affecting the value of M is the matter of the insulating plate. It is proved, by Faraday's discoveries in Induction, that plates of air, glass, gum-lac, and sulphur, though of equal thicknesses, would have different effects as insulating plates in the condenser. The value of M increases, in fact, from each kind of matter to the following in the above series. The principal reason, however, for the employment of gum-lac rather than the more easily obtained medium of air, is the greater power of the former in resisting discharge through its substance—a power that renders the gum-lac safely reducible to a very small thickness when it is employed as the insulating medium. Before leaving the Theory of the Instrument, we should observe that the value of M can be easily determined for any particular condenser. When the condenser has been charged and insulated, and the plates have been separated, the total charges, P and Q on the discs, may be estimated by the Proof Plane and the Torsion Balance. The ratio of Q to P is M, and from this we easily find the condensing power $\frac{1}{1-M^2}$. Another method is, to estimate first the free charge upon the receiving plate, and then, after having touched the receiving plate with the finger, to estimate the free charge upon the condensing plate. The ratio of the latter charge to the former is the same as that of Q to P, and therefore equal to M. This useful and beautiful instrument appears to have been invented by Æpinus. It was afterwards improved by Volta, and applied by him in his delicate researches in electricity. Since the time of Volta, the condenser has been constantly in the hands of experimental philosophers, and has contributed in a great degree to the progress of electric science. As might be expected, the instrument has passed through a great variety of forms. Among these, there is one that deserves to be specially noticed—Peclet's double condenser. This instrument contains a singular addition to the common condenser—a third conducting plate, which is varnished upon both faces, and placed between the receiving and condensing plates. A charge is first induced in the middle plate, according to the common method. The receiving plate is then removed, and a charge is induced in the condensing plate, which, by its reaction, disguises the greater part of the charge which induces it. The receiving plate is replaced, and the process is repeated. The condensing plate is charged finally with the result of a double condensation. This form of the instrument is very delicate, but its indications are very uncertain, so much so, indeed, that some have spoken of the instrument as utterly useless. The figure, as we have said, represents the most common form of the instrument, as well as the earliest. In illustration of the services that have been rendered to science by the condenser, we may refer to Volta's detection of the electric charges at the poles of the Pile, and to Pouillet's beautiful investigations on the evolution of electricity in combustion and evaporation.

Conduction in Heat. Bodies do not transmit heat with the same rapidity. This can be very readily established by holding pieces of steel wire and hardwood successively in the fire for a few minutes, when the first will become too hot for the fingers, while no difference of temperature in the wood will be noticeable. The heat felt will be due merely to radiation from the fire. In the same way we may melt a glass rod in the fire or before a blowpipe; while the heat is conducted so rapidly through a silver wire that it cannot be held long in the fingers. There is, then, a difference of conducting power in bodies; and those which conduct well, are termed good

›iductors, as silver and steel wire.—Heat pro-
:gated in conduction, is supposed to pass from
ɜ particle to another, which touches it. We
ɔuld expect, therefore, to find some sort of rela-
a between the density of a body and the form of
particles in contact,—and the conducting power.
fficient indications of such relation are already
.nd.—The methods employed to test the con-
:ting power of bodies have been various. Thus,
:genhouz poured boiling water into a vessel,
: the sides of which were holes filled with mer-
ry, into which thermometers dipped; and then
tched the rise of the thermometers. He also
ted bars of several metals with wax, and
tched the rapidity with which the melting
gressed, and the height on the bar to which
rose in a given time. These methods were,
vever, rather capable of indicating the place
a body in a list of conductors, than of ascer-
ining the exact proportions of their conducting
ver. Despretz's experiments were conducted
h square prisms of the matter to be exa-
ned, all coated with the same varnish, so as
radiate similarly. Along the prisms, at
ular intervals, holes were cut, and mercury,
o which thermometers dipped, poured into
m. The bar was then strongly heated at one
remity, and after alternating and varying
newhat, the thermometers at each point came
mark (the heat being kept still applied) a
stant temperature. The range of tempera-
es marked in different substances gave, accord-
to the theoretical principles wrought out by
rier, a measure of the conducting power. An
a of the results obtained will be given by
se got with a copper bar heated by the flame
an argand lamp. The numbers represent the
esses above the temperature of the room in
ich the experiment was performed, noted by
successive thermometers:—

1st 119°·5.	3d 58°·7.	5th 33°·5.
2d 80°·9.	4th 44°·1.	6th 32°·8.

The results at which Despretz arrived from
se experiments are as follows:—

Metals.	Conducting Powers.	Substances.	Conducting Powers.
old	1000	Tin	304
latinum	981	Lead	180
ilver	973	Marble	23·6
opper	898	Porcelain	12·2
ron	374	Fire clay	11·4
inc	363		

These three latter substances, however, cannot
ily be experimented on, and the results given
, at best, doubtful. It deserves notice that a
ıeral law, that the more expansible and fusible
tals conduct worst, appears to be indicated.—
e difference of conduction is very frequently
strated in ordinary life, when we touch dif-
ent bodies at the same temperature; which,
vever, differs from that of the fingers. Suppose
iece of wood and of iron, both colder than the
ɔ, to be touched—the one by the one hand,
and the other by the other. The hand placed on the iron will feel the cold much more than the other, because the iron, conducting heat more rapidly, tends more rapidly to bring down the temperature of a body in contact with it, to what it has itself. This explains why it is very difficult to light a fire in a massive metallic grate, and so easy to do so in a fire-place surrounded with bricks. The former conducts the heat away too rapidly—more so than the kindling matters can supply it; and the latter scarce at all dissipates the heat.

There are many points of view in which this difference of conducting power may be made economically important. Thus, vessels of glass and earthenware ought to be made as thin as possible where they are likely to be exposed to sudden variations of heat and cold. Test-tubes, for example, must be of very thin glass at those points where the heat is applied; if not, the outer glass contracts or expands by heat; while, from the bad conducting power, the inner part retains its heat, and therefore its extension. This is the cause of very frequent breakage in ordinary household ware. So again, in ice-houses, the walls should be built double—enclosing within, all round, a space of air which conducts very weakly, and which will, in consequence, impede the transmission of heat from without to the ice. Here the CONVECTION (*q.v.*) by which apparent conduction through air is produced is not possible, because there is not provided a constant supply of new air. The same plan is adopted in some wine-coolers, in which it is desirable to keep the temperature considerably below that of the surrounding air.

The conducting power of *liquids* is much less considerable than that of solids. Woods hold a sort of intermediate position between the earths and the liquids, probably because there is a mixture of both in their composition. Their conducting power seems to vary from about $2\frac{1}{2}$ to 4 times that of water. Charcoal is very remarkably nonconducting, probably from the amount of watery vapour and of air which it condenses within its mass. Liquids were not, for some time, imagined to conduct at all. They do indeed transmit heat, but it was thought that the principle of *convection* fully accounted for all that they accomplish in this way. Experiments, however, were instituted, and, although no very reliable numerical results have been attained, it has been found that, taking the conductibility of gold as 1000, that of water may be represented by 9 or 10. Comparative tables of their conducting power have not been prepared to any considerable extent. Dr. Thomas Thomson gives the following proportions:—

Water,	conducting power	1	Equal volumes.
Linseed oil,	—	1·111	
Mercury,	—	·2	
Water,	conducting power	1	Equal weights.
Linseed oil,	—	1·085	
Mercury,	—	4·8	

Brewster's very interesting experiments upon conductibility, upon optical principles, are too complicated for insertion here. They will be found in his *Edinburgh Cyclopædia*, and in the *Philosophical Transactions* for 1810.

Gases are still worse conductors than liquids. Their conducting power is, in fact, quite insensible. Doubtless, a room very rapidly gets heated, but it is not so much by the conduction, as by the radiation of heat. Gases do, to a certain extent, unquestionably conduct, as all bodies must; but their transmission of heat is chiefly due to convection. Its existence can be readily proved by holding the hand above and below a flame, at equal distances from it. There is a constant upward current of heated air, which makes it impossible to keep it in the one position; while in the other, though the source of heat is exactly the same, and the distance also, the heat received by the hand is quite inconsiderable. The existence of some air in his vacuum, probably accounts—on this principle of convection—for Pictet's observation, that a bar, heated at the middle in vacuum, heats more rapidly upward than downward. So far this air, indeed, must have existed in the vacuum, because in the pores of every body a certain amount of it is confined, which, on being liberated, would expand out, giving a very rare atmosphere, yet capable of producing sensible effect.

A very remarkable consequence of the small conducting power of gases is, its effect upon the warmth of clothing. In the pores of clothing materials there is always a certain amount—often enormously disproportionate in volume to the other matter of which they consist —of air imprisoned. In some kinds of clothing this air is held with considerable force—in most not. But by such, the temperature of the body is not readily allowed to sink far below the average. The heat which the cold outer masses tend to take from it traverses very slowly the layer of air intervening, which is not permitted to change. The naked body is, indeed, also surrounded by such a layer, but the warm air is allowed to pass off, and be constantly supplied by new air, requiring fresh supplies. Thus the process of convection soon cools us. Clothing generally is quite capable of preventing this, and the greater the proportion of the air to the other material, the warmer is the clothing. Thus, Rumford found that a thermometer cools through 135° when embedded in the following substances, as follows:—

	Seconds.		Seconds.
Air in	576	Fine Lint	1032
Cotton Wool	1046	Sheep's Wool	1118
Raw Silk	1283	Beaver's Fur	1296
Eider Down	1305	Hare's Fur	1315

This represents merely the time taken when the embedding substance was not manufactured. Some of these, as we know, admit of closer texture in manufacturing—some of looser. In the latter case the warmth is increased—in the former diminished. It was thought at first probable that these differences might be occasioned by the different natures of the silk, and lint, and wool, &c.; but it was found that the effect increased when there was less compression, and more air consequently, permitted, and diminished on compression. Radiation has some effect undoubtedly in producing this difference of warmth in clothing; but the foregoing is the chief cause of it. The same principle explains the power of snow to protect vegetation. It also is very porous, and filled like fleece with air. Conduction from the cold upper air and sky down to the snow-covered earth is therefore very difficult, while there comes up from the heart of the world to the cold surface an unchilled supply of genial warmth.

M. de Senarmont has recently published a remarkable series of experiments on the conduction of heat in crystalline bodies. Perfect crystals are usually so small, that this had been thought impracticable. He employed crystalline plates cut from the crystal, according to certain relations with the optical axis, and passed through them a silver wire heated, and kept at a constant heat, by conduction from a fire at a distance. He notes the passage of the heat across the plate, by covering the plate with a thin coating of wax, which melts as the heat advances. At any time there will be formed isothermal curves round the centre of heat; that is, curves along whose borders the heat is equal. If these be circles, the crystal will have conducted heat uniformly in all directions; if figures of other forms, not so. The outline of these isothermal curves will, at any moment, be the border of the wax which divides the melted and unmelted portions. Employing many very delicate mechanical and theoretical refinements, M. Senarmont arrived at the following results:—

1. In crystals of the regular system the conductibility is equal in all directions, and the isothermal surfaces are therefore spheres. It ought to be noticed that, instead of a *plate*, a *solid crystal* is here spoken of.

2. In the right prismatic system, with square base, the conductibility takes a maximum or minimum value in the direction parallel to the axis of the figure; it is equal in all directions perpendicular to this axis, and the isothermal surfaces are ellipsoids of revolution round the symmetrical axis.

3. In crystals of the rhombohedral system, or that of the right rhomboidal or rectangular prism, the conductibility has three values—a maximum, a mean, and a minimum—following three rectangular directions, always parallel to the axis of the crystal, and the isothermal surfaces are ellipsoids, whose three unequal axes coincide with the axes of symmetry.

4. In the system of the oblique rectangular prism, the conductibility takes three different

es, at right angles. The first is parallel to crystallographic axis, and the second and l being connected apparently with no axis ymmetry, depend on the special nature of each vidual crystal. The isothermal surfaces are efore ellipsoids with three unequal axes, only of which is previously determined in posi-

t is only needful to state these laws to show once the very close analogies which subsist veen the propagation of heat and of light ugh crystalline substances.

Conduction of Electricity. A few re-ks are made upon this subject, in the article ELECTRICITY. In the present article, we may r into some of the more important details. duction, according to the common use of the 1, denotes not the transference of electricity eneral, but a particular mode of transference. re are, in fact, four pretty distinct ways in ch free electricity is transferred through space. se are, conduction, disruptive discharge, elec-ytic discharge, and convection. The last of se modes bears the same relation to the conduc-1 of electricity, that the convection of heat rs to the conduction of heat. We have a d example of convection in the case of an elec-ied conductor, insulated in air which contain-ting particles of dust. The particles are ats cted to the body, then electrified by discharge contact, then repelled, taking with them a por-1 of the original charge. By the continuance the action, the charge may be dissipated in a y little time. This instance illustrates the erence between convection and conduction. ctricity is transferred effectively in both ways; in convection, the charge is transferred by aining stationary upon a moving body, while conduction the electricity is transferred through tionary matter. Common conduction is dis-guished further from electrolytic discharge by powerful agency of the chemical forces in the ter, an agency that affects the entire character the electric phenomena. With regard, finally, disruptive discharge, it is true that, from the st violent cases of discharge in this form, to most quiescent forms of conduction, we can ce experimentally a series of cases differing le from each other; yet the common pheno-na of disruptive discharge are so very distinct m those of conduction, that the two modes of nsference are justly described, in general, un-separate heads. The circumstances that af-t the conduction of electricity are various. nong these, the most evident is the specific iducting power of bodies. When two insu-ed bodies are charged with opposite electrici-s, and connected by a rod that touches both, result depends upon the species of matter of ich the rod consists. With one kind of matter charges remain on the original surfaces, with other kind they unite through the rod, and are utralized in whole or part. We say, therefore, that one rod has a greater conducting power than another. With limitations to be afterwards mentioned, it is found that the degree of conducting power is constant for the same kind of matter We therefore speak of the specific conducting powers of bodies. There are such clear distinctions among the various kinds of matter in regard to this property, that bodies have been arranged in tables in the order of their conducibility. At the bottom of such a table, or among the best insulators, are glass, silk, wax, gutta percha, sulphur, spermaceti, oil of turpentine, &c. The metals have been always recognized as the best conductors. The liquids, with the exception of the oils, are good conductors. The latter fact explains the conducting powers of many bodies. The presence of moisture in the pores or upon the surface of a nonconductor, will give it a greater or less degree of conducting power. Without entering farther into this point, we may observe, that the various bodies, natural and artificial, take such a wide range in regard to specific conducibility, that it is common to speak of some bodies as perfect conductors, and of others as perfect insulators. Faraday, however, has hazarded the proposition, that all bodies, from the metals to gum-lac and the gases, conduct electricity in the same manner, though in very different degrees. We have not space here to show how powerfully this proposition is supported by experiment. Another circumstance that affects conduction is, the temperature of bodies. Well-dried glass, at common temperatures, is one of the best insulators; as its temperature increases, it receives a continually increasing conductive power, until, at a red heat, it conducts very well. Similar effects have been observed with other nonconducting bodies, solid and liquid. Temperature affects also the conducting powers of the metals. But it is remarkable that the effect in this case is contrary to that observed in the case of bad conductors; as the temperature of a metal increases, the conducting power diminishes. Another circumstance that affects conduction, is the physical state of bodies, as liquid or solid. It has been long known that, while water is a good conductor, ice is a very good insulator. A globe of ice has been, indeed, successfully employed in the electric machine, instead of the common cylinder or plate of glass. It has been stated further by Faraday, as a very general law, that bodies assume a conducting power during liquefaction, which they lose during congelation. The connection between this law and that of the influence of temperature last mentioned can hardly be doubted. Among the circumstances that affect conduction, we should not omit to notice the dimensions of the conductor and the intensity of the charge. The former circumstance will be taken up more properly in the article on CURRENTS; the latter may be thus briefly illustrated. The same body may act in a Voltaic circuit as a good conductor, and in a Thermo-electric circuit

as a perfect insulator; the reason being this, that the intensity of the electricity evolved in the former case is much greater than that in the latter. For further information upon the subject of conduction, see CURRENTS. We have now to refer to the theory of conduction. In the older theoretical speculations upon electricity, conduction was regarded as the mere passage of the electric fluid or fluids; and the only agency ascribed to the conductor was, that of affording a passage to the electricity with more or less facility. An important addition was made to the theory of conduction by Delarive. He compared a continuous conductor to a series of indefinitely small conductors placed in a line and insulated from each other. If two oppositely charged bodies were put in contact with the extremities of this line, the first effect would be a system of inductive actions, by which every one of the small conductors would be electrified positively on the one side and negatively on the other. The *limiting* case of these inductive actions would be a system of discharges between contiguous conductors, and this would constitute the conduction of electricity through the whole series of small bodies. This view of conduction is evidently to a great extent hypothetical; and yet its importance cannot be questioned. Its value consists principally in this, that it explains the electric properties and actions of masses by the assumption of molecular properties and actions of the same kind. Faraday's *Theory of Conduction* might now be noticed, but it may be taken up much more satisfactorily in the article on INDUCTION. Certain general relations of the subject will be found also under ELECTRICITY.

Cone. A solid, whose base and every section parallel to it is a circle—more generally—any curve returning into itself—and which terminates in a point called the apex. The line from the apex to the centre of the base is called the *axis;* the *height* is the perpendicular on the base from the apex. The ordinary *right cone* has a circular base, to which the axis is perpendicular; the *oblique cone* has the axis inclined to the plane of the base. A *truncated cone* is the lower half of a cone cut by a plane parallel to the base. The cone is in fact a species of pyramid with an infinite number of infinitely small sides. The pyramid and cone depend on the same principles, and the same rules apply to them. The convex surface of the right cone is equal to half the product of the circumference of the base by the side of the cone—the side being a line drawn from the apex to any point of the circumference of the base. The solid content of the cone, like that of the pyramid, is one-third of that of a cylindrical solid of the same base and height, *i.e.* one-third of the product of the base by the height. The volume of a truncated cone is equal to the product of half the sum of its parallel ends by the perpendicular distance between them. Its convex surface is equal to the product of half the sum of the circumference of its ends, by its side.

Congelation, Line of Perpetual. See SNOW.

Congelation — Regelation. (See ICE.) The subject now referred to has recently assumed an importance that, until recently, did not belong to it. We shall discuss its peculiarities and physical bearings in detail under article ICE; but it is needful that a few general remarks be made here.—Congelation is the process by which liquids pass into the solid state, whether through effect of pressure or of the lowering of their temperature. The latter cause was, until recently, held as the chief cause of congelation, and the temperature of freezing water was taken therefore as an absolute point of temperature. But it is now very apparent that temperature is only one element. Gases can be pressed into the liquid form, and liquids into the solid form, by a combination of the two powers,—pressure and the lowering of temperature. Interesting relations between pressure and temperature, in reference to the freezing of water, were first brought out by Mr. James Thomson, C.E., Belfast. (See ICE.) — Furthermore, in reference to temperature alone, the existence of singular anomalies has long been known, and remained as a great puzzle; for instance, liquids often refused to solidify at the canonical temperature. Faraday has recently given forth some striking views on this whole subject:—"In all uniform bodies possessing cohesion, *i. e.*, being either in the solid or the liquid state, particles which are surrounded by other particles having the like state with themselves, tend to preserve that state even though subject to variations of temperature, either of elevation or depression, which, if the particles were not so surrounded would cause them instantly to change their condition. As water is the substance in which regelation occurs, we will illustrate the principle by the phenomena which it presents. Water may be cooled many degrees below 32° Fahr., and yet retain its liquid state for, as far as we know, any length of time without solidification; yet, introduce a piece of the same chemical substance, ice, at a higher temperature, and the cold water freezes and becomes warm. It is certainly not the change of temperature which causes the freezing, for the ice introduced is warmer than the water. We assume that it is the difference in the condition of cohesion existing on the different sides of the changing particles which sets them free and causes the change. The cold water particles would willingly, as to temperature, have solidified without the ice, but were held fluid by the cohesion with them of other like fluid particles on all sides. In the other direction, Donny's experiments have taught us that the cohesion amongst the particles of water is so great, that it will support a column of the fluid four or more

et high, when there is no other power to sus-.in it; or will cause it to resist conversion into ıe state of vapour at temperatures so much gher than its ordinary boiling, or condensing ›int, that explosion will occur when the conti-ıity, and therefore the cohesion, is destroyed. he water may be exalted to the temperature ' 270° Fahr., at the ordinary pressure of the .mosphere, and remain as water; but the intro-ıction of the smallest particle of air or steam ill cause it at once to burst into vapour, and at e same time its temperature falls.—This ability hich water has to retain by cohesion its liquid ate, refusing to solidify when below the freezing ›int, or to become vapour when above the boil-.g point, it has in common with many other bstances."

Congruences. Two whole numbers, a and .are said to be congruous to each other, with spect to the modulus m, when the difference — b is divisible without remainder by m. The lation of congruity is in itself so elementary .d patent, that it has attracted the notice of athematicians from the very infancy of the eory of numbers. Many important proposi-ms in that theory are mere statements of con-uity in particular cases—such as Fermat's eorem and Sir John Wilson's. But the subject .s risen into new importance since the publica-m of Gauss's *Disquisitiones Arithmeticæ*, a work which many original results of an interesting ıd valuable kind were obtained by a thorough ıdy of relations of congruity. Gauss proposed new notation, which is very likely to prevail ıiversally: he writes a *Congruence*, or a state-ent of congruity, between a and b, in a manner ry similar to an equation between a and b, only ith three lines instead of the two =. An im-ediate advantage derived from the notation is, at it reminds us of the close analogies which ist in fact between congruences and equations, ıd enables us to submit congruences to many ınsforming operations with the greatest ease, ıd without any danger of confusion. After auss, other mathematicians have been occupied ith the subject, especially Poinsot, Gallois, and bri. They have found the properties of con-ıences to be of the utmost value in the higher rts of algebra and of the theory of numbers. ıt the most valuable result obtained from the ıdy of congruences undoubtedly is, that by is means the theory of numbers has been saved m the reproach and inconvenience of an utter scontinuity of parts. A large part of the eory of numbers might now be thrown into a m as systematic and as closely reasoned as the eory of equations, and in many points bearing close resemblance to it. The best connected .position of the subject will be found in Serrets' *gebre Superieure*, where three chapters (23, 24,) are devoted to it. The English student of e higher Algebra would do well to master this portant work.

Conic Sections. Four curves formed by sections of the right circular cone made by a plane in different directions. If the plane cutting the cone be parallel to the base the curve will be a circle. If it cut it slant across, cutting the two sides, the curve will be an ellipse. If it be parallel to the side of the cone it will be a parabola, and any other section will be a hyperbola, if it be inclined to the axis at a less angle than the side of the cone. If two cones were set one on the top of the other, so that the one is just a continuation of the other through the apex, a plane which by section would make a hyperbola would cut the second cone as well as the first, though none of the other planes would. There are thus two equal branches of the hyperbola belonging to the two cones respectively.

Other methods of describing the conic sections on a plane are frequently given. From that already specified the name comes. The circle is described by a compass. The method of describing the ellipse, parabola, and hyperbola, is mentioned elsewhere. The general equation which represents all of the conic sections is $y^2 = mx + nx^2$ —, in which, for the parabola $n = o$, for the ellipse $m = \frac{2b^2}{a}$ and $n = -\frac{b^2}{a^2}$, for the circle $m = 2a$; $n = -1$, (b and a being equal), and for the hyperbola $m = \frac{2b^2}{a}$ and $n = \frac{b^2}{a^2}$. See ELLIPSE, HYPERBOLA, PARABOLA.

Conjunction. When two or more bodies are seen very nearly at the same point of the sky, we say that they are in conjunction. Conjunctions are *geocentric* or *heliocentric*, *i.e.* we *do* see the bodies together or nearly so, in the former case; and in the latter we *would* see them so if we observed from the sun. It is always necessary to remember that our observations at the surface of the earth require to be reduced to what they would be at the centre. This is customary in order to secure a method of marking the exact times of conjunction, and that every observer may make his observations, at once useful to every other. The conjunctions most frequently mentioned are of the planets and sun. When the sun and a planet are in the same direction from us, they are in conjunction. The terms *inferior* and *superior* conjunction refer to this. Some of the planetary orbits are within the earth's orbit. These are called *inferior* planets, such as Mercury and Venus. Now they will clearly be in conjunction with the sun twice: 1st. When they are between us and the sun; and 2d. When the sun comes between us and them. The former is said to be *inferior conjunction*, the latter *superior conjunction*. The *superior* planets, whose orbits lie entirely without that of the earth, such as Jupiter, never come between us and the sun, and therefore there is no *inferior conjunction* with them. When, however, we come between

them and the sun, they are said to be in *opposition* to the sun.—*Grand conjunctions* are said to be those where several stars or planets were found together, or near one another. Thus, on March 17th, 1725, Mercury, Venus, Mars, and Jupiter, were all to be seen by the same telescope at the same time, without altering its direction. The Chinese history records a grand conjunction, in the reign of the Emperor Tehuen-hiu (2514-2436 B.C.), of five of the planets. Astronomers calculate that such a conjunction actually took place in 2461 B.C., when Saturn, Jupiter, Mars, Mercury, and the Moon, were within 14° of one another. The moon could, however, scarce be mistaken for a planet, and in all likelihood the grand conjunction is merely a conjunction of three or four planets, of much more recent date, magnified into five for the glory of the Celestial Empire.

Connecting Rod. A rod in a machine intended to transfer motion from the place of its generation to a point where it is required. It is attached to one point which moves directly, and to another point intended to move. The motion of the former, is by it communicated to the latter. There are different forms of it, according to the kind of motion originally produced and the kind which may be required in the point to which it is fastened. See *Willis's Principles of Mechanism.*

Conoid. A solid formed by the revolution of a conic section round its axis. The sphere, the paraboloid, the ellipsoid, and the hyperboloid, are the various conoids.

Conservation of Force. See Article FORCE.

Constellation. The first appearance of the celestial vault gives us no idea of order or uniformity. The stars seemed strewed up and down; and we should not expect, therefore, in this irregularity to find them keep their relative positions. They do so, however—with a few exceptions (planets)—and hence the vault, night after night, presents the same appearance to the spectator. This makes it possible for the observer to watch all the phenomena of any one star. If the stars were visible in the day-time, he might follow it round and round its circuit. But the sun blots out the stars in the morning, and the night restores them to the sky. All this time the stars have been moving, and are, therefore, not found in the same place in which he left them in the morning. How, then, shall he rediscover his lost star? In this difficulty, he has recourse to the *grouping* of the stars. He notices three or four which have a peculiar configuration —which stand at certain distances—and which appear with certain lustres. These are different in their total appearance, from any other equal number in the sky. He recognizes the group then at night, and each star by its individual position in the group. This grouping, arbitrary for each observer, would be sufficient, if he could remember it distinctly, and if he confined his observations entirely to himself. But he wishes to communicate what he observes, and to exchange communications. In order that he may do so, a *uniform* system of grouping is adopted, and this system is also intended to assist the memory. Unfortunately, this last end has been very inadequately attained. The groups are called CONSTELLATIONS.—The system of grouping universally employed bases principally upon the Grecian mythology. Figures of men and of beasts are supposed to be outlined in the sky by various groups of stars, and mythological names are assigned to them. The stars in each constellation are named by the letters of the Greek alphabet (α, β, γ, &c.), in the order of their brilliancy, and when there are more stars than there are letters, the ordinary numerals follow in succession. The division of the stars into constellations is very ancient. It is mentioned in Job, and in Hesiod and Homer. Aratus made a formal arrangement; but the first generally received was that of Ptolemy, in which the heavens, *visible to him*, were divided into 48 constellations—12 in the zodiac, 21 in the northern, and 15 in the southern hemisphere; Hevelius added 12 new constellations, Halley 8, Bayer 12, La Caille 15, and different other astronomers 12 more. The complete number is thus 107. The following is a list of the constellations:—

CONSTELLATIONS OF PTOLEMY.

Northern.

1. Ursa Minor (the Little Bear).
2. Ursa Major (the Great Bear).
3. Draco (the Dragon).
4. Cepheus.
5. Bootes (the Ox Driver).
6. Corona Borealis (the Northern Crown).
7. Hercules.
8. Lyra (the Lyre).
9. Cygnus (the Swan).
10. Cassiopeia.
11. Perseus.
12. Auriga (the Charioteer).
13. Ophiuchus (the Serpent Bearer).
14. Serpens (the Serpent).
15. Sagitta (the Arrow).
16. Aquila and Antinous (the Eagle and Antinous)
17. Delphinus (the Dolphin).
18. Equuleus (the Little Horse).
19. Pegasus.
20. Andromeda.
21. Triangulus (the Triangle).

ZODIACAL CONSTELLATIONS.

22. Aries (the Ram).
23. Taurus (the Bull).
24. Gemini (the Twins).
25. Cancer (the Crab).
26. Leo (the Lion).
27. Virgo (the Virgin).
28. Libra (the Balance).
29. Scorpio (the Scorpion).
30. Sagittarius (the Archer).
31. Capricornus (the Goat-horned).
32. Aquarius (the Water Carrier).
33. Pisces (the Fishes).

SOUTHERN CONSTELLATIONS.

34. Cetus (the Whale).
35. Orion.
36. Eridanus.
37. Lepus (the Hare).
38. Canis (the Dog).

39. Procyon.
40. Argo.
41. Hydra.
42. Crater (the Goblet).
43. Corvus (the Crow).
44. Centaurus (the Centaur).
45. Lupus (the Wolf).
46. Ara (the Altar).
47. Corona Australis (the Southern Crown).
48. Piscis Australis (the Southern Fish).

MODERN CONSTELLATIONS.
I. Added by Hevelius.

1. Antinous.
2. Maenalus.
3. Canes Venatici (the Greyhounds)
4. Asterion and Chara (the Giraffe).
5. Cerberus.
6. Coma Berenices (Berenice's Hair).
7. The Lizard.
8. The Lynx.
9. Sobieski's Shield.
10. The Sextant of Urania.
11. The Little Triangle.
12. Leo Minor (the Lesser Lion).

II. SOUTHERN CONSTELLATIONS.
Added by Halley.

1. Columba (the Dove).
2. Charles's Oak.
3. Grus (the Crane).
4. The Phœnix.
5. Pavo (the Peacock).
6. The Indian Bird.
7. Musca (the Bee).
8. The Chameleon.

III. SOUTHERN CONSTELLATIONS.
Added by Bayer.

1. The Indian.
2. The Crane.
3. The Phœnix.
4. The Bee.
5. The Southern Triangle.
6. The Bird of Paradise.
7. The Peacock.
8. The Toucan.
9. The Male Hydra.
10. The Dorado.
11. The Flying Fish.
12. The Chameleon.

SOUTHERN CONSTELLATIONS.
Added by La Caille.

1. The Sculptor's Workshop.
2. The Chemical Furnace.
3. The Astronomical Clock.
4. The Rhomboidal Net.
5. The Graving Tool.
6. The Painter's Easel.
7. The Compass.
8. The Air Pump.
9. The Octant.
10. The Square and Rule.
11. The Telescope.
12. The Microscope.
13. The Fabulous Mountain
14. The Great and Little Cloud.
15. The Cross.

HE REMAINING MODERN CONSTELLATIONS.
Added at different times.

The Reindeer.
The Hermit.
The Farmer.
Poniatowski's Hawk.
Frederic's Honours.
The Sceptre of Brandenburg.
Herschell's Telescope.
The Balloon.
The Quadrant.
The Cat.
The Log.
George's Harp.

There are several maps of the stars, in which the constellations are usually traced out as on the celestial globe. Flamsteed's Celestial Atlas, London, 1729, and Bode's, Berlin, 1805, are the best. There are several excellent maps published by the Society for the Diffusion of Useful Knowledge.—The arrangement of the constellations as they stand is not very natural. It is still, however, employed for the purposes of reference—and as the point of first importance in such matters is the uniformity of the standard, it is not necessary to disturb it. The Chinese and Japanese, indeed, depart from this uniformity, but their astronomy has not yet been very extensively introduced into Europe.—In consequence, however, of the slight esteem in which our system of constellating the stars has been held, various attempts, sufficiently amusing, have been made to change it. Some pious astronomers, grievously scandalized at the employment of the names of fabulous divinities by Christian men, gave a spiritual sense, where it was possible—making Aries stand for the ram that was substituted in the room of Isaac—and others, as the venerable Bede, finding this method rather unsatisfactory, cut the knot, and placed the twelve Apostles triumphantly in the twelve zodiacal signs. This reformation was carried over the whole of the constellations, and all the profane of the old times were swept away.—Weigelius of Jena adopted another plan. He taught heraldry by the stars, substituting for the old constellations the arms of the various princes of Europe, and the insignia of the different crafts. Thus Ursa Major, being a quadruped, became the elephant in the Danish arms; and the Pleiades were metamorphosed into an Abacus, which he would have to be the symbol of merchants. Those divisions have passed away, and the reign of Orion and Bootes still endures.

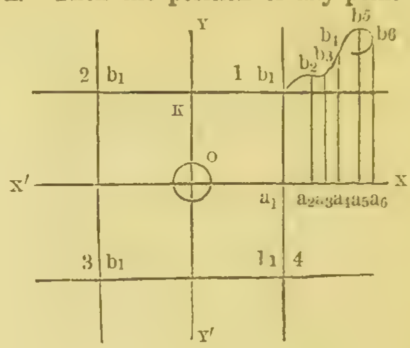
Convection. That property of liquids or gases by which, when in a free state, constant motion is secured to them, through changes of temperature. The term is employed chiefly in connection with the theory of dew. When a windy night arises, the deposition is said to be prevented by convection. This is not properly convection, but from its effects being analogous, it is so termed. The air as it cools is carried away and replaced by fresh air coming from or passing to those quarters of the earth where the sun is yet shining. When a mountain intervenes, too, the hotter air above intermingles with the cooler air below, by force of the current, and is carried over, so intermingled. Again, the cooling of air, and the deposition of dew upon hill tops is prevented by convection. Before there can be an approach to such a deposition, the temperature must become very low, from the lowness of the dew point upon hill tops. When it becomes so, the air is considerably heavier than that, in lower strata, farther removed from the actual, irregular surface of the earth. It commences, therefore, to slip down

the hill side, and thus lighter air comes to take its place, is again cooled, and again creeps down before depositing any or much dew. The heat which these successive strata give to the earth prevents its cooling so rapidly, and they themselves, on whose cooling the dew depends, are thus carried away before they can cool. It is only at the plane surface of the earth, or in valleys, where they cannot take place, that dew is plentifully formed.—Convection again assists the formation of dew by carrying up the air from which part of its vapours has just been condensed, and which so becomes light by the disengagement of the latent heat of its vapour. Air not so heated and with all its vapour falls down, and the process recommences. The difference between this and the last case is, that the dew is formed here before the air is allowed to move. Liquids are generally heated by convection—when heat is applied from below. Thus water, held nearly vertical and boiled in a test tube, to the bottom of which a spirit lamp is applied, scarce at all conducts the heat. If, therefore, the weight of hot and cold water were the same, the layers below might be at a very high temperature and those above in their ordinary condition. But the heated water being lighter, ascends, and lets cold water down to the heated place, which in its turn reascends, and so continues the process. Some liquids, such as mercury, are not bad conductors, but in the great majority of instances it would be very difficult to heat liquids thoroughly but for the property of convection. It is with the utmost difficulty that any conduction is detected in water. If we place a lamp in a vessel which floats in water, the vessel itself freely conducting heat, the layers below will be long before they take a heat sensible to the thermometer, and even then the heat seems conducted down along the side of the glass, or other containing vessel, rather than through the water.

Convergent. When a series of magnitudes is such that, by taking the sum of as great a number of them as we choose, we do not arrive at a result above a certain finite quantity, the series is said to be convergent. The name originates in the fact that the terms of such a series must successively decrease, the second being less than the first, the third than the second, and so on.—The same term is applied in physics to the lines of rays which, originating at different points, go on constantly approaching one another, so that if they be lengthened out sufficiently they will meet at a point. See LENS and MIRROR.

Convex. See CONCAVE.

Co-ordinates. The method of co-ordinates is due to Des Cartes, and is perhaps his most valuable contribution to mathematical science. It first enabled us to indicate *quality* by the ordinary signs of *quantity*, and to compare different qualities quite as accurately as we had been wont to compare quantities. We can only give a very meagre outline of it. Suppose that we have a curve, such as $b_1 b_2 - - - - b_6$ upon a plane surface, on whose plane two lines at right angles to one another, X X′ and Y Y′, are drawn. Then the position of any point in the



plane will be quite determined if we know its distance from the lines Y Y′ and X X′. Thus, if we know the distance of a point from the line X X′, we will evidently cut off a line O K equal to that distance, and through it draw a line parallel to X X′. Any point in this line will answer the conditions of the question, for any such point will be at the specified perpendicular distance from X X′. Again, if we know the distance from Y′, a similar line through a_1, (Oa_1 being equal to that distance) will contain all the points that can answer to the conditions. Since then the point specified must be in each of those two lines, it can only be in that point where the two lines meet. But it will be said, are there not two lines each of which may be the locus of points at given distances from X X′, Y Y′? (the *axes* of co-ordinates.) Thus may there not be four points, the four marked b_1 in the figure, each answering to the specification fully? According to geometrical principles undoubtedly *yes*, but the algebraical extension of this method enables us to specify which of these four possible points we mean in any case. This introduces us to the first glimpse of an estimation of quality. If the measurement of the distance from the line Y Y′ is to proceed along X′ X from O towards the right, the distance is considered as positive and marked by a quantity representing its absolute amount, with the sign $+$. Similarly, if we are to measure from O, along O X′ towards the left, we should indicate it by prefixing the sign $-$, to the absolute quantity representing its length. And again, if we are to measure the distance from X X′, upwards from O along Y Y′, we should have $+y$; and if in the opposite direction, $-y$. Suppose then that we have x and y both positive, we should have the point marked 1. If we have $+y$ and $-x$, the point marked 2; $-x$ and $-y$, the point marked 3; $-y$ and $+x$, the point marked 4; supposing always that Oa_1 represents x, and O K represents y.—The axes, or lines of reference, need not be at right angles but oblique in any way to each other, and in that case the distance would most

…perly be estimated, not by the perpendiculars …n the point upon the lines of the axes, but … the lengths of lines from the given point …allel to the one axis and intercepted against … other. When we have the position of a …ve with respect to any given axes, and the …ition of a new set of axes, with regard to … old ones,—given, it is plain that methods …y be discovered for determining the position of … curve, with respect to this new set. The …blem which proposes this is that of the …nsformation of co-ordinates. The various …nulæ which enable us to solve it are given … every work on analytical geometry. — The …der will be able to comprehend what is …nt by an *equation* to a curve from this. …is, in fact, an algebraical formula which …resses the value of y in terms of the value of … If now any value whatever be given to x, …orresponding value will be found for y, and …s there will be a point in the curve found. …s, suppose that Oa_2 is given as a value of …n any equation, and that by solving the …ation we find a_2b_2 to be the value of y, then … point of the curve which corresponds to those …es will be very readily found by drawing …2 parallel to O Y, and making it equal to y, …ust found. The point b_2 will be a point in … curve. Suppose now that for the successive …es Oa_2, Oa_3, Oa_4, Oa_5, Oa_6, of x there be …d corresponding values $a_2\,b_2$, $a_3\,b_3$, $a_4\,b_4$, …b_5, $a_6\,b_6$ for y, the points b_2, b_3, b_4, b_5, … will be points of the curve. If the number …he values of Oa thus taken be almost infinite, … that the distances between two successive …nts a, become indefinitely small, those between … successive points b, will become so likewise, … by joining them we shall obtain a polygonal …re very closely resembling the curve which … equation indicates. The nearer these points …e to one another, the closer the resemblance. … will be quite manifest that in this way a …resentation of all *qualities* of the curve may … made in the original equation. When it has …n to its highest point, we shall get the great-… possible value of y to match. When it cuts … line of the axis, y will be equal to o; when …s parallel to the axis of x, two very near …nates y, y′, will be equal; when the curve …minates in one direction there will be no value … for values of x beyond a given point—when …e are two values of y for one of x, as $a_5\,b_5$ … Oa_5, the curve has returned upon itself, … so on. Thus every quality of the curve … be represented in the equation, through the …form reference to co-ordinates.—What are …d co-ordinates of three dimensions are three …s, each of which is perpendicular to the …e of the other two—in rectangular co-ordi-…s. In oblique co-ordinates, the three lines … any three not in one plane. By means of … co-ordinates we can refer curves which are … entirely in one plane, the lines through which we make reference being parallel to the axes as before. The methods of transformation of co-ordinates of three dimensions are a little more complicated than those where there are only two. The three ordinates are usually marked x, y, z, the signs + and — being prefixed according as the reference is to the one or other half of the complete axis. With merely x, y, z specified without regard to sign, there could be 2^3, (2^2 in the last instance) or 8 points, answering to the specification.—The method of co-ordinates is much used for resolving forces in mechanics and for finding the value of the resultant of a number of them. Each force is resolved into three others, one along each axis of co-ordinates, and the algebraical sum of all these components for each axis is taken, the resulting three being compounded into one force, which is the complete resultant. Where all the forces are in one plane, we employ co-ordinates of two dimensions.

Copernican System. The correct description of the general character of the planetary motions is termed the Copernican System; for the sufficient reason that the planetary character of the Earth was first discovered by the immortal Copernicus. It is often difficult, when one observes a change of direction in external bodies, to dissever their *real* from their *apparent* motion —the latter arising from change of place on the observer's part. The planets, for instance, seem to pass through various and irregular courses in the sky. How much of this apparent change of place is real?—how much owing to change of place on our part? The ancient world conceived the Earth stable, and thereforce accounted all apparent external motion to be *real:* hence the complexity of their Astronomies. Copernicus determined otherwise. He asserted that the rotation of the Earth on an axis, explains the apparent diurnal revolution of the entire celestial vault; and, by the still more venturous proposition, that the Earth is a planet, revolving annually in an orbit around the Sun, he removed all primary difficulties from Astronomy. His views, however, were *hypothetical.* He could not demonstrate the fact, either of the annual *revolution* or of the Earth's *rotation.* The former was not exhibited as *a fact* until Bradley discovered the *Aberration of Light;* and the demonstration of the latter, *as a fact*, we owe quite recently to the Frenchman, Foucault. (Compare EARTH, *rotation of.*) It is quite an error to suppose that the Copernican Theory, or anything akin to it, had been foreshadowed by the Greek, Philolaus. The only real anticipation was by Aristarchus of Samos, whose writings Copernicus did not know. For details, see SOLAR SYSTEM.

Corona. See CROWNS.

Corona Australis (*the Southern Crown*). A constellation of the southern hemisphere. It contains no very bright stars.

Corona Borealis (*the Northern Crown*).

Its companion constellation in the northern hemisphere. Neither does it contain any above the third magnitude.

Corpuscular Theory. See MOLECULAR.

Corrections. What is termed the *Theory of Corrections*, has come to be of vital importance in all the exact sciences depending on the use of instruments and measurements. An instrument, however skilfully made, cannot pretend to the attribute of absolute exactness; but as the highest exactness is required, the modern observer is, therefore, never satisfied with the best instrument with which art can furnish him. On receiving it, it is his first duty to suppose *that it is all wrong, in every part.* And his next is, by carefully comparing it either with another instrument already tested, or with certain known unchanging facts in Nature, to detect *the nature and amount of its errors.* These obtained, the observer *corrects*, or allows for them, in every individual observation; and only, after effecting these CORRECTIONS, does he consider his act of observation complete.—It would be impossible within our space to survey in detail a subject so extensive as this. Suffice it to advert to a few leading points. I. An instrument may be faulty in *position* merely. For instance, the Transit telescope of the Astronomer may not be quite in the *meridian;* its *axis* may not be perfectly *level;* and its central vertical wire may not be in the optical axis of the tube, *i.e.*, it may be out in *collimation.* Each of these errors must affect every observation; so that as they cannot be entirely removed mechanically, their amount must be measured, and their effect on the observed transit of every star, *corrected for.* It is the same with all other instruments. II. Again, the *structure* of the instrument may be such, that no one part of it, taken by itself, can be made absolutely exact. For instance, to subserve the purposes of an astronomical circle, the circle used should be a perfect circle, and its pivots absolute cylinders. The mechanician cannot realize such a requisition—such perfection being, for the most part, *ideal;* but his art is, to contrive the instrument so, that *in virtue of its structure*, the various errors of its separate parts *compensate each* other. This is the secret of the only possible *mechanical* CORRECTION. III. Errors of construction will remain however, which cannot be compensated. And these, the observer must ascertain and correct for. Of such sort, for example, are the false zero point of a thermometer, or inequalities of the bore of its tube. It is astonishing how far solicitude and skill may go to effect correction, for even complex and apparently unmanageable mechanical errors. For instance, that most faulty meridian circle at the Cape of Good Hope—a circle in which literally no part was right—became, in the hands of the late Mr. Henderson, the means of first detecting the *parallax* of a Fixed Star. Nevertheless it needed the rare skill of that eminent Scottish observer—one of the foremost in Europe—to deal with the comparatively intractable case. To raise his instrument above such defects, is the pride of the great mechanician.

Correlation of Forces. See FORCE.

Couples. The theory of couples, introduced by Poinsot, is now universally accepted by writers on mechanics. We shall require, therefore, to show what it is, and what it means.

When two parallel forces act at the extremities of a rigid rod, if their directions be the same, they may be replaced according to the theory of parallel forces, by a force equal to their sum and in a line parallel to their lines of action, acting at a point *between* them. When they act in opposite directions, they may be replaced by one equal to their difference, acting in a line parallel to their lines of action, in the direction of the greatest, but *beyond* the points where they are applied. The nearer the two forces, in this latter case, are to equality, the farther does the point of application of the resultant move outwards. When they become equal, the resultant becomes zero, and the point of application of the resultant is placed at an infinite distance from the points of application of the original forces. We have, therefore, in this *limiting* case, a zero force, with an infinite lever arm for the resultant, that is, the result that we obtain indicates nothing but that we must seek for some other method to arrive at one. In fact, what action will such a pair of forces exert? Manifestly, they will turn the rigid rod round. Suppose, for example, two ropes attached to the bows and stem of a ship, and pulled in opposite but parallel directions with equal strength. The ship will not move forward in the one way or in the other. The relation in which it stands to the two directions is quite the same over the whole. The relation in which the middle point stands to them is perfectly so. A point nearer the bows will be influenced rather by the nearest force, and a similar point near the stem by that opposite which is nearest it. There will thus be a rotation of the system round the middle point or pivot, all the other points moving in circles about it. The effect of a couple, therefore, is to produce *rotation.*—A couple, then, producing essentially *rotation* cannot be replaced by one force (or by any number of forces which may be replaced by one), which produces *translation.* When we have, therefore, to consider the motions which a set of forces impress on a body, or the conditions of its equilibrium when they are applied to it, we have to keep these two things perfectly distinct. We obtain thus, through the Theory of Couples, this answer in those cases:—The motions resulting will be such a motion of translation, along with such another motion of rotation; or such an expression for the total force inducing translation, and such another for that inducing rotation must be each equal to zero, in order that there may be equilibrium.—Where there is left in a system a force and a couple, if the force be not perpendicular in its line of action

: the plane in which the forces of the couple are ...tuate, it may be convenient to reduce it to two ...rces—not, however, acting at one point. It is ...t possible, in any case where a couple and a ...rce not in its plane are found, to reduce ...em to a single couple or to a single force. ...here the resulting force is in the plane of the ...uple, the reduction to a single force is always ...ssible, and, therefore, since a force and a ...uple are inconvertible, they cannot be reduced ... a single couple.—The line which joins the ...o points of application of the parallel forces in a ...uple is called the *lever arm* of the couple. ...hat is called the *moment* is the product of the ...rpendicular distance between the two lines of ...rce, and either force. The axis is a line drawn ...rpendicular to the plane of the couple, and ...oportional in magnitude to the moment. It ... immaterial where the axis be placed, as re...rds the forces of the couple, for any couple in ...e same plane, and having the same moment as ...other, is equivalent to it. Hence two parallel ...es of equal length represent equivalent couples, ...d, if in opposite directions, destroy one another. The *axis* is always used instead of the couple. ...hen we wish to ascertain the conjoint effect of ...o couples, we treat the two axes as we would ...o forces, with this difference, that we can ...ways transport either axis parallel to itself, ...l so obtain a resultant axis, whose length re...esents the moment of the resultant couple, and ...ose direction is perpendicular to its plane.—The ...ee technical equations of equilibrium which ...press that the resultant couple must in that ...e be zero, are given under EQUILIBRIUM.—An ...estigation of the two propositions funda...ntal in the Theory of Couples that we have ...icated—which the student perhaps, keeping ... mind the proposition of the parallelogram of ...ces, may discover for himself—will be found in ...*hamel's Cours de Mecanique*, vol. i., *Pratt's ...chanics*,

Co-Variants. See POLYNOME.

Crowns: Coronæ. When the sky is covered ...h light clouds, we often see a coloured circle ...which red predominates, surrounding the moon ... the sun,—its diameter comprising only a few ...grees. At other times several concentric rings ... observed, separated by intervals in which ...en predominates: these are *Crowns* or *Coronæ*. ...e corona is not complete unless there are ...eral series of concentric circles. The circle ...rest the sun is of a deep blue; the second ...le is white; and the third red, which termi...es the first series. In the second series the ...cession of colours is purple, blue, green, pale ...low, and red. The cause of the phenomena ...IFFRACTION. See also ANTHELIA.

Cryophorus. A word signifying a carrier ...old. It is the name of a very pretty little ...rument illustrative of the principles of evapora...n, whose form is given in the figure. It consists ...ely of a tube with two bulbs, B and C. In constructing the instrument, one of the bulbs, as C, is at first open, and by it water is poured into the vessel till the other bulb stands about one-third full. This water is then boiled, and

the whole instrument fills rapidly with steam. When all the air is expelled by the steam, and only a clear jet of steam issues from the orifice, it is closed by the blowpipe, and the tube thus hermetically sealed. The instrument is then allowed to cool down with its end B downwards. The water gradually flows back to it, and a very small quantity of vapour of water is left in the rest of the instrument. If now C be surrounded with any freezing mixture, or in any way be subjected to a degree of local cold below 32° Fahr., the water in the bulb B will very soon freeze, and it is this phenomenon which the instrument is intended to effect.

The phenomenon is explained thus. Every quantity of water gives off vapour, and if it pass into a space where the temperature is kept at a certain point, and be not allowed to pass off from that space and dissipate, there will be a certain amount of vapour and no more given off. This amount increases with the increase of temperature. Now suppose the temperature suddenly lowered. The space can no longer hold so much vapour, and the superabundant quantity will be condensed. Now there is a certain amount of heat latent in every quantity of vapour, and when condensation takes place this heat is liberated. See HEAT, LATENT. The temperature of the space where condensation has occurred will therefore—the moment after all the vapour superabundant has been condensed be raised so that it is capable of receiving more vapour which the water will immediately give off. If the cold be applied again, the vapour will be recondensed, new heat given off, new vapour formed and again condensed. In the cryophorus this process goes on, when a freezing mixture, or any method of supplying local cold, is applied to the bulb C. The vapour in C is condensed. This condensation liberates heat, giving capacity in C for more vapour, supplied immediately from B, and recondensed by the continued application of cold. That the condensation of vapour then, liberates heat, is the principle illustrated, next to the theory of states of saturation of vapour, corresponding to temperature, in the bulb C. The condensed vapour does give off heat, because in being formed it takes in latent heat. Thus to send off vapour at any temperature from water, we must use much more heat than in raising the water to the same temperature, without permitting vapour. A quantity of heat not sensible

to the thermometer is essential to the existence of a body in a vaporous state. When, therefore, the bulb, C, suddenly raised in temperature by the condensation of vapour into water, demands to obtain more vapour from the water in C, the heat for this new vapour must be obtained. It is got from the water in B, and the process goes on so rapidly in ordinary cases that the air without is not able to supply heat as rapidly to B, and the water inside, as it thus loses it. The temperature is thus gradually lowered, and ice is formed.—The principle of the cryophorus explains many of the most interesting phenomena of meteorology. We shall see that the formation of dew, and fogs, and mists, is very closely connected with it. The rapid formation of the ice, and the quantity formed in a given time, may be taken as rough measures of the power of the freezing mixtures of which we treat. The phenomenon is in any case interesting, and it is easily shown not at all to depend upon actual conduction or radiation of cold (or inverse radiation of heat), by placing a little water quite near the cryophorus—which is not affected almost at all sensibly to the thermometer by this conduction or radiation.

Cube. A solid with six faces, each of which is a square, each square being therefore parallel to the one opposite it. It is a frequent form for crystals. Those that assume it are not doubly refractive along any line of ray. It was a celebrated problem in the ancient geometry to find a cube which shall be double of a given one. In fact it was required to find a cube root of 2 by geometry. Modern researches have shown that this, along with the two other celebrated problems of the trisection of an angle and the quadrature of a circle, cannot be solved by pure geometry. In arithmetic, the cube of a number is the product obtained by multiplying it three times by itself; thus, $5 \times 5 \times 5 = 125$. Conversely the cube root is such a number as when multiplied 3 times by itself will produce the given number. Thus 5 is the cube root of 125, and 125 the cube of 5. See ROOTS.

Culmination *of a star:*—the moment at which the star passes the meridian, and is therefore highest in its diurnal revolution. The culminating point of a great circle is that where it intersects the meridian.

Cupola. A hollow spherical body in the shape of a reversed cup, which surmounts a circular edifice. It is the same as the dome, only that the former name applies more generally to the inner, and the latter to the outer surface. See DOME.

Currents, Atmospheric. See WINDS.

Currents, Electric. Reserving a more detailed exposition of the subject of currents for the article on ELECTRODYNAMICS, we may give here a brief introduction to the subject, and add a few statements in regard to the laws and the nature of currents, that could not so properly be made in the article referred to. We have the example of an instantaneous current in the discharge of the Leyden Phial or Battery, or other similar arrangement. According to the common form of the experiment of discharge, no effect is observed except at that part of the circuit where the discharging rod is separated from the coating, or more generally where the conducting circuit between the coatings is interrupted, so as to give rise to a spark or other phenomena of disruptive discharge. But we know that the conducting part of the circuit, is also in a peculiar electric state at the instant of discharge; a state essentially distinct both from the natural condition of the body, and from the condition of mere charge or electric tension. In these circumstances, when the opposite electricities are—according to the common modes of statement—moving through the circuit, uniting, and mutually neutralizing each other, the circuit is said to be traversed by an electric current. To derive from this illustration an instance of the more common and important kind of current—that is, the continuous—we have only to suppose that one of the coatings of the Phial is connected with a continual source of electricity, while the other coating is uninsulated, or connected with a source of opposite electricity. In these circumstances the discharge condition will be sustained in the circuit that unites the coatings; new charges being supplied as those formerly evolved are neutralized: in other terms, the circuit will be traversed by a continuous current. The discharge of the Leyden Phial has been employed in illustration, for the purpose of connecting in a simple way the subject of currents with that of statical electricity. But the most common type of the current in connection with its source is the Galvanic Battery, or the simple Galvanic Circle. Two plates, one of zinc the other of copper, are partially immersed near each other in dilute sulphuric acid; and their unsubmerged parts are connected by a wire. It is found that the wire is traversed by a current, as if the copper plate were a source of positive electricity, and the zinc of negative. It is important to remember that the direction which is conventionally ascribed by scientific men to the current, is in the above instance from the copper to the zinc, through the wire. In general, what is called the direction of the current is the direction in which the positive electricity appears to move in the circuit. Having obtained a simple type of the current, we may consider some of its effects, confining ourselves to those by which the current itself is most clearly manifested, and by the observation of which it is usually detected and measured. One of the earliest known actions of electricity in motion is the calorific, of which we have an instance in the incandescence of a fine wire by the discharge of a Leyden Battery through it. Similar effects, rising in intensity to the fusion and brilliant

combustion of metallic circuits, are produced by continuous currents of sufficient power. In general, the temperature of a circuit is raised by the passage of electricity through it. This thermic influence upon the circuit has been employed in several ways for the measurement of the current. In Delarive's *Rheometer*, or current-measurer, for example, which was the first invented instrument of this kind, the current is detected, and its power approximately determined, by the temporary extension of a fine metallic wire forming part of the circuit; the extension being due to a rise of temperature. In another and more sensible instrument, the fine wire traverses the bulb of an air-thermoscope, and the effect observed is the expansion of the air by the heat communicated to it from the wire. Instruments of this kind were to a great extent useless for the purpose of measurement until those laws had been determined, by which the intensity of the current is connected with the quantity of heat evolved by it in the circuit. And though we now have this knowledge through the recent discoveries of Mr. Joule, the instruments referred to are of little or no use, compared with those to be afterwards mentioned. Another class of effects of electricity in motion may be generally termed the electro-magnetic. A circuit that is traversed by a current is found to exert certain magneto-inductive and mechanical actions upon magnets and magnetic bodies in its neighbourhood. The laws of these actions, and more especially of the mechanical, have been determined with great accuracy; and they have given to physical science one of its most valuable instruments, the Galvanic Multiplier. See GALVANOMETER. A third class of effects of the current are the chemical. When certain liquids form part of the circuit, they are chemically decomposed, and the elements are set free, in the simplest cases, at those parts of the liquid where the current enters and leaves it. By the labours of Becquerel and others, and especially by the beautiful researches of Faraday, the laws of this class of actions have been very fully investigated, and they have supplied a rheometric instrument in his Volta-Electrometer, that leaves nothing to be desired in regard to the measurement of currents which have a certain intensity. These brief remarks on the effects of electricity in motion are sufficient for our present purpose; which is to give some indications, however slight, as to the several ways in which the existence of currents is usually detected and their strength estimated. Having considered some of the effects, we may now look with equal brevity to the sources. A current may be obtained in general by the discharge of statical electricity by whatever means excited. But the more important kinds of current are derived from sources which evolve statical electricity of excessively feeble tension. In the present state of science, the most valuable source and that most commonly employed in experiment is chemical action, exemplified in the common forms of the Galvanic Battery. Heat is also an important source when peculiarly distributed in heterogeneous metallic circuits, as will be shown under THERMO-ELECTRICITY. A third source of currents is found in the magnetic forces, combined with certain movements of the conducting circuit in the field of force. The evolution of electricity in this way is well exemplified in Faraday's magneto-electric machine. The sources and effects of electricity will fall under our special consideration in other places, so that we need not dwell upon them further at present. An essential character of the current is the constancy of its powers at all parts of the circuit. Thus, it has been long known that all equal lengths of the circuit, when acting upon similar magnets in similar positions and at the same distances, produce equal deviations. A similar law holds in regard to electro-chemical decompositions. If several processes of decomposition go on at different parts of the circuit, the power expended is the same for each; so that, if the compounds be the same, the quantities of the elements evolved in the several processes are equal. The truth of the general law has appeared to be more than questionable in regard to the calorific power of the current, as we shall see afterwards; but the law as it stands is highly important. The laws of the Intensity of Currents have been investigated with much diligence and success. Sir H. Davy was the first to attempt the investigation; and besides obtaining other important results, he laid down these two laws, that the intensity of a galvanic current varies inversely as the length of the circuit, and directly as its section, supposing the source to be of constant energy. These laws have been extended, since his time, to liquid circuits, and to Thermo-electric currents. Pouillet discovered that the first law, as stated by Davy, required an experimental correction; and the correction when made, gave this beautiful result, that the Pile itself affects the Intensity of the current, not merely by its action as a source, but also by an action as part of the entire circuit in transmitting the electricity, and that the latter action is introduced into Davy's law by attributing to the Pile a constant effective length for all conditions of the rest of the circuit. The importance of the laws of intensity may be inferred from this consideration, that they indicate some of the conditions which are essential to the development of currents, and to their development with a greater or less power. Closely connected with this subject is that of Conductibility. The specific conducting powers of the metals have been very diligently investigated by several philosophers, and according to various methods. Passing by the older methods of Davy, Pouillet, and others, we may mention one suggested by Faraday.

When conductors move in a certain way in the neighbourhood of magnets, they are traversed by electric currents, and it appears that the intensity of the currents varies directly as the conducting power of the moving body, if other things are equal. By employing therefore a constant magnetic arrangement, and causing different conductors to move similarly in the field of force, we would only have to measure the currents in order to obtain the specific conducting powers. This method has confirmed the results previously obtained by laborious and unsatisfactory experiment. Copper, gold, and silver are the best conductors, being as 633, 600, and 466. Lead is only as 52, mercury between 10 and 20. Pouillet has endeavoured to estimate the conducting powers of liquids, and to compare them with those of the metals. For the conducting power of the saturated solution of sulphate of copper, the most powerful liquid that he examined, he found a value only a twelve millionth part of that of gold. We obtain from this example a good idea of the width of range taken by bodies, in regard to conductibility: for the liquid here compared with gold, is an excellent conductor compared with other bodies usually consigned to the class of conductors.

Currents, Oceanic. See OCEAN and TIDES.

Curvature. Literally the amount or degree of the *bending* of any mathematical curve. The general principle of the treatment of all questions of curvature is easily explained. The direction of a straight line is determined when two points in the line are determined. Through any two contiguous or *elementary* points in any curve, a straight line, and only one straight line, can be drawn. That straight line is the *tangent;* or the straight line which of all others, most nearly coincides with the curve at the point or points through which it is drawn. The position and magnitude of a *circle* again is determined by the position of any *three* points in its circumference: through *three contiguous points* in any curve, therefore, a *circle* may be supposed to be described. And as the *tangent* indicates the direction, this circle—named the osculating or Equicurve Circle—indicates or measures the bending, or curvature of the curve at the said point. The finest mathematical theory of curvature yet given is that of Lagrange, and it rests on the foregoing view. The curvature of any Line is *technically*, *single*, or *double.* If the line is in one plane, its curvature is single; if on the other hand it is a twisting line in space, it is termed a curve of *double curvature.* The theory of all such curves is complete; although it cannot be said not to be cumbrous. See QUATERNION.

Cyanometer. An instrument invented by Saussure for measuring the intensity of the blue of the sky. Imagine a band of rectangles, of which the first is of the deepest cobalt blue, whilst the last is almost white; the intermediate rectangles presenting all imaginable shades between these extremes. By comparing this scale with the sky, it is easy to detect with which rectangle the shade of the sky corresponds; and the number attached to that rectangle will indicate the degree of blue. By an ingenious process, Saussure obtained *fifty-one* shades regularly graduated, between white and black: so that the scale of this Cyanometer comprises fifty-three degrees, corresponding to combinations of white and deep blue, in definite proportions. The instrument, simple as it is, fully answers its purposes.

Cycle. A name applied to an interval of time, after the passing of which certain phenomena, chiefly astronomical, recur in the same order and with the same circumstances as during its continuance. No phenomenon in the universe will in all likelihood ever again recur identically. All is in unceasing mutation—no star that has once been in any place in the infinite space will, for ever, return to it, or any cloud sky that has once shaded the earth will ever renew itself. But this does not prevent the succession of circumstances nearly similar, in cycles of nearly constant length. Thus though the length of the year changes by infinitesimal amounts, we can take a certain approximation to it, as quite near enough for all purposes, and we can be sure that after it passes, the sun will pass through a series of positions, which to us appear the same or practically so, as during the cycle of the year. It would be impossible in this article to name and give an account of all the astronomical cycles within which phenomena recur. The year and the month, as the most important of them, will be noticed elsewhere. The word *cycle* is applied in a somewhat narrower meaning than that which we have explained, and which properly belongs to it. The year and the month depend on the recurrence of one astronomical phenomena, which again does not depend on the recurrence of several others. There are special phenomena such as eclipses, whose recurrence is the result of peculiar conjunctions of phenomena, and the name cycle is properly applied where these phenomena are themselves recurring in the interval from their commencement together, to their recommencement. We shall explain some of them here in order. And first—the methods by which the commencement of the year and the day are adjusted. By far the most important adjustment which we have in this regard are found detailed in the articles BISSEXTILE and CALENDAR. The first we shall specify here is what is called the *solar cycle*, or cycle of Sundays. This is chiefly of chronological interest. Were our year composed of an exact number of weeks, as 52 weeks, we should have a constant recurrence of days in the same order, and if Sunday were the first day of the year, it would be the first day of the year for ever after. But the year is made up of 52 weeks and a day. Hence

unday (take any other day of the week as well) oes backward a day every year. The first of his year is on a Sunday, suppose—then that of ext year will be Monday, of next—Tuesday, nd so on. In this way we should come back to unday after seven years, and go through the ame series of changes. But the calculation is gain disturbed. Suppose the first year to be .g. 1854. Then next year (1855) will com- nence on a Monday, this beginning, suppose, on unday. Next year (1856) commences on uesday, but it contains 52 weeks and 2 days. Hence the next year will commence not on Vednesday, but on Thursday. This upsets herefore the cycle of seven years which we had nagined. We come back to a year commencing ith Sunday—1860, but we do not go through ne days, taking all but Wednesday as com- iencements of the year. The year 1860 has lso two odd days, and the next, 1861, com- iences on Tuesday. We will go on. So then— Vednesday, Thursday, Friday—then (omitting ie fourth day, Saturday) Sunday, with still change in the order. Monday, Tuesday, Vednesday, omit Thursday, Friday, Saturday, unday, Monday, omit Tuesday, Wednesday, hursday, Friday, Saturday, omit Sunday, Ionday, Tuesday, Wednesday, Thursday, omit riday, and so on. Each of these cycles of ven years would be complete but for this nission of every fourth day in the succession commencing days. All these omissions go in ie one direction—the pushing forward of the mmencing day by one additional day. There one of them every four years. In seven of ese four-year periods there will be seven of em, and the year will therefore be seven days one week pushed forward as to its commenc- g day. Here therefore at the end of this ne we will get back to the same day of the eek for the commencing day, with the same ries of omissions to succeed it for another period seven of those four-year periods. In 28 years erefore there will be a constant recurrence of e same series of days of commencement of the ar. Thus for 1854, 1855, 1856, 1857, the ys commencing are Sunday, Monday, Tues- y, Thursday, and so on for the succeeding ars. The same series begins again at 1882, d will go on for 28 years exactly the same. is period of 28 years is called the *Solar Cycle.* r the *old style*, in which the Julian year alone s employed, the method was perfect. But cording to the Gregorian correction—(see SSEXTILE)—the years 1700, 1800, 1900, are t leap years, though divisible by 4, nor any mber of hundreds, which is not itself also visible by 4. Hence at 1900 the cycle is terrupted. The year 1900 should have its mmencing day two days behind that of next ar. It has Monday. The year 1901 should ve Wednesday, in order that the cycle might it. But the year 1900 is not, though the end of one of the periods of four, one which has two odd days, but only one. Hence Tuesday will be the commencing day of 1901, and so it goes on. But as Wednesday was the commenc- ing day of 1883 (1901—28) the comparison between the 28 years just then passed and the next 28 would be all at fault, and would, *if* followed, put us a day too far forward in our reckoning until the next date of disturbance. The series of days will require then to be changed for 1901 and the succeeding years, and will go on so changed until the next interrup- tion. Usually these interruptions will recur every 100th year, but 2000 is a leap year, and the cycle of days marked in 1900 will go on uninterrupted for 200 years in consequence, requiring new change at 2100, then requiring still another change at 2200, and so on. It will be useful to give for this century the days of commencement:—1800, Tuesday; 1801, Thurs- day; 1802, Friday; 1803, Saturday; 1804, Sunday; 1805, Tuesday; 1806, Wednesday; 1807, Thursday; 1808, Friday; 1809, Tues- day; 1810, Monday; 1811, Tuesday; 1812, Wednesday; 1813, Friday; 1814, Saturday; 1815, Sunday; 1816, Monday; 1817, Wednes- day; 1818, Thursday; 1819, Friday; 1820, Saturday; 1821, Monday; 1822, Tuesday; 1823, Wednesday; 1824, Thursday; 1825, Saturday; 1826, Sunday; 1827, Monday.—If to each of these numbers we add 28, or 56, or 84, we will get the series of years for which the commencing day is the same, and also the commencement of a like cycle. Thus $1806 + 28 = 1834$, has Wednesday for its first day (as 1806 has). So also $1806 + 56 = 1862$, and so also $1806 + 84 = 1890$. When we add still higher multiples of 28, *i.e.* 112, 140, &c., we get $1806 + 112 = 1918$, and here we would be led a day too far for- ward. Tuesday therefore is the day for this, and so Tuesday is the commencing day of 1918 + any multiple of 28, as long as this does not reach 2100.—*Cycle of Indiction.* A period, (quite arbi- trary,) of 15 years, originated by the issuing of a tribute edict by the Roman emperors every fifteen years. It is of frequent occurrence in old history, chiefly ecclesiastical. The position of any year in the cycle of indiction may be found by adding 3 and dividing by 15. Thus $\frac{1855 + 3}{15} = 123\frac{13}{15}$, and therefore this is the 13th year of such a cycle. If we take 312 from the year given and divide by 15, thus $\frac{1855 - 312}{15} = \frac{1543}{15} = 102\frac{13}{15}$, the remainder 13 indicates the year of such a cycle as before, and the quotient 102 gives the number of the cycles past. We are therefore living in the 13th year of the 103d cycle of indiction. The year 313 is taken as the first year of the first cycle— the origin of this chronological period.—There is

a period or cycle—called variously the Sothiac period—the Canicular year—the Annus Magnus or Annus Vagus, which merely requires notice. It was used among the Egyptians. Their original practice was to have 12 months of 30 days each, intercalating 5 days, as a full equivalent for the year. They very soon found the length of the year to be 365¼ days. But the priests attached mystic importance to the dates of their recurring festivals, fixed according to the old method, and would not introduce the fourth day. Hence, while the people adopted a year measured by the recurrence of what is called the heliacal rising of Sirius, the priests instituted the Sothiac period of 365 × 4 years, within which it is evident that the error of ¼ of a day, constantly accumulating, would amount to a complete year of 365 days, which would then be considered as non-existent, so that, thus the 1461st year commenced with an agreement between the sacred and popular year. It is probable that the institution did not last over one complete cycle. Had it done so, it could not have failed to be noticed that 1460 years would not serve for such a period, because ¼ of a day is not the amount of annual error, but 1508 years rather.—The next periods of importance are connected with the adjustment of the moon's motion with that of the sun, and in consequence—as on their relative positions eclipses depend, also with the calculation and prediction of eclipses.—The first is the Metonic cycle of 235 lunations, introduced by Meton, who lived in Athens about 432 B.C. A lunation is the period from full moon to full moon, or from new moon to new moon. The period of 235 lunations of 29·53059 days each gives 6939·69 days. Now 19 years of 365¼ days each give 6939·75 days. Hence the recurrence of new moons, at the same parts of a cycle of 19 years, if the Julian method of reckoning the year were adopted, would be pretty accurate. Each cycle then would indeed be a falling back of ·06 of a day in the occurrence of new moon, but this would not amount to a whole day until above 300 years. Hence, tabulating as in the solar cycle the dates of new and full moons for 19 years, we should have the same dates on which they would recur during each year removed from the former by any multiple of 19 years. Thus given the dates of all the new and full moons in 1854, we would have the same dates for 1873, 1892, &c. There would be slight disturbance, however, always to be allowed for, caused in this arrangement, by the fact that there is not a year of 365¼ days, but three years of 365 and four of 366. This might produce an error of nearly a day either way. Still the cycle was very useful. The Gregorian reformation of the calendar has yet further, however, deranged it, so much so indeed as to render new tables necessary, as in the solar cycle, for every time that the interruption comes. —The Metonic cycle of 235 lunations contains also 255·021 *nodical* months, that is, months which are measured by the interval between the moon's orbital intersections of the ecliptic. At the recommencement of a Metonic cycle, therefore, the moon is very near the same position relative to her node, as she was at the beginning of it. In ·021 of a nodical month she will be accurately at the same point. Now, eclipses, both of sun and moon, depend, as we shall see (ECLIPSES) upon the new and full moon, and on the position of the moon in her node. When the new moon is at her node there is a solar eclipse; when the full moon is so a lunar one. Hence in the successive Metonic cycles the eclipses will run at the interval of ·021 of a nodical month from identical dates; that is, however, ·021 × 27·212 days, rather more than half a day. Hence the Metonic cycle will not serve very well as a cycle of eclipses, but will very well for marking the corrections pointed out to suit the Julian and Gregorian Calendars, as a cycle of lunations.—This cycle of 19 years, or 235 lunations, is used for finding Easter, being there called the cycle of the *Golden Number*. It is also called the *lunar cycle*. This latter term one readily understands; the former originates in the sanctity attributed to the church festivals, the dates of which the ancient church constantly fixed by periods of new moon. The methods of calculating Easter Day will be found in chronological treatises.—The next cycle which was constructed as an improvement upon the Metonic, was the *Calippic*, due to Calippus, a disciple of Plato, who flourished about 330 B.C. He found the errors of the Metonic cycle very manifest in observing an eclipse near the time of the death of Alexander the Great. He merely made a cycle of four times as much length as that of Meton, all but 1 day. His cycle contained 76 years all but 1 day therefore, and it contained still more near approximation than Meton's to an exact coincidence of lunations and nodes. There were in it very nearly 940 lunations, 1020 nodical months, and 1016 sidereal months. It had the faults of the Metonic cycle, less exaggerated than his. He commenced calculating his cycle from the new moon immediately following the summer solstice of 330 B.C. Meton's cycle commenced on the 15th July of the year 432.—The best period for the calculation of eclipses which has come to be very well known is the "Saros," a period of Chaldean origin. This period consists of 223 lunations. In that period there are 241·029 sidereal months, so that the moon at the end of it is very nearly in the same position in the sky as she was at the commencement. There are also 238·992 anomalistic months, so that she is very nearly in the same position in her own orbit also. There are besides 241·999 nodical months, so that she is very nearly in the same position regarding the ecliptic, having gone forward by ·001 of a nodical month,

of about ·027 of a day. Now the occurrence of eclipses depends upon the position of the moon near her nodes, and her conjunction with or opposition to the sun. Here then, supposing her to be in opposition or conjunction, and at her node, at the commencement of a saros, she will be in conjunction or opposition again and very near her node (having passed it about 10 minutes before), at the end of the saros. Hence in this period all eclipses will very nearly occur. Chronologically, however, this is not equal to the Metonic or Calippic periods as not giving the dates of eclipses at once; but it is far more accurate, as giving the time of their occurrence. The 223 lunations make up 6585·32128 days or 18 years of 365 days each, 15 days, 1 hours, 40 minutes, 38 seconds. It has no definite period of commencement, so that if we choose to commence it in one year we may have five, or in another four leap years in the 18. In the first case the saros consists of 18 years, 10 days, 7 hours, 40 minutes, 38 seconds, and in the second the same with 1 day added to it. The original Chaldean period is $6585\frac{1}{3}$ days, which is wonderfully accurate, being only in error by 19 minutes 22 seconds, and they were wont to treble the period in order to get at the exact number of days. This method secured nearly the ending of each saros in the same part of the day as the beginning of it, a point of great importance, as far as its use in predicting solar eclipses goes. Like all such periods, which make one recurring phenomenon coincide with another, the *saros* does not completely answer its end. The little differences of nearly 40 minutes in each Saros accumulates, and carries the moon out of the position where eclipses may occur—putting partial eclipses for total, and ultimately destroying the partial ones. The saros cannot well be used, therefore, as a means of discovering eclipses far removed from our times. In the saros there are generally about 70 eclipses, of which 29 are lunar, and 41 are solar, visible in some part of the earth. In the course of a year the number of eclipses so visible may be as many as seven and as few as two. See Article ECLIPSES—The *Paschal Cycle* is an ecclesiastical one, exactly like the solar in principle. There the even disturbance of one day per week is not permitted by the introduction of leap year. Here the regular recurrence of Easter, on the same day of the year after 19 years, cannot take place on the same day of the week, because the commencement of the year does not. The two cycles of 19 and 28 years do not coincide, just as the two cycles of 7 and 4 years. The same principle which procured their coincidence in a larger cycle, by multiplying them together, does so here also. Hence 19 × 28, or 532 is the period within which the recurrence of Easter on the same day of the year—for all the successive Easter days of that period takes place.—The *Julian Cycle* is a contrivance on the very same principle, introduced by Joseph Scaliger, to make the cycle of indiction and the *solar* and *lunar* cycles, coincide. It is merely 28 × 19 × 15 years, or 7980 years. Its commencement was fixed at 4713 B.C. Hence by subtracting the number of any year B.C. from 4714, or adding that of any year yet reached A.D. to 4713, we will get the year of the Julian period. By dividing this by 28 we get the year of a standard *solar* cycle, in the remainder by 15 that of *indiction*, and by 19 that of a standard *lunar* cycle.

Cycloid. A curve thus described. Take a circle and mark on it one point. Roll this circle along a straight line in any plane, and observe the various points successively covered by the marked one on the circle. The curve so traced will be a *cycloid*. Galileo was the first to remark it in 1615. Its equations are $x = a\,(\omega - \sin.\,\omega)$ and $y = a$ versin, ω, where a is the radius and ω the angle between the radius to the point, (x, y,) and the radius to the point where the generating circle touches the constant straight line. They may also be put in these two other forms:—

$$\frac{y}{a} = \text{versin}\,\frac{x + \sqrt{(2\,ay - y^2)}}{a}$$

$$\frac{dy}{dx} = \text{cot.}\,\frac{1}{2}\,\omega.$$

Cylinder. A solid terminated by three surfaces, one of which is convex and continuous—being in the cylinder proper, circular—the other two parallel. A *right* cylinder is one in which the line joining the centres of the limiting circles is perpendicular to their plane. Every other cylinder is oblique. A right cylinder, in the confined sense we have indicated, may be conceived as described by a rectangle revolving round one of its sides as in the figure, and an oblique one may be formed by cutting a right cylinder slant across. The more philosophical definition of a cylinder as including all possible ones, would be that an infinite number of infinitely thin planes, bounded by curves of the same size and character, returning into themselves, are laid one above the other, the cylinder being the complete outline of the solid so formed. The line between the centres is the axis of the cylinder, and the two parallel sides are the bases. The height is the perpendicular distance between the bases. The content of the cylinder is the product of the height by the area of either base. The convex surface is equal to the product of the circumference of the base by the length. Similar cylinders are those with similar bases, having their axes inclined similarly, and their proportion to the linear dimensions of the bases the same. The oblique cylinder with elliptic ends is sometimes called a *cylindroid*.

Daguerreotype. The beautiful department of photographic art which is so designated, derives its name from its inventor, Daguerre, who, along with another indefatigable French experimentalist, Niepce, after long continued researches, succeeded in perfecting its different processes, and published them to the world in July, 1839. A short *rationale* of the art will first be given, and afterwards a description of the different steps necessary for the production of a Daguerrean picture. A surface of pure silver is caused to combine with iodine, and in the more recently improved methods also with bromine, whereby a film of ioduret and bromuret of silver is produced which is exquisitely sensitive to light, in such a way that if exposed even for a single second to a feeble daylight an incipient change is produced in it, which though not obvious by mere inspection, yet becomes evident by the facility which it has acquired of condensing vapours, particularly that of metallic mercury, on its surface. If the plate be long exposed to light, a change on the surface becomes apparent without any development by mercurial vapours, though this requires a much longer time, at least 1000 times greater than that which is necessary to determine the first affinity for mercury. If, instead of the plate having been exposed to diffused light, it be put in the field of the camera obscura, the image impresses the magical change on the different parts of the surface, to an extent proportionate to the intensity of the light (to speak simply), and thus a latent image of the picture is produced, which is afterwards brought out by exposure to mercurial vapour, which, by adhering to the parts acted on by the light, gives them a white appearance, while the parts on which the light has not acted remain of the original dark aspect of the polished silver. The sensitive bromo-ioduret of silver is now removed from the plate by a weak solution of hyposulphite of soda in water, after which it is washed, and is no longer sensitive to light. It is then protected and strengthened in its appearance by boiling on its surface a solution of chloride of gold. Afterwards it is carefully covered by glass, and protected from the contact of air and damp. Even with all precautions, it is to be regretted that, owing to the extremely oxidizable nature of mercury and silver, the brilliant aspect of these beautiful phantoms after a time begins to fade; and, except in the case of their being hermetically sealed in cases of glass, there is reason to believe that in comparatively few years most of them will perish.—As to the practical operations of the Daguerreotypist, space compels us to be brief. The silverized tablets of copper are to be had of the shops. They must be again polished immediately before being exposed to the iodine. Olive oil and finely powdered pumice are first rubbed on by circular strokes of a dossil of fine cotton wool. The wool is again and again renewed, till the whole of the oil and pumice are removed, after which the surface is wetted by a piece of cotton dipped in one part of nitric acid to fourteen of water, which is afterwards wiped off by clean cotton, and fine pumice reapplied. It may then be finished by circular movements or strokes, as they are called, on a piece of velvet, till a fine black polish comes over the whole surface. It is now placed in the iodine box, of which a representation is annexed, into which a small quantity of iodine has been

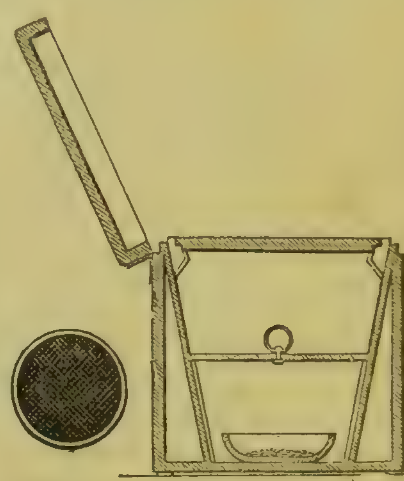


Fig. 1.

put, and is allowed to remain till the surface is of a pale yellow colour when examined in a feeble light. It may now be placed in the camera, but it can previously be rendered much more sensitive by exposure to the vapour of bromine, rising from a small quantity of bromide of lime placed in a box similar to the iodine box, till it is of a rose colour; after which it is to be exposed in the camera for a period varying from a fraction of a second to a few minutes, the time depending only on the sensitiveness of the surface and the strength of the light. This, experience only can teach. The image is now to be developed, by placing the plate in the mercurial box, at a height of a few inches from a small metallic pan of mercury, in which is placed a thermometer, the plate being inclined at an angle of 45° to the rising vapour. A spirit lamp is placed under the pan, and the temperature raised so as not to exceed 167° Fahr. The development of the image is to be watched by the light of a candle held near the small window in the side of the mercurial box, while the eye is placed at the other. The plate is then to be removed from the box, and while still in the dark room it is to be placed for a moment under water, and then

ved to and fro in a solution of hyposulphite oda, till the yellow colour is removed, and then shed in a gentle stream of water, the water

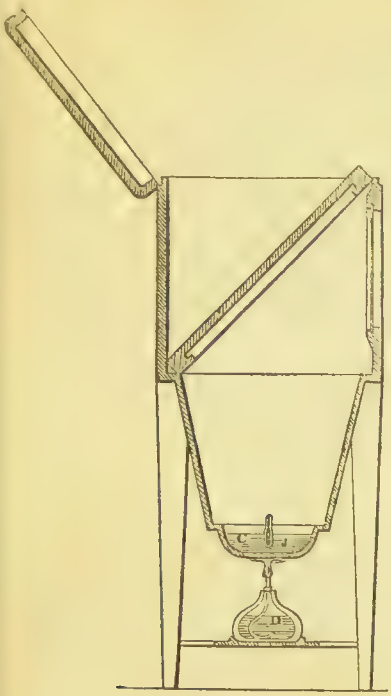


Fig. 2.

g afterwards blown from the surface if any s adhere, lest in drying they should leave ks. The plate is now to be held by a small of pincers, and covered by a solution of gold, e by dissolving four grains of chloride gold ght ounces of water, and sixteen grains of osulphite of soda in two ounces of water. the two solutions, gradually shaking them ther. This liquid keeps well, and, as above ted, is poured in sufficient quantity to cover plate, which is then held over a spirit lamp till ng is produced, and a considerable portion is orated, after which the remainder is to be wn off, and the plate quickly washed and dried. cannot be denied that though the Daguerreo- as far as sharpness and delicacy of detail is erned, is the most perfect of all the photo- hic processes, yet it is the most difficult of ipulation. So much is this the case, and so ult is it to get the plates sufficiently clear free from scratches, that it is scarcely a pro- for an amateur. The metallic glare of the ures and the livid look which they frequently me are also objections from which the re- of the arts described under the heads CALO- and COLLODION are free, and, as already d at, there is reason to believe that the ures so produced are less liable to decay. y can also be more easily obtained of a larger and can be manipulated in the fields for the production of out-door views far more easily than the plates of the Daguerreotype process. It ought perhaps to be stated, as an important point in manipulation, and also as curious in itself, that a momentary exposure of a plate to bromine vapour which has, after being made sensitive, been exposed in the camera, completely effaces the impression, so that no image would be developed were it submitted to mercurial vapour; and that the same action of the bromine renders the surface again sensitive. So that it is unnecessary to clean the plate, if before developing we suspect the picture to be imperfect. All that is necessary is to submit it for a little to bromine, and slip it into the slide of the camera again to be exposed.—It is also worthy of notice that it is in connection with the Daguerreotype process that the most successful attempts have been made to procure, directly by photography, pictures in their natural colours, an achievement which, could it be accomplished, would leave little to be desired in this beautiful art, which has already done so much for the instruction and delight of mankind and for the glory of its illustrious discoverer.

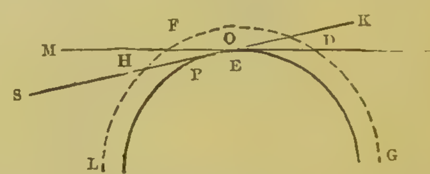
Daltonism. A name given to that imperfect sensation or appreciation of colours with which many persons are afflicted: it is derived from the name of the great chemist Dalton, who had the infirmity in excess. The imperfection is a singular one: the form of objects is readily discerned and judged of; nay, the eye is sensible to the smallest amount of light: but the sensitive or perceptive apparatus is apathetic to certain tints; and either the former does not transmit notice of them to the brain, or the latter cannot judge of the transmitted intimation. Seebeck closely investigated the infirmity. He divided Daltonians into two classes,—the *first* composed of persons who are deceived rather with regard to shades of colour, than as to different colours; who confound, for instance, clear orange with pure yellow, lilac with bluish-gray, &c., &c.: the *second*, of those who really do not know many distinct colours, having for the most part an extremely feeble appreciation of the less refrangible rays of the spectrum, or of the rays near the red end. Two general conclusions are drawn by Seebeck from a large series of observations. 1. Daltonism never affects the yellow rays: every eye, however imperfect otherwise, distinguishes yellow. 2. The sensations of complementary colours, are inseparable; so that the eye is sensible or insensible to both at once: for instance the eye that perceives *blue*, will also perceive *orange;* the eye that cannot distinguish *red* will not be able to distinguish *green*, &c., &c. There is a far greater number of *Daltonians* than is generally supposed; more among males than among females: the defect is also hereditary. Dr. George Wilson of Edinburgh not unwisely asks whether it may not often blind railway guards to the character of those *coloured*

signals which it so much behoves that they interpret aright? By a very simple means Daltonians may be made aware of their error, viz., by the use of coloured glasses. If, for instance, one's eye confounds green with red, it is enough to use a red glass, and the difference of the colours will, to his great astonishment, be perceived in an instant. See memoirs by Seebeck, Wartmann, Szokalski, and Dr. George Wilson.

Dalton's Law:—that two gases placed beside each other in a given space, expand towards each other, ultimately, as toward vacuum. Thus, for example, if carbonic acid and common air be put into one vessel, the carbonic acid will spread after a time through the whole vessel as though air were not present at all; and the air will, in its turn, expand as if there were no carbonic acid. The pressure upon any part of the vessel will be the sum of the pressures of the expanded portions. Thus, if a of the vessel be filled at first with the acid, and b with the air, the volume of each will become a + b, and according to Marriotte's law, the pressure of the first will be $\frac{a}{a+b}$ of what it was, and of the second $\frac{b}{a+b}$ of what it was. Suppose p and p′ the two pressures on each square inch, then the ultimate pressure will be $\frac{a\,p + b\,p'}{a+b}$. If the two gases have originally the same pressure p, $\frac{a\,p + b\,p'}{a+b}$ will become equal to p. That is, the pressure on the sides of a vessel, acted on by a given internal pressure, is not altered by the mixture within it of two gases, originally of the same elasticity.—Dalton's law does not require that the mixture of gases, and the completion of the process, should be instantaneous. In fact, it never is. If, for example, carbonic acid at atmospheric pressure be enclosed in a vessel with a plaster of Paris lid, put over water, and a small hole be bored in the lid, the water will rise until it nearly fills the vessel. If the operation of Dalton's law were instantaneous, there could be no such rising, the carbonic acid would immediately disappear almost entirely into the air, and the air would take its place; but the pressure would be constant, and equal to that of the atmosphere upon the outside water. In point of fact, some hours would elapse before the level had sunk to its original height. Most gases, indeed, expanding towards vacuum, do so with violent accompanying explosions. Gases mixing according to this law, conduct themselves with perfect quietude.

Dasymeter. An instrument invented in 1780 by M. Fouchy, with the view of ascertaining the density of different descriptions of gas. It is merely a globe of very thin glass, capable therefore of nearly floating in air, and which is weighed against ordinary weights in different gases. The principle of Archimedes (Balance—Hydrostatic) will immediately show the reader how the instrument should be used. The glass ball displaces its own volume of the gas; and the weight of that amount is deducible from comparison between the ball weighed in a gas, and *in vacuo* or an atmosphere of known density. We thus come to know the density of the gas.

Dawn. During a considerable time before the morning Sun is visible, there is not complete darkness in the sky. The luminary appears at the very moment when his *direct* rays reach the eye; and this partial illumination must be due to some other influence; principally to the reflective power of the atmosphere. The sunlight enters the atmosphere before it reaches us directly. The



spectator at E will not see the sun until he rises above the horizontal line M N, but when he is at S his lowest ray will be along S P K. As the other rays will all pass on the upper side of S K, unintercepted by the earth, if we conceive the ring L F G to be the outer limit of the atmosphere, the whole atmospherical space, except that portion from K P round to the similar line on the other side, will be filled with light. Hence light will freely pass through the atmosphere F O K, which is within the range of vision of the spectator at E. If this atmosphere allowed light to pass freely through it, these rays would not reach us: but it possesses the property of a mirror, reflecting part of these rays back; and so it gives us partial light. This light is called the dawn. It has commenced whenever the lowest ray has passed into the space F D; and terminates as soon as the sun has risen to the line F D or its production.

Day. Applied, in ordinary language, to the period of light from its appearance to its disappearance. This period (see Dawn and Twilight) is somewhat indefinite, and its length varies in different seasons of the year. In this sense the word could never become a scientific term. The scientific day includes the night-time, and consists of 24 hours. The old name, which was synonymous with it, is *nycthemeron.* According to this use, we have what is called the *true solar day*, the mean *solar day*, and the *sidereal day.* The true solar day is the interval which elapses, between two successive transits of the sun over the same meridian. But this interval is not uniform throughout the year. Supposing a watch adjusted to keep time with the sun, it would require to be moved forward or backward almost every week. A correct watch goes regularly,—but the sun is irregular in his move-

nt. Hence the idea of a *mean solar day.* ...s is precisely what its name imports. The ...ble of the days for a year are added together; ... the interval between the first and the last ...them divided by their number. The result ...he *mean solar day.* The correction by which ... true solar time is brought to mean solar ...e is the *Equation of time.* See EQUATION. This correction is sufficient for most physical ...poses. The astronomical or *sidereal* day is ...ained otherwise. This sidereal day is mea...ed by the rotation of the earth. Our globe ...ves round an axis; and so makes all objects ...hout and beyond it, to change their apparent ...itions, and gradually to return to them. But ... rotation of the earth is always accom...hed in exactly the same time; or if it vary ...all, it has been proved by Laplace, to vary so ...le as to be altogether insensible during the ...ble duration of our present era. The period ...this diurnal motion may be taken, therefore, ...an absolute and invariable measure of time; ... that period, or the *sidereal* day, is the ...rval between two successive transits or cul...nations of a *fixed star.* Now, as the apparent ...ion of the sun is backward among the stars, ...s sidereal day must be shorter than the mean ...ar day; and if the sidereal and solar day ...uld begin together, the one would gain, in ... year, one whole day upon the other.—Taking ... mean solar day as the standard, the length of ...s sidereal day is 23 hours, 56 minutes, 4·092 ...onds. This period is divided into 24, giving ...ereal hours; then, subdivided into 60, and that ...in into 60, giving sidereal minutes and ...onds. They will all be shorter than the ...an solar hours, minutes, and seconds, in the ...portion of 24 hours to 23h 56′ 4·092″ (as ...400 to 86,164·092). Pendulums are readily ...structed of such a length as to beat sidereal, ...tead of mean solar seconds; and those of ...onomical clocks are so constructed.—The ...an solar day is equivalent, to what is ordi...ily called the civil day. The four times of ... day's commencement which have been in use ...different times are—mid-day (period of tran..., midnight (time of equal division of the ...rval of transit), sunrise, and sunset. The ...bylonians used to commence their day at sun..., and reckoned round again 24 hours to the ...t sunrise.—The Egyptians began their day ...midnight: Hippocrates introduced, and Coper...us confirmed the use of this in astronomy. ...is difficult, however, to see what this prac..., still followed, has to recommend it. There ...no physical event coincident with the com...ncement so made, and in the case of the ...ronomer especially, the observations he makes, ...ng, many of them, in the night, would be so ...orded as occurring on different days. This ...es rise to a considerable amount of trouble. ... observatories, the day is calculated as 24 ...urs, and not as two periods of 12 hours each. Thus, the hour before its commencement is the 23d of the preceding day.—The method of commencing the day at noon is, of course, that of ordinary life.

Decimal Coinage.—Until within the last sixty or eighty years, decimal coinage was little known. Almost all the European states employed systems of coins bearing more or less analogy to our own. The steps from one coin of account to the next, like the majority of those used in the progression of weights and measures, were made by numbers apparently chosen for their great divisibility, more especially by binary factors. The favourite numbers were 20, 12, 16, 8, and 4; there was one example of the use of the inconvenient number 7, and another in which the factor 3 was employed. Thus England has had from time immemorial her present money table of account. In France the old money scale was 12 deniers = 1 sou, 20 sous = 1 livre, which, except that the livre was very soon depreciated to an extremely low value, was the precise counterpart of our pounds, shillings, and pence. Piedmont, Sardinia, Liguria, Lombardy, and Tuscany, had the same system, the names of the coins were changed to lira, sol, and denier; and in the case of Tuscany, a coin of 7 lire, called a scudi, was added at the top of the scale. In Belgium, several systems of account existed, bearing a more or less close analogy to the English and French scales. There was the Brabant florin of 20 sous, each sou being 12 deniers, and the livre tournois, which was divided in the same way. Another scale in use in Belgium was 16 deniers = 1 sou, 20 sous = 1 florin. A fourth was 3 mittes = 1 denier, 8 deniers = 1 gros, 12 gros = 1 schilling, 20 schillings = 1 livre de gros. In the Netherlands, 16 pennings made 1 stiver, and 20 stivers 1 guilder. In the systems of some other countries, the prevalence of the same factors was observable in a greater or less degree. In Russia, on the other hand, the decimal, or rather centesimal, scale of 100 copecks = 1 rouble, has long existed; and in Portugal, accounts were kept by simply stating in common arithmetical notation the numbers of an imaginary minute coin, called a rei, contained in the sum to be recorded, the term milrei being used, as its etymology imports, to denote 1,000 reis. In the actual Portuguese coins, the law of the English and French coinages is traceable in a modified shape. The modern movement in favour of decimal coins was commenced by the United States of America immediately after the declaration of independence. Their previous system of account had been identical with that of England, except that the value of the pound had been more or less depreciated in almost all the provinces. The coins in circulation were principally Spanish dollars, and the dollar was on that account selected as the unit of the new coinage. It was divided into 10 dimes, the dime into 10

cents, and the cent into 10 mils; but though all these coins have been put into circulation, dollars and cents alone have survived as instruments of reckoning, dimes having never been used in accounts, and half-cents, quarter-cents, &c., having superseded mils in the reckoning below the cent. The French Revolution gave the next impulse to decimalization. A very symmetrical system, embracing decimal coins, weights and measures, and a decimal reckoning of time, starting from the establishment of the Republic as its epoch, was attempted to be introduced, with the double view of attaining to scientific uniformity and obliterating the landmarks of the past. The coinage as well as the metrical system has proved permanent. The transition was very simple. The franc is identical with the average value of the old livre, which varied somewhat in different provinces. This was divided into ten décimes, each of which was therefore equal to two of the old sous, and the décime was again divided into ten centimes, or hundredth parts of the franc. As happened in the case of the dime of the United States, the décime has been found useless, and accounts are kept simply in francs and centimes. The example of France was followed (in most instances by compulsion, but in some voluntarily) by many surrounding countries, and the franc and centime coinage, or some equivalent system, is now established by law in Belgium, Switzerland, the Netherlands, and a great part of Italy. The influence of the United States has, in like manner, so far modified the quasi-English system of Canada, that the provincial legislature has legalized the dollar and cent reckoning as a method of account.—The extent to which the legal establishment of decimal coins has succeeded in different countries in extirpating the old methods of account was very fully investigated by the Decimal Coinage Commission of 1856. (See Prelim. Report and App., 1857, and Final Report and App., 1859.) The results appear to be, that in the United States, France, and some other countries, the new systems have become firmly established in commercial transactions. Belgium having been compelled to adopt the French notation in 1803, repudiated it after the Peace of Paris, and re-adopted it in 1832; but the change has not yet become radical and complete, and people are said still to be obliged to use tables of the various monies, past and present. Comparatively little impression seems to have been made in any country upon the habits of retail traders. In France, where the experiment has been most successful, shopkeepers speak of sous in preference to centimes. In America, retail prices are more frequently fixed with reference to the old provincial shilling and pence coinages, which have ceased to exist for upwards of sixty years, than in terms of dollars and cents. The same tenacity of habit has been shown by the shopkeepers and poorer classes in other countries, long after decimal accounts have become universal in large transactions.—The project of introducing decimal subdivision into the coinage and general metrical system of this country has been much canvassed from 1815 to the present time (1859). The first official step in this direction was the appointment, in 1816, of a Commission to consider the advisability of establishing a more uniform system of weights and measures. The Commissioners (Sir J. Banks, Sir G. Clerk, Mr. Gilbert, Dr. Wollaston, Dr. Young, and Capt. Kater) reported that a duodecimal scale was far preferable to a decimal one, on account of the facility which it afforded for expressing such quantities as a-third, a-fourth, and a-sixth of a foot, and the like, and for continual halving of measures of quantity and weight. In 1824, a proposal by Sir John Wrottesley to introduce a decimal coinage by reducing the farthing from $\frac{1}{960}$ to $\frac{1}{1000}$ of a pound, and by coining double shillings, was not very favourably received, but the scheme (now known as the pound and mil scheme) has since acquired considerable importance. In 1838, a Commission (comprising Mr. Airy, the Astronomer-Royal, Mr. Bailey, Mr. Drinkwater Bethune, Sir J. Herschel, Sir J. Shaw Lefevre, Sir J. Lubbock, Dr. Peacock, and Mr. Sheepshanks) was appointed to consider the best method of restoring the standards of weight and measure which had been destroyed in the fire at the Houses of Parliament. Their report, in 1841, gave a new impetus to the decimal movement. Their attention was drawn to the subject by the consideration of the best method of subdividing the units of weight and measure, and they recorded a very decided opinion in favour of the establishment of the pound and mil coinage as a preparatory step to a general decimal metrical scale.—A supplementary Commission on the restoration of the standards, composed of many of the old members, with the addition of the Marquis of Northampton, Lord Rosse, Lord Wrottesley, and Professor Miller, reiterated the recommendations of their predecessors. In 1847, the first positive step towards decimals was taken in the House of Commons, by the adoption of Sir John Bowring's proposal to coin a two-shilling piece, which was subsequently acted on by the issue of the florin now in circulation. This was followed by the appointment of a Committee of the House of Commons to consider the practicability and advantage of adopting a decimal system of coinage. The Committee reported, in 1853, decidedly in favour of the pound and mil scheme, but the discussion which followed brought out a number of rival decimal schemes, and exhibited some advantages of the existing system, which had not been brought before the attention of the Committee. In 1855, however, a resolution of the House of Commons was passed in favour of the further extension of the decimal system; but before taking any final step, the Government

ferred the question to a Commission, composed Lord Monteagle, Lord Overstone, and Mr. ubbard. Much additional information was llected by the Commission, and one point which d been previously neglected, was made the sub- ct of especial investigation, viz., the compara- e value of decimals and pounds, shillings, and nce, for the purposes of ordinary retail traffic. e results arrived at may be thus summed up:— *As to account keeping and paper calculation.* at for these purposes a decimal is superior to y non-decimal coinage, inasmuch as it trans- tes all money calculations from compound into nple arithmetic. But it appeared that in some surance offices a system of decimal calculation s employed in conjunction with the present nage; and those who had tried this method ted, that without any appreciable difficulty, y obtained all the advantages of decimal lculation, and that decimal coins would afford m no assistance at all. II. *As to retail ffic and mental calculation.* On this point was established, that decimals are of much s service in mental than in written calcu- ions, and that the divisibility of 12 and 20 2, 3, 4, and 6, and 2, 4, 5, and 10, respec- ely, greatly facilitates the common operations mental arithmetic. Having regard to the nty divisors of the number 10, the Commis- ners came to the conclusion, that for common p reckonings, most of which were worked in head, the present coinage was more manage- le than the pound and mil system would be. I. *As to the convenience of the coins of the two tems,* the advantage was ascertained to be on side of pounds, shillings, and pence. The und and mil notation offered no commercial nimum comparable with the penny, and the nt of sufficient binary factors in the radix dered the adjustment of the intermediate ns impossible without introducing inconvenient ios. IV. *As to the difficulties of transition.* e experience of foreign countries proved the treme difficulty of uprooting old habits. The l (or new farthing) of the proposed decimal le being $\frac{1}{50}$ of a shilling, and our farthing $\frac{1}{48}$, followed that no number of pence, except ltiples of sixpence, could be exactly trans- ed into equivalents in the pound and mil tem. Thus a penny toll would be represented $4\frac{1}{6}$ mils, and could not be paid in the new ns. These and other difficulties of an analo- us kind, arising from the incommensurability he two systems of coins, were considered to der the transition (even if otherwise desirable) extremely hazardous experiment. Other imal schemes were found to be free from e of these inconveniencies, but they were all ected as impracticable on account of the aver- n of men of business to be deprived of the und as a principal coin of account. The report ordingly pronounces against any attempt to roduce the decimal principle into the coinage, and may be regarded as having finally established the intrinsic superiority of the English coinage to any decimal or non-decimal system which has ever been founded or proposed.

Declination (of a Star). The angular distance between any star and the pole, is called the polar distance of the star. The complement of this is the declination, as we term it;—it requires, however, to be corrected, so that it may become the same as we should have, if looking from the centre of the earth. It is almost the same, and the difference is too small to be sensible in the case of the stars. It is not so with the sun and planets. The declination may be defined, also, as that part of a great circle passing through the star, which is intercepted between the star and the celestial equator. Declination and right ascension are the usual data for fixing the place of a star. Circles of declination correspond to what are meridian lines upon the terrestrial globe—circles, namely, described through the pole and the given star. Parallels of declination correspond to parallels of latitude, being smaller circles, parallel to the equator, all the points in which have the same declination.

Declination (of the Magnet) is the angle which the vertical plane through the magnetic axis of a magnetized bar, makes with the plane of the meridian of a place. See MAGNETISM.

Decomposition of Forces. See COMPOSITION.

Decomposition of Matter, is a term employed sometimes to denote the mere mechanical separation of particles; as in a rock which crumbles gradually under the constant action of mechanical forces. Generally, however, the phrase is employed in a different and indeed opposite sense, to signify the resolution of a chemical compound, into its several simple elements. The difference between this and the former kind lies in this, that here the decomposed particles possess properties entirely different from the smallest masses of the original whole; whereas in the former case the decomposed particles are identical in nature with the undecomposed mass.

Decussation. Arrangement of bodies in the form of an X. In optics the focus of a lens, *i.e.,* the point through which the rays collected by the lens all pass is called the *point of decussation.* The phrase is little used.

Deferent. An ancient astronomical term. It means the circle, on which the centre of another moves; while a body is supposed to be passing along this latter itself. A planet moving round the sun as its centre, which centre again has a similar motion in space, may be taken as an example. The sun would here be moving in the deferent. The term belongs to the Ptolemaic hypothesis, and originated in it.

Deflection. A term employed to signify any bending of a body from the position which it would naturally be expected to

occupy. Thus, we talk of the planets being deflected in their orbits, when, instead of obeying the tangential force, they move in elliptic curves. It is used also in regard to the alteration of the true course of ships produced by currents or winds acting upon them. It is further used to signify that bending, or altering of direction, to which the rays of light are subject, which is now called DIFFRACTION (*q.v.*)

Degree: as an angular measure, is the 90th part of a right angle. As a circular measure, it is the arc comprised between two radii which make such an angle between them. Thus, a whole circle is divided into 360 degrees. Undoubtedly the origin of the division is to be found in the approximation to 360 days in which the sun performs his annual circuit;—one degree of the ecliptic being thus, very nearly, the amount of the sun's diurnal motion among the stars. The number 360, thus adopted, is convenient enough, as giving a very large number of sub-multiples, perhaps larger indeed than any similar number. Thus, of the first twelve natural numbers, only 7 and 11 are not sub-multiples of it.—Yet it is one of those cases where the decimal system might be introduced with very great advantage. Wallis, Briggs, Newton, and a great many of the continental philosophers, have suggested and enforced this. The French have actually adopted it. The facility with which decimal subdivisions can be expressed according to our notation is so great, that it is not to be exchanged for almost any other advantage. — The method adopted in France is this—the right angle is divided into 100, and therefore the circle into 400 equal parts. We divide our degrees, next, into 60ths, which we call "minutes," and those again into 60ths, which we call "seconds." They divide their degrees into 100ths, which are minutes, and these into 100ths again, which are seconds. — In reducing, therefore, angles measured in France to English, we must multiply the number of degrees by 9, and divide by 10, $\left(\frac{90}{100}\right)$ or simply subtract one tenth of the number.—We will multiply the number of minutes by 6, and divide by 10, $\left(\frac{60}{100}\right)$ to get the proper fraction of a degree, which would be the value of the specified quantity, if the degrees were the same, and then multiply by 9, dividing by 10, to arrive at the fraction of an English degree, similarly with the seconds.—We require to remember, then, that the English degree is $\frac{10}{9}$ths of the French or centesimal degree—the English minute $\frac{100}{54}$ths, and the English second $\frac{1000}{324}$ of the French minute and second respectively.

Degree of Latitude, is the space, along the meridian, through which an observer must pass in order to alter his latitude by one degree. That is (see LATITUDE), the space along the meridian through which he must pass, in order that he may see the same star, one degree nearer to or farther from, the zenith.—This must be found by actual measurement. Eratosthenes is the earliest of whose measurement we find account. Taking a degree near Alexandria, 250 B.C., he found 694$\frac{4}{9}$ stadia as the probable length (421,350 English feet, or 79$\frac{4}{5}$ English miles). Ptolemy found it 59$\frac{1}{2}$ English miles. Posidonius, in the time of Pompeius Magnus, found 68·95 as the length of the degree. This latter observation is remarkably accurate for the time; but the state of observation was such then, that we can scarce regard it as more than a fortunate guess. It was a matter, however, of great interest scientifically to know the length of the degree, and after the French came to make it their unit of measure, as in their metre (1-10,000,000th of the quarter circumference of the earth) it became of great commercial value to know it exactly. Observations were accordingly instituted in great numbers.—Those which are recorded after the commencement of last century may be relied upon as generally correct. Huyghens, observing considerable discrepancies in the length of degrees obtained at different places, suggested the idea that this, and the alteration of length of the duration of a pendulum, might be explained by supposing the earth similar to Jupiter, which had just been discovered to be not quite spherical. The amount of ellipticity was calculated by Newton, and others; and is considered to be about 1-300th of the equatorial diameter.—It will be noticed, from any table of observations, that the degrees become generally longer towards the pole, and shorter at the equator. This arises from the bulging out of the earth at the equator, and its flattening at the pole. These two results at first appear contradictory. Cassini was misled in his first measurements by this. He found that his degrees diminished towards the poles (in all likelihood from errors of observation), and he considered this result in accordance with Newton's theory of the flattening of the earth at that place. His mistake being pointed out to him, he maintained that his experiments were nevertheless accurate; and that, in spite of gravity, the earth must be flat at the equator, and elongated instead of flattened at the poles. His persistence in the opinion was the occasion of two expeditions being despatched to Lapland and to the Equator; which established the general result of the increase of length in the degree, toward the poles.—If we consider in what manner the number of degrees of latitude over which one advances in a given space, is measured, we shall avoid Cassini's mistake. It is by the

gle between the vertical lines at the two ces,—for it is to these that we refer the star ich we observe. If we notice, for example, at a star that was in latitude 45° has changed 47° 10′, we are certain that we have passed er 2° 10′ of latitude; and as both measure-nts refer to the respective *vertical lines*, we in ct measure *their* divergence. If now we take egg, and draw perpendiculars at its end, and its middle to the surface—two perpendiculars parate by half an inch at each spot will meet oner at the end than at the middle, because e curve of the surface is greater there. Hence e angle contained between these two intersect-g perpendiculars will be greater *there;* and as e space corresponding to it at the surface is e same in the two cases, the space correspond-g to equal angles between the intersecting lines ll be less at the end—that is, at the elongated rt. Now this part corresponds to the equa-rial, and the middle of the egg to the polar gions; and the lines of the perpendiculars are e lines of verticals at the spots; wherefore e length of a degree will be less at the equator an at the poles,—increasing from the equator the poles, through the intermediate space.—ae inequality of the lengths of a degree is thus, once a proof and a consequence of the ellipti-l figure of the earth.

Degree of Longitude, is the space be-een two meridians that make an angle of 1° the poles, measured by the arc of a circle,

Deg Lat.	English Miles.	Deg. Lat.	English Miles.	Deg. Lat.	English Miles.
0	69·07	31	59·13	61	33·45
1	69 06	32	58·51	62	32·40
2	69·03	33	57·87	63	31·33
3	68·97	34	57·20	64	30·23
4	68·90	35	56·51	65	29·15
5	68 81	36	55·81	66	28 06
6	68·62	37	55·10	67	26·96
7	68·48	38	54·37	68	25·85
8	68·31	39	53·62	69	24·73
9	68·15	40	52·85	70	23·60
10	67·95	41	52 07	71	22·47
11	67·73	42	51·27	72	21·32
12	67·48	43	50·46	73	20·17
13	67·21	44	49·63	74	19·02
14	66 95	45	48·78	75	17·86
15	66·65	46	47 93	76	16·70
16	66·31	47	47·06	77	15·52
17	65·98	48	46·16	78	14·35
18	65·62	49	45·26	79	13·17
19	65·24	50	44·35	80	11·98
20	64·84	51	43·42	81	10·79
21	64·42	52	42·48	82	9·59
22	63·97	53	41·53	83	8·41
23	63·51	54	40·56	84	7·21
24	63·03	55	39·58	85	6· 0
25	62·53	56	38·58	86	4·81
26	62·02	57	37·58	87	3·61
27	61·48	58	36·57	88	2 41
28	60 93	59	35·54	89	1·21
29	60·35	60	34·50	90	0·00
30	59·75				

arallel to the equator, passing between them. is always proportional to the cosine of the ngle of latitude. The foregoing table of the lengths of a degree of longitude, is calculated according to this rule. It supposes the earth to have a diameter the same as at the Equator; where the length of a degree of latitude is assumed to be $69\frac{1}{15}$ English miles,—and also that the earth is perfectly spherical.

Deliquescence. A body is said to be deliquescent when it absorbs aqueous vapour from the air, and dissolves in it. All salts soluble in water are so far deliquescent. The property is made use of by chemists to dry gases which contain aqueous vapour. These are passed over a layer of deliquescents—such as chloride of calcium.

Delphinus (*the Dolphin*). One of the old Greek constellations. It succeeds Aquila in the sky. It has no stars larger than of the third magnitude. Its principal cluster of stars comes on the meridian, about a quarter of an hour after the chief cluster of Aquila.

Deneb. An old Arabian word used to signify the tail. It is employed sometimes to signify the place of a star in a constellation. Thus Deneb adige, the bright star in the swan's tail; Deneb eleet, the bright star in the lion's tail (β Leonis). The name is now almost appropriated to this latter star. The words are Arabic.

Density. The density of a body is the degree of closeness between its particles. The term depends upon the hypothesis that the ultimate particles of matter have a weight, and, therefore, mass proportional to their bulk. The hypothesis may, or may not be true. It is probably not to be depended upon. The density of a body is considered to be the proportion of its mass to its bulk. The mass is considered to be the space which the really solid particles of the matter of a body occupy together, omitting all the intermediate spaces by which, in actual bodies, these are separated. If the particles of the mass be always of the same weight for their size, we may measure the density, or closeness of particles, by the proportion of the weight to the volume. Whether the theory be correct or not, this is the practical method used to determine the density. It coincides with the *specific gravity* of a body (*q.v.*)

Depression, Angle of. When we look at an object elevated above the horizon, the angle which the line, from it to our eye, makes with the horizon, is called the angle of elevation. This angle is just that made by that line, and the line in which a vertical plane through it would cut the horizon. If now we draw a line parallel to this latter line, the angle which it makes with our first line of direction is called the angle of depression. It is equal to the angle of elevation. It is the depression below his horizon at which a spectator at the object would see the observer.

Derivations. An algebraic term. The nature of it is explained under CALCULUS.

See the work of Arbogast.

Determinants. See POLYNOME.

Deviation, Angle of. See DIOPTRICS.

Deviation of the Line of the Vertical. The figure of the earth is that of an ellipsoid, flattened at the poles. It is very nearly, but not quite, circular. Were the earth composed of homogeneous matter, the law of attraction which holds between all substances (of the inverse square of the distances) would require that at any point of the surface, a body, free to move in any direction, should stretch a string perpendicularly to this elliptical surface. The irregularities which present themselves in hills and hollows must be considered as not existing, and the surface which we take instead of the actual one would be the level at which the sea would stand if it could freely interpenetrate all our hills. If then the earth were homogeneous, or if it were made up of homogeneous layers all similar in form to the outer rim, though differing in physical character, the direction of a *plumb line* would be perpendicular to this surface.—Now, instead of this, we discover considerable deviations from such a law. According to it, the degrees of latitude would measure more, for example, as we proceed from the equatorial to the polar regions. We do not find this uniformly true. No doubt, there is a general tendency to this augmentation; but in certain cases it is temporarily disturbed, and that sometimes over a very large extent of country.—We have supposed that the earth is quite elliptical. May not the error of this supposition be enough to account for the deviations of which we speak? So far indeed it is. A vertical line is deflected from the true vertical sensibly, for instance, by the near presence of a very high mountain, as in Maskelyne's experiments on Schehallion. But it has been found that although this element of the problem be taken into account, it does not give a satisfactory reason for all the deviations. They sometimes are found in spots where no great elevation or depression of the surface is perceptible. There has not yet been offered more than a hypothetical explanation. That, however, accords so remarkably with the facts that very great confidence may be placed in it. It consists in the general statement, that the earth is *not* formed of homogeneous matter. It is certainly not formed of homogeneous layers. But, although we could only be warranted in assuming that a free weight will hang perpendicularly to the surface in those cases; it might still happen to do so, although the mass of the earth be heterogeneous. Geologists assert, however, that they can point out, in the character of the soil, and the nature of the neighbouring rocks, a sufficient cause, in the majority of cases, for the actual deviations. They even venture to predict from this deviation that the earth has, in some places, had its crust rent by certain fissures, which nevertheless they cannot yet reach,—and to tell us the direction in which these rents have been made.—Whatever be the explanation, the fact that the *plumb line* deviates from the true perpendicular cannot be questioned. Very careful astronomical, and very expensive geodetical experiments are, however, necessary, before this deviation becomes sensible to observation.

Dew. There is, perhaps, no phenomenon recurring so frequently, and so manifest in its effects, that waited so long for a satisfactory explanation as this. — Before stating that explanation, we shall require to have, clearly before us, the various phenomena of dew, that we may understand the problem, and verify its solution. It will be interesting to trace in history the successive observations of these phenomena, and watch the theories to which each new observation gave rise.—The first, noted extensively, was the fact that dew falls almost solely on nights when the skies are cloudless, and the atmosphere at a low temperature. This coldness was found always to obtain, when dew had fallen. In point of fact, it is now found that the temperature must be considerably lower in a night when dew is formed, than it has been through the previous day; but this was not the form in which the statement was at first put. If we had a temperature of 120° Fahr. through the day, dew might, readily enough, be obtained at temperatures which we should call hot (60° or 70° Fahr.) These circumstances did not trouble the observers,—and they noted correctly that cold was necessary to the formation of dew. They found, however, almost simultaneously, that the sky required to be wholly, or in a great measure cloudless, and that either the stars or the moon must be distinctly visible. They conceived, then, that in the latter phenomenon the real *cause* was to be detected, and that the cold and the dew were *concomitant effects*. They supposed that the stars and the moon shed down a cold, and therefore, a humifying influence. They farther conceived this confirmed on observing that more dew falls at full moon than at any other time (which fact, by the way, they could not have observed properly); and they joined to this, their own crude notion that there is a peculiar chill in the lunar rays. Some transcendental ideas about the celestial purity of this stellar and lunar rain, completed the most ancient theory of dew.—It is worth while to point out how plausible this method of philosophizing at first sight may seem, and to warn the reader against similar attempts to explain phenomena. The ancients started with two facts, sufficiently observed, to be perfectly trustworthy. But, at that point, observation ceased. They raised hypotheses to account for the phenomena; but never thought of inquiring whether, though the explanation might serve, if these hypotheses were true, such were really to be found *in rerum natura.* They met with the success with which similar attempts—except in a very few, very rare instances—are sure to

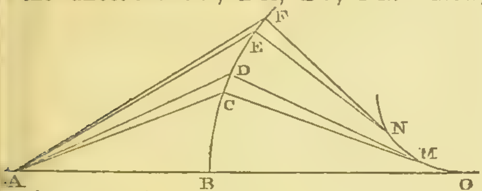
neet. The philosopher, if he wishes to arrive at *ruth*, must always fall back upon Newton's nodest motto, "Hypotheses non fingo."—Hence lew was conceived to fall from the sky; and the rigin of that expression (found in all known anguages) is to be sought in this plausible heory. Aristotle first called attention to the real mount of accurate knowledge upon the point, nd added two new facts, viz.: that dew is never ormed in windy weather, or on the summits of nountains. Both observations are to be taken within a limit. Dew is not formed by any means o readily under either of these circumstances. t is wrong to say that it is never found. These observations, however, were very valuable; and he use which Aristotle made of them was remarkable. His theory, forms an illustration, almost s striking as the former, of the manner in which arefully registered observations, cautiously easoned upon, will lead us to new truths. He onjectured that dew was merely a discharge of apour from the atmosphere. Vapour was water ningled with heat, which rose during the day me, or so long as it could obtain heat. Vapour, he nagines, will never rise high in the atmosphere, ecause it must lose its buoyant heat; conceiving hat to be some thin fluid which might become etached. Hence dew is most copious on low ing ground. Moreover, if vapour did rise high, e agitation of the upper air would dissipate it. arrying out this idea, wind dissipates the vapour; on windy nights, therefore, dew will not formed. The sun then, and not the moon stars, is the cause of dew, because through its at vapour is produced, and in its absence the ld comes.—This theory is, for that age, a most narkable approximation. It still deals slightly l hypotheses; but every hypothesis, right or ong, originates in some fact which renders probable. Considering the uncertain state of e science of heat (reduced only in this century a branch of mathematical physics); and the er ignorance then prevalent concerning the de of formation and the character of vapours; result more close to the truth has probably ever en reached upon such a subject.—Aristotle's ory was, however, not appreciated in his own , nor by his successors among the Greeks and mans. The notion of the influence of moon l stars was too beautiful for a Greek to give and the Romans could not do better than ow the example of their masters. In the k ages, physical inquiries were not favourably wed, and even the schoolmen did not venture support Aristotle's theory. The domain of physical sciences was given over to the almists, and they could not, on any account, fail support the claims of the moon and stars. Baptista Porta rejected that part of Aristotle's ory in which lies its chief merit—the attriion of dew to condensed vapour—and tried show that the air itself was condensed. He nded his conclusion on the existence of moisture, and sometimes hoar frost, due to the congelation of what had been moisture, on the inside of a glass pane. This observation justified him, indeed, in rejecting the theory of the celestial influences. A similar conclusion was deduced from this other fact, that a bell glass placed over a plant to cover it from the frost, was copiously covered with dew in the inside, while very little appeared on the outside. The same conclusion was inferred from the circumstance, that plates of copper exposed during the night have only their *under* surfaces bedewed. —These facts served to explode completely the hypothesis of stellar influence: but a new one began to rise into notice. It was this,—the moisture with which bodies are bedewed, comes from the earth, and very copiously from plants; both which, under the influence of cold, exhale their moisture. Experiments were instituted as to the quantities of dew, obtained at different heights from the ground; and the result, with some variations certainly, seemed to show, that, —as might have been expected from this supposition—it forms more copiously at the surface of the earth than higher up in the air. In pursuing experiments tending further to establish this point, Muschenbroek stumbled upon a new phenomenon, hitherto quite unsuspected—that dew forms more copiously, upon some substances than upon others, placed in the same circumstances as to position and temperature of the surrounding air. He discovered, moreover, that it is formed, when it was very improbable that it could have risen from the ground.—It was now, therefore, held that dew does not fall from the sky; nor was it, at all events, necessary to its formation, that it should rise from the ground; and the readiest explanation which offered itself, was, that Aristotle's theory had been, after all, the true one, and that dew is produced by the condensation of vapour, held, as it were, in solution, by the air. According to this theory, moreover, phenomena, which, on close examination, one can see no reason for distinguishing from the formation of dew, received a natural explanation. When a mild spring comes after a severe winter, secluded passages, vaults, and other places concealed from the direct influence of the sun, and to which heat is very slowly conducted, become damp, and sometimes dripping with moisture.—If, therefore, this theory be correct, a deposition of dew will be obtained when we expose a very cold substance to the air of an ordinary room at any time. This testing experiment was tried, and succeeded admirably. Thus, if we have a tube with two pendulous bulbs at the end of it, in one of which is water as cold as we can obtain it, and around the other a cloth wrapped, which is moistened with a little ether, dew will be found upon the first bulb. The ether evaporates, and as heat is required to convert liquids into gases, it must have heat from some quarter. This it easily

procures from the water, which is cooled considerably, and on the outside of the bulb containing it dew is deposited. The phenomenon will be more evident if the latter bulb be blackened.—Philosophers now perceived that they were upon the point of discovering something, which, whether or not it might give a perfect explanation of the whole phenomena, would, at all events, cast considerable light on them. Several observers, therefore, gave great attention to the subject, and published very valuable experimental results. Prevost of Geneva, Six of Canterbury, and Leslie of Edinburgh, especially distinguished themselves. Their observations went, chiefly, to render indisputable the general truth of the observations already recorded—such as the invariable relative coldness of the body upon which dew forms; the progressive diminution in its amount, according as the bedewed substance is more and more removed from the earth's surface; and the effect which different substances exercise in modifying the quantities.—The next, and most important contribution was made by Dr. Wells, whose experiments, though perhaps not conducted with the greatest accuracy possible, nevertheless, were of the highest importance in establishing the theory of the subject. The essay which he published upon it, takes rank with the finest models of experimental inquiry after truth. His chief observations were, that the coldness of the surface bedewed, always *precedes* the formation of the moisture, and is not a consequence of it; and that dew is always most abundant when the air contains most vapour of water—other circumstances remaining the same.—Having thus reviewed the successive theories of dew, and seen by what prominent characteristics the phenomenon is distinguished, we now proceed to explain the theory at present universally admitted.—Water is found in the gaseous as in the liquid state. It is gaseous as steam, and as those vapours which, in a hot day, we see rising from rivers, or from a field on which rain has just fallen. In the atmosphere, therefore, two gases—dry air, and this aqueous vapour, exist together unmixed. According to Dalton's Law they ultimately each expand, occupying the same space as if the other did not exist; and there will always be a perfect intermixture if sufficient time be allowed. The first effect of this must be, to remove or carry up the aqueous vapour from the surface of all water. Now, it has been found that, at any temperature, water will evaporate entirely under whatever gaseous pressure, if means be provided for the removal of the aqueous vapour as it forms. If it be kept in a closed vessel, the stopper, even although not very close fitting, prevents that vapour from free passage into the atmosphere; and the aqueous vapour, therefore, which the water gives off, not being allowed to dissipate, forms a perfect barrier against further evaporation. In a space filled with water and with air at a certain temperature, the water will evaporate to a *certain* extent, corresponding to the temperature; and if the vapour be kept from dissipating, it forms then a perfect cover against further volatilizing. But, in free space, unless there be an atmosphere already quite filled—to an extent corresponding to its temperature—with vapour, it will dissipate, according to the universal law of the dissipation of gases, into the higher atmosphere, where there is less vapour. The bar to further evaporation is then removed, and it continues until the adjacent space be saturated as far as its temperature will permit; it then ceases until the vapour shall have again partly disappeared, and so permits the formation of more. Air, at any temperature, will contain only *so much* of vapour of water—a fixed amount for each temperature. This amount increases with the temperature, and in an increasing ratio.—Suppose, now, that this process goes on for some time, say through a summer's day, when, as the temperature of the strata of air near the surface will be 70° or 80° Fahr. it will have a very great capacity for vapour of water. Evaporation will go on rapidly, as on a hot day we see that it does, and the atmosphere becomes filled with a very considerable amount of water in this state. Now, if any great cooling of the atmosphere takes place, it gets to be no longer capable of containing the same amount of vapour of water. If, for example, in the day we have chosen, as much vapour have passed into the air as would have saturated air at 60° Fahr. and if the air at night cool down to 40° Fahr. there is a considerable quantity of vapour which the air at this temperature cannot hold. This excess is condensed and deposited in the form of dew.—So far, this is precisely Aristotle's theory, with some more definite understanding of the cause of the condensation, and of the other results by which it is preceded; but there is yet one point requiring explanation, and that is, the method in which the cold is produced. The mere absence of the sun, one cannot expect to produce cold. That causes a cessation of the increase of heat which had been going on. During the presence of the sun, in the afternoon, and at sunset, indeed, there is this diminution of temperature while the sun is present. The theory of radiant heat which Prevost started, and which is now so conclusively established, gives a full explanation of the phenomenon; and for the first statement of that explanation we are indebted to Dr. Wells.—During the day time, heat is transmitted from the sun to the earth. Wherever we have two bodies of different temperatures, there is a radiation of heat from the hotter to the colder. Now, the sun is a body at a very high temperature, and the earth at a moderate heat. During the day, then, those parts of the earth to which it is day, being exposed to the

sun, absorb quantities of radiant heat. The air also absorbs it. It also is a body exposed to the sun; though one which both radiates, and absorbs much slower than the earth. Hence it also is heated during the day time. According to this statement of the case, however, the upper air would have the highest temperature. Two circumstances prevent this. The very rarity of the upper air—the property in which it is likest heated air—prevents it from heating by radiation. It absorbs less rapidly the heat from the sun. The earth absorbs that, most rapidly of all, and as the air can conduct heat, it gives a portion of its heat to the stratum lying upon it. That stratum becoming heated, rises, and a new stratum is deposited to be again heated, and again to rise. There is thus a tolerable uniformity of temperature produced near the surface.—Next, we have, on a clear night the earth laid open to the stars and to the interstellar spaces. We do not, by any means know the temperature of the first, but they are too far away, for their radiant heat, any more than their light, to be able to exercise considerable effect on us. The earth, then, and the interstellar spaces are exposed, one to the other. Now, we know that the temperature of the latter is at least 100° Fahr. below zero, while the surface of the earth, having just absorbed an amount of radiant heat from the sun, is perhaps 50° or 60° Fahr. above it. Radiation commences immediately, and if uninterrupted, would continue until the temperatures were equalized. The effect of this radiation, while it lasts, is to lower the temperature very materially at the earth's surface. Let us consider the state of the air during this period. It is not affected by the transmission of the radiant heat from the earth outwards, to space: but it serves itself to radiate some heat away. It does this so slowly, however, that the effect in cooling it from this cause, like the heating effect from the same, during the day, is comparatively insignificant. Now, as seen, the chief effect is produced by the conduction of heat from the air to the ground. By this, however, the stratum of air next the ground, is cooled, and it does not rise to give place to another. The cooling process goes on, therefore, until the stratum is equal in temperature to the ground, or rather, its temperature continues to diminish indefinitely, along with that of the ground. Now, during the day, vapour has been collecting in this stratum, perhaps not quite up to its saturated point, but generally very near that. Hence, when this cooling lowers the temperature, the air is no longer able to retain its vapour; and moisture is detached and deposited as dew.—By conduction, the air above also cools, and dew gradually separates from it. As air, however, very slowly conducts heat across except when the heat puts the heated air in motion (see CONVECTION), this process is very slow. Hence very little moisture is detached from the upper portion of the atmosphere. But, it may be asked, does not dew deposit upon bodies at considerable elevations from the earth's surface—elevations that are yet such, that if dew fell from them on the earth, there would be a shower of dew? In answer, let it be noted, in the first place, that the layer of air in immediate contact with the earth does not remain constantly so, through the night, in virtue of its superior heaviness. It is well known that when vapour is formed from water, a considerable quantity of heat becomes latent; and when, on the other hand, condensation takes place, heat is evolved. When, therefore, dew begins to separate from the bottom layer, it receives a sudden accession of heat from the condensed vapour, which lightens it, and carries it upward. Thus, a new layer, more saturated with vapour than the former one, after its loss, descends by the superior weight due to its lower temperature, and takes its place. Therefore, there is no necessity for a *fall* of dew in any case. Yet dew seems to separate, nevertheless at considerable elevations, though less copiously than at the surface. It only separates, however, when a body which radiates heat well is placed at these heights, and the quantity in which it separates depends upon this faculty of the body. This explains Muschenbroek's observation of the different capacities of different substances for dew. A good radiator becomes rapidly cold, and dew is rapidly formed on its surface; while a bad radiator in the immediate neighbourhood remains unaffected. Thus, a slip of tin foil pasted upon a sheet of glass, will remain perfectly dry while the rest of the glass is profusely covered with dew. This property may be strikingly shown by experiments, which, founding upon it, describe figures and letters in dew, on plates on which glass and tin foil have been suitably arranged. The exquisite little figures made by dew, and especially by hoar frost, on window panes, depend upon this principle. The glass, from various causes, does not radiate uniformly.—According to this property, therefore, no dew separates in free air, at any considerable elevations—unless a very good radiator have been placed there.—It still remains to be explained why, in this last named case, dew is never formed so copiously as at the earth's surface. This partly arises from convection of air. If we place a body which can become cold by radiation, at a considerable height, it renders the layer of air in immediate contact with it also cold, and so, heavier than the air between the earth and it, which is but little cooled by radiation, and, if at some height from the earth, not very much by conduction. This air, therefore, falls, and as at the side, it is unsupported, it falls away from the cold plate, leaving warmer and lighter air pressing up to supply its place. If indeed the plate be perfectly horizontal, the air at the surface of it would remain, and dew would be produced; but it is not easy to adjust it so,

and the least inclination will allow the heavy air to fall away, and prevent dew from being formed in considerable quantities, if at all. Hence dew will more copiously form at the bottom of a vessel so situate than in any other position. Besides this, however, there is, generally speaking, not so much vapour of water present in such a case. The vapour which has risen through the day, requires time to interpenetrate the air. If time be given, Dalton's law will be completely fulfilled; but it is not given, in the course of a day, when new disturbance is added, every minute, by new vapour forming at the surface. Hence at certain heights there is less moisture present in the air than at the surface; and so the radiating body must be cooled lower still, before dew is formed. Dew will therefore commence to form later in the upper air, from convection, and the lesser supply of aqueous vapour; and the same causes will evidently cause less of it to be formed there, in the most favourable case, than at the ground.—Two other phenomena noted in our historical sketch are explained in the same way. Dew is more copiously formed on the inside of a bell glass than on the outside, and on the lower side of a copper plate, held at a moderate distance from the earth. In both cases convection prevents a copious deposit on the upper surface;—and, in both instances, the interval between the ground and the plates is small enough for the whole air included in that interval to be cooled below the point of saturation; and therefore for dew to be formed even at its loftiest places. This is accomplished chiefly through that conducting power of the copper or glass, which takes the supplies of heat they continue to dissipate, from their whole mass, lower as well as upper. This must draw its supplies, in turn, from the air which must constantly descend as it cools, until all of it has passed the point of saturation (dew point). —When the cooling goes on rapidly, the vapour deposited on dew, finally solidifies into *hoar frost*. The two phenomena rest upon the same principles, for their explanation. While dew is beneficial to plants, hoar frost is eminently destructive. Hence gardeners seek to prevent such a refrigeration, as would cause hoar frost. Their method is very simple. They spread mattings over their plants, and protect them from the sky—precisely as running water is protected by ice. See CONGELATION. The radiation thus takes place between the plants and the matting, rather than between them and the sky. The matting radiates to the sky, and loses its own heat rapidly; but, at first, its temperature is not far below that of the plants, which therefore transmit very little heat to it. Even when the matting has become frozen, the temperature is much higher than that of interstellar space; and the radiation from the plants is not, therefore, nearly so violent. If the process were continued, indeed, the three temperatures would be perfectly equalized, but the return of day prevents that. The matting serves merely to retard the equalization of the temperatures of the sky and ground, by interposing. Sometimes there is not even dew formed in this way; but very seldom, except in the severest weather, is hoar frost formed.—The same principle explains another phenomenon, which we noted in connection with the subject. Dew is very seldom formed except in a clear sky. When the sky is clear there is free radiation to the interstellar spaces. When, on the contrary, there are clouds in the sky, the radiation is from them to the sky, and from the ground to them. To be perfectly correct, indeed, we should say that there is radiation from the ground to the sky, but the moment that the temperature of the earth gets lower than that of the portion of the cloud turned towards it, a counter radiation sets in from the cloud; supplying the loss from the radiation to the sky, and permitting only a very slight decrease of the temperature from that point. Radiation from the earth to the spaces is, moreover, just as in the matting, nearly intercepted by the interposed screen. The worse conductors such bodies are, the better will they serve the purpose of protecting the earth, and it would not be easy to find a worse conductor than the clouds.—We do not enter into a detailed explanation of how the theory accounts for all the different phenomena. In a windy night dew is seldom deposited. No one layer of air is kept in contact with the earth long enough to cool, as may readily be seen. The fact that it is formed most copiously in valleys, and least on pointed hill tops, will suggest its own explanation, in the principle of convection; and other such phenomena may safely be left to the ingenuity of the reader. It is impossible to conclude, however, without pointing out the very striking protection which the formation of dew affords against those night frosts, which, without it, would totally destroy all our present vegetation. The moment a particle of vapour is condensed into dew, the heat which had been required to preserve the gaseous state is disengaged and set free. This heat partly serves to keep up the supply of air, well saturated with vapour, by driving up the layer which has been just used, and thus prepares to repeat its effects; and partly goes to the earth, where it serves to supply the loss by radiant heat, and to retard the progress of its refrigeration. When the new, well saturated layer descends, this process is repeated; and thus a regular supply from the heat given in the day, to form vapour of water, is made to moderate the rapidity with which, according to the laws of heat, the earth would cool in the night time. Instances of this compensating character are found throughout His works. There is no waste of energy permitted; and that which seems uselessly squandered during the day, proves to have been only gathered up for indispensable service in the ensuing night.

Diacaustic Curve. A species of the ıustic curve (see CAUSTIC) formed by refraction. ıe properly caustic curve formed by reflection, termed also catacaustic.—When a number of ys come from a luminous point, such as A, and ter upon a rarer or denser medium, whose rder is such as B C D E F, they do not proceed the direction A C, A D, &c., but are refracted the directions C M, D M, E N, F N. Now,



agine A C and A D to be lines infinitely near, one to the other, their refracted rays C M d D M will come to cut, generally, in a point, e M. If we further imagine all the innu- rable rays refracted from the curve to be so uped in pairs, of those nearest one another, obtain a series of points of junction, like M, ich will present together the appearance of a rve, described by a dotting instrument, and if imagine the rays infinitely near, each suc- sive point of junction will be infinitely near preceding, so that we obtain a complete rved line—according to the description of a e as made up of an unlimited number of points, l infinitely close, the one to the other.—This ved line, represented in the figure by O M N, alled the diacaustic curve.—In order to find diacaustic curve mathematically, in any inite circumstances, we require to remember t the sines of the angle of the incident ray, l of the angle of the refracted ray, with a pendicular to the refracting surface, bear a stant ratio, when we pass from one medium nother.

Diagometer. A sort of electroscope in- ted by M. Rousseau, where the dry pile is de to measure electrical forces. The instru- it is of use in teaching us the conducting rer of bodies as regards electricity. It is etimes made use of in commerce for mea- ing the purity of oils.

Diagonal. In plane figures any line drawn n one corner of the figure to any other, not the same side as the first. In polyhedrons, the line joining two angles whose vertices not on one plane side. The diameter of a le is sometimes called its diagonal. The nber of diagonals capable of being drawn n any one angle in a polygon is the same as number of sides wanting three.

Dialling. The sun dial consists merely of raight rod firmly attached to a plane surface, ng which the sun throws its shadow at the erent hours of the day, in such a way that may know the time of day by the position of shadow. As the sun is not at the same t in the sky at the same hour of the day in different times of the year, it is manifest that the only sun dial of any value, will be one which will throw the different hour shadows constantly in the same direction; and every dial will not do this. The straight rod requires, in fact, to be pointed towards the pole star of the place. Then, as the sun's diurnal revolution round the pole is uniform—(with slight variations that are neglected in dialling,) the place of the sun at different hours of the day, will be, at all times of the year, in the same hour circle; and the sha- dow of a line towards the pole, from any object anywhere on the circumference of that circle, will differ only in magnitude, not in direction. Each hour will therefore be indicated by a shadow line along it all the year through.—Dials are usually constructed with the plane (dial plate) horizontal, the rod (gnomon) always being turned towards the pole. In either case, the shadows for the different hours are not at equal angular distances. The hour circles, and therefore the planes of shadow are. The reader will at once comprehend the reason by cutting an orange through its centre ob- liquely to its axis. Each lith is of equal size, but the exposed surface of each on the freshly cut circle will not be so.—If the orange were cut perpendicularly to its axis, they would be so. If in different directions, oblique, to its axis, the relative spaces for the liths would be different. Hence, as the horizontal planes of each place are inclined to those of any other, and the direc- tion of the axis is constant, the graduation of one horizontal (or vertical) dial would only be adapted to the one place where it might be used. If the dial plate be made, instead, perpendicular to the direction towards the pole, the graduation will be perfectly uniform and regular at every place—as much for one hour as for any other Hence the principle of the universal dial. It is merely a rod through the middle of a circular disc whose plane is perpendicular to it, graduated uniformly, into 24 divisions. Its use in any place presupposes this, and only this—a distinct knowledge of the direction of the north pole from the spectator.

Diamagnetic. A term introduced by Fara- day in connection with his discovery of the in- fluence of magnetic force upon all bodies. By a diamagnetic is meant any substance which, when placed in a field of magnetic force, does not con- duct itself in the same way as the magnetic bodies, Iron, Nickel, and Cobalt. Before Fara- day's discovery in 1845, the known properties of almost all the diamagnetics in relation to mag- netic action were merely negative. It was known, indeed, long previously, that masses of Bismuth and Antimony act in a remarkable way upon a neighbouring magnet, repelling the near- est pole, instead of attracting it as Iron does; but this remained an isolated and barren fact, and seems to have received little attention till it was reduced by Faraday's discovery to a case of

general magnetic action. In the present article a statement will be given of some of the more important facts brought out by this discovery,—general and theoretical considerations will be reserved for the articles on MAGNETISM and MAGNETIC INDUCTION. It was in the course of experiment already referred to under CIRCULAR MAGNETIC POLARIZATION that Faraday first observed the new mechanical action of magnets upon bodies placed in the field of force. A bar of heavy glass was suspended centrally between the poles of a powerful horse-shoe electro-magnet. When the force of the magnet was developed, the bar no longer swung indifferently, but moved round its point of suspension into a direction at right angles to the lines of force, or at right angles to the direction that would be taken by a bar of soft iron placed in the same part of the field. Round this position the bar made a few vibrations, and finally settled in it. If displaced from it by the hand, it returned to it and settled as before, showing that this position of the bar, which Faraday calls the equatorial, was a position of stable equilibrium. When the bar was placed originally, at rest, in the axial position, or in the position of stable equilibrium of an iron bar, it did not move out of this position under the action of the magnet; but if displaced a little in either direction it moved on into the equatorial position and settled there as before; showing that the axial is a position of unstable equilibrium. It was observed further, that if the bar was placed near one of the poles, it was not only *directed* round its point of suspension, as above-mentioned, but was also repelled as a whole by the nearest pole, the centre of gravity of the bar sensibly receding from that pole, and so remaining while the magnet was retained excited. The effects are simplified by the employment of one electro-magnetic pole instead of two. In this case, the bar is always repelled by the pole in the direction of the lines of magnetic force passing through it; and it moves at the same time into a position perpendicular to the lines of force. The effects are further simplified by using a cube or ball of heavy glass instead of an oblong bar. When such a body is acted on by one pole, it moves constantly outwards in the direction of the lines of magnetic force. When subjected to the action of two poles, the effects are more complex; but there is a simple law that explains them all. It is this, that a particle of heavy glass or other diamagnetic tends constantly to move outwards, or into the positions of weakest magnetic action. The directive tendency of an oblong diamagnetic across the lines of force when one or two poles are employed can be easily accounted for by this law. It is a simple result of the joint tendency of all the particles towards the positions of weakest force. The property of being repelled by magnetic poles is not peculiar to heavy glass; it is on the contrary common to the most of natural and artificial bodies. It may be said without exception, that every known substance, when subjected to electro-magnetic forces of sufficient power, will give some positive result, either of the magnetic or the diamagnetic character. And it is in comparatively few cases that the results are of the common magnetic kind presented by iron and nickel. Faraday himself tested a great number of bodies taken from all classes, amorphous and crystalline, liquid and solid, organic and inorganic. In this examination, the liquids were experimented on by being enclosed in glass tubes, the action of the glass under the magnetic forces being taken into account. Some of the results may be stated. Transparent bodies were the first examined, as was natural in the circumstances of the discovery. And it was found that even such transparent bodies as the singly and doubly refracting crystals, whose magnetic rotatory action upon light is too feeble to be observed, are yet subjected to directive and repulsive actions by magnetic poles in their neighbourhood, as other diamagnetics are. Of opaque bodies again, whose condition under magnetic action could not be tested by means of light, some are found to act very powerfully. Phosphorus may be mentioned as equal, if not superior in its mechanical indications, to heavy glass. Sulphur and India rubber are also well directed and repelled. Among liquids, alcohol and ether are evidently diamagnetic, and water still more so. Of organic bodies, the most are clearly diamagnetic. Wood, starch, sugar, leather, beef, mutton, apple, bread, blood are instances: they are all repelled by the magnetic poles, and, when in oblong volumes, directed equatorially. With regard to the gases, Faraday obtained no positive result in the first series of his experiments in this field. It was discovered, however, by Bancalari, that flames are mechanically affected by magnetic forces; and the subject was resumed by Faraday and other philosophers with success. It was found in the first place, that a current of heated air ascending between the poles of an electro-magnet conducts itself as a diamagnetic, separating under the magnetic action into two streams which ascend on different sides of the axial line. On the other hand, a descending current of cold air is not divided under the action of the poles, but keeps to the axial line. A descending stream of oxygen acts as a powerful magnetic body; if its proper line of descent is on either side of the axial line, its direction is changed by the action of the magnet, so as to intersect the latter line. All the other gases, when treated similarly, are found to be diamagnetic, with the exception, perhaps, of nitrous gas. To render the effects of the magnetic action more evident, Faraday enclosed the gases in soap-bubbles, and set them afloat in the magnetic field; and this test was found to be very delicate. Oxygen in these circumstances was powerfully attracted, and the other gases gave results in accordance with previous experi-

its. By employing glass tubes as the enve-s of the gases, in connection with a delicate ion-balance, Faraday has even compared the es magnetically with each other, and with mselves in different states of density and tem-ature. The contrast brought out by this hod between the two principal components of atmosphere is very interesting, and highly ortant also from its bearings on the Theory Terrestrial Magnetism. The magnetic power oxygen suffers evident diminution by a inution of density, and also by an increase temperature; though the gas cannot be efied or heated to such an extent as to lose magnetic power. Nitrogen, on the other nd, which is a diamagnetic, undergoes no sible change in its magnetic relations by utmost attainable changes in density and perature. In this brief view of the natural ies in their magnetic relations, we have to r finally to the metals. Iron, nickel, and alt, have been long known as magnetic metals, ugh the second is far inferior in power to n. To these metals Faraday has added the owing,—platinum, palladium, titanium, man-nese, cerium, chromium, and perhaps osmium. ne of these are inferred to be magnetic from ect observation of their actions in the field of e, while the character of others has been de-mined by the actions of their chemical com-nds. The rest of the metals appear to be n-magnetic in the common sense of the term. ey are found to be all subject to the influence he magnetic force, and they produce the same eral effects as heavy glass and the other dia-gnetics already referred to. The repulsive es are manifested on the diamagnetic metals very different degrees. Some of the metals, gold, copper, silver, are inferior as diamag-ics even to water; and with the exceptions of imony and bismuth, they are all inferior to vy glass. The last mentioned metal has been racterized by Faraday as one of the best sub-nces for the exhibition of the entire diamag-ic phenomena of the mechanical kind. The duct of the various compounds of the metals ler magnetic action has been studied with at attention. We may merely state the gene-l law which has been arrived at upon this nt by a very extensive induction;—that the idedly magnetic and the decidedly diamag-ic metals preserve their magnetic characters oughout all changes of mixture and chemical nposition and solution. By inference from s law, Faraday has determined the magnetic racter of some of the metals, whose indications the magnetic field could not be depended upon. need hardly be observed that it is not meant the above statement, that a solution of salt iron for example, will act evidently under gnetic force in the same way as iron itself s; for it may or may not so act according to cumstances. The magnetic action of a mixture or chemical compound of different matters is the resultant of the actions of all its constituent parts; and the law is, that the action of each part is proper in kind and quantity to the part itself, and independent of the circumstances of mixture and chemical composition into which the part may enter. A solution of iron then will be magnetic or diamagnetic according to the strength of the solution, that is, according to the proportion of water or other diamagnetic matter with which it is diluted. It is interesting to observe generally in this connection that the magnetic and diamagnetic properties of the parts of any mixed body, while they oppose each other in their effects, appear to interfere in no degree with each other in their proper actions. The delicacy of the experimental researches that have been conducted in this field is well illustrated by the phenomena of mixed bodies. Glass in a pure state is evidently diamagnetic, but in the common forms of green-bottle and crown it is as evidently magnetic, in virtue of the small quantity of iron present in its mass. Wood in a pure state is diamagnetic; but to obtain a chip that will conduct itself as a magnetic body we have only to detach it with a common knife. Common paper again has been sometimes found to possess magnetic properties; and the fact has been explained by the contact of iron with the paper in the process of manufacture. In the previous part of this article, bodies have been considered in their magnetic relations without reference to the *enveloping matter*, and they have been spoken of as absolutely magnetic or diamagnetic. This view has preserved us from unnecessary complication in the statement of elementary facts, but it now requires an important correction. The conduct of a body in the magnetic field depends as much in fact upon the nature of the enveloping matter as upon the nature of the body itself. As a simple instance:—there are certain substances which set equatorially and are repelled in air, while in water they set axially and are attracted; in other terms, they act as diamagnetics in a magnetic field which is occupied by air, while, in the same part of a precisely similar field which is occupied by water, they act as magnetics. Phenomena of the same kind are presented by other media. There are certain remarkable experiments of Faraday's that are worthy of notice in this connection. Glass tubes were filled with solutions of iron of different strengths, and were hermetically sealed. Vessels were also filled with the solutions, and placed successively in the magnetic field. The tubes in succession were immersed moveably in the vessels when occupying the field of force; and though all the tubes were magnetic they sometimes pointed equatorially. The phenomena presented this constant law;—that if the solution in the tube were stronger than that which enveloped it, the tube conducted itself as a magnetic body; if on the other hand, the in-

ternal solution were the weaker, the tube was repelled and directed equatorially, and could be distinguished in no way from a diamagnetic. These facts have an evident bearing upon the nature of the distinction between bodies as magnetic and diamagnetic. But we consider them at present merely with an inductive view, and without reference to theoretical questions that may be raised in connection with them. From the above statements it is evident, that we cannot properly speak of a body as magnetic or diamagnetic without reference to the medium in which it is placed. But in practice we can dispense with the reference without any danger of mistake, by agreeing that where no medium is mentioned the air is understood. After this, the action of media as far as determined may be easily described. A magnetic body will act as a magnetic in every diamagnetic medium, and in every medium of less magnetic power than itself, but as a diamagnetic in every medium of greater magnetic power. A diamagnetic will act as a diamagnetic in every magnetic medium, and in every medium of less diamagnetic power than itself; but as a magnetic in every medium of greater diamagnetic power than itself. In conclusion, we may observe, that these and other facts are powerfully in favour of the idea, that bodies differ from one another as magnetic and diamagnetic only in virtue of the difference of degrees in which they possess the one common magnetic property. And that therefore a mass of bismuth or heavy glass recedes from the magnetic pole, not because of a repulsive action of the pole upon the diamagnetic, but because of a greater attraction exerted upon the surrounding medium: just as smoke ascends in air, and light solids in water, in opposition to the proper attractions exerted upon them. This view was stated by Faraday as probably the true one, in connection with his singular experiments on solutions of iron already described; but he has seen reason to question it, and to speak of the movements of diamagnetics in the field of force as due to a proper repulsive action of the poles, an action therefore entirely different from the common and long-known action of magnetic force. Whatever view be taken upon this point, there can be only one opinion as to the importance of the discovery of "the new magnetic condition." Magnetic science has received from it a very great extension. Those subtle forces that were formerly known only as exerted upon iron and two other metals are now recognized as acting effectively upon all bodies, and as exercising, in all probability most important functions in various departments of the great system of nature.

Diaphanometer. An instrument proposed and employed by Saussure for measuring the transparency of the air. It consisted of a number of circles of different diameters drawn on white grounds. Having placed these beside each other, the observer recedes until the smaller circle becomes invisible, and measures the distance. Then he recedes until a larger circle becomes likewise invisible, and again measures the distance. If the air be perfectly transparent these distances ought to be exactly proportional to the diameters of the circles; if they are not, the difference is due to the absorption of the luminous rays, or to the imperfect transparency of the atmosphere.

Diaphanous. Transparent. See TRANSPARENCY.

Diathermanism; Athermanism; Thermanism. Reference has already been made to the phenomena indicated by the foregoing terms, in article ABSORPTION. The Inquirer who first carefully surveyed them was Melloni. Desiring to ascertain according to what laws radiant heat either passes through different bodies, or is absorbed by them, this philosopher first investigated how much of the incident heat was neither radiated nor absorbed, but rejected or sent back by way of *reflexion.* This—it turns out—whatever be the plate on which it falls, is $\frac{1}{13}$ of the whole; so that of 100 rays falling on the plate, only 92·3 require to be accounted for by transmission and absorption. This preliminary established, Melloni proceeded to evolve, by a long and elaborate series of ingenious experiments, how far the transmission of radiant heat, through plates, is affected by the *substance* of the plate, by its *thickness*, and by the *source*, and *degree of the heat* incident on it. Different substances vary exceedingly in their power to transmit radiant heat; for instance, *rock salt* transmits the whole; *alum*, scarcely any rays; *black glass*, and *smoked quartz*, transmit abundantly, notwithstanding their opacity. In general, the quantity transmitted varies with the thickness of the plate; but this only up to a certain limit; for after the plate has reached a certain thickness, that thickness may be further increased without lessening the amount of transmitted heat. This amount varies also with the temperature of the source of the rays: sources of low temperature appearing to contain a smaller proportion of transmissible rays: and Melloni has reached the further and definite conclusion, that *the least refrangible calorific rays of the solar spectrum are likewise the least transmissible.* These phenomena, regarded as a whole, manifestly point to the conclusion, that the action of bodies in transmitting heat, is quite analogous to the action of transparent or coloured media in transmitting *light.* The characteristic of coloured media, in this respect, is, that they exert an absorbing power, by preference, over a certain colour: for instance, if a glass plate permits the red rays only to pass, another similar plate, behind it, will not absorb the ray transmitted by the first, whilst a violet plate would impede or absorb the whole of it; or, if the coloured plate, instead of transmitting a simple ray, transmitted a compound tint, the results of

interposition of an additional plate would be ·ely similar, although analyzed with some ·culty. Observe now the phenomena in ·tion. As soon as radiant heat has passed ·ugh a certain thickness of any substance ·ible of transmitting it, the heat becomes ·*fied* or *thermanized* in relation to that sub- ·ce: not only does the ray traverse it more ·y, but it is not subject to any further ab- ·tion on the part of the same substance. If, ·ever, a second substance be brought to act ·he ray thus *thermanized*, quite a different ·lt ensues; rock crystal, for instance, acts in ·rbing heat that has passed through glass, · as if the heat was in its natural state, or · thermanized at all; that is, it absorbs a · portion of it. Glass acts in the same way ·eat that has passed through rock crystal: · two substances, therefore, are quite in the · position with regard to radiant heat, as two ·rently coloured glasses are, with regard to ·t. Now, it is this peculiar property of ·tances—a property enabling them to *select*, · were, the elements of the ray of heat which · claim to absorb, that Melloni designates as · *diathermancy;* or, as he rather might have ·d it, their *thermancy:* the substances thus ·*ting*, are *thermanizing* substances; and the · modified by the action of such substances ·id to have been *thermanized* with respect to ·. Rock salt, for instance, is *diathermanous*, · not *thermanizing*, because it transmits all ·incident rays; and the heat which passes ·ugh it is not *thermanized*, but natural heat, ·essing all its original elements.—As to the ·ence of different *sources* of heat, it must be · in mind that every source does not neces- ·y give out all the elements of natural heat; · as there are coloured flames, there probably · sources of radiant heat which send out ·tive heat, a heat already *thermanized:* ·er must it be concluded that the degree of ·*temperature* of the source, necessarily deter- ·s that its heat contains fewer absorbable ·; for Melloni, has recently established that · salt properly smoked by the flame of a ·le, absorbs a proportion of heat which is ·ter, the higher the temperature of its source.

·iathermanous. A body that transmits ·nt heat is called *diathermanous; ather- ·ous*, if it transmits none.

·ichroism. The optical phenomenon thus ·gnated is peculiar and complex. It may be ·ly described as follows.—Every coloured ·ly-refracting crystal on which a vertical · falls, may be considered as permitting ·ass through it, in the *first* place, a certain ·tity of coloured light, *not polarized;* which · passes freely in all directions, without ·rence to the optical axis, or axes of the crys- ·—the crystal acting in regard to it much · any other coloured medium; and in the ·*nd* place, a certain quantity of light polarized according to Biot's law for biaxial crystals. This latter quantity of light being necessarily variable, according as one transmits polarized light through the crystal or analyzes that light transmitted by a doubly refracting apparatus, there results—when the variable light is itself coloured—a numerous series of shades of colour; from that of the non-polarized light when it passes alone, to that formed by the mixture of the colour of the non-polarized light with the colour of the whole polarized light. If attention is paid to these extreme shades only, the phenomenon is termed *dichroism:* but as there are many intervening shades, every doubly refracting coloured crystal with one or two axes, manifests a veritable *polychroism.*

Dichroscopic Microscope. See MICROSCOPE.

Dielectric. Any substance through or across which the electric forces are acting. Thus, the air between two electrified surfaces is a dielectric, the wall of a charged Leyden jar, and the insulating plate of a charged condenser are also dielectrics: for the electric forces act through these substances, and produce sensible effects at their terminating surfaces. The importance of the functions of the dielectric in all the electric actions is the most distinguishing feature of Faraday's Theory as contrasted with the old Theory of Electric Fluids. Thus, in explanation of the inductive action of one charged conductor upon another, the dielectric was altogether ignored in the old theory, except as insulating the fluids and as occupying a certain extent of space. Faraday on the other hand explains the ordinary inductive action by the supposition of a system of actions among the particles of the matter that lies beween the charged surfaces. In this way the dielectric becomes of essential importance in explanation of the entire phenomena of induction. Further, he regards the peculiar inductive state of the particles of the dielectric as an essential condition of charge itself. It will be evident that if these suppositions are true, the dielectric has an active influence of the most essential kind in all the actions of conduction and discharge. So that, in short, whenever we have charge or discharge in any form and in any intensity, the action of the dielectric is supposed to enter as an essential condition of the phenomena. These ideas are simply suggested and powerfully supported by experiment; and if they have the disadvantage of vagueness, it is because they are not more definite than sound reasoning from existing experimental data would warrant. They have the great merit of linking together the varied phenomena of electricity in a harmonious whole, and of thus fulfilling, to a certain extent, the principal office of a Theory. For a statement of these theoretical points, and for a description of the specific properties of dielectrics, see INDUCTION.

Difference: Differences: Calculus of

Finite Differences. The simple meaning of the arithmetical word *difference*,—viz.: the excess of a larger quantity over a smaller—is virtually lost, in reference to the same term, when used in the higher branches of the Mathematical Analysis. We shall attempt very briefly to convey some notion of its nature and comprehensiveness, as thus employed. Suppose that any series of numbers exists, whose terms, however irregular they may appear, are held together by a certain law,—the character of that law will evince itself by the fact, that if the differences of the numbers be first taken, the differences of these differences next, then the differences of these second differences, &c., we shall arrive at a term or order of differences whose difference is 0. For instance, take the following numbers, and find the successive differences:—

Series.............	43	47	53	61	71	83, &c.
First difference.....	4	6	8	10	12	
Second difference..	2	2	2	2		
Third difference...	0	0	0			

Now, the nature of the law of the series written down above is expressed by the fact that its *third* differences are *Zero:* and the terms of any series or set of numbers may be calculated, if we know the first term and a few characteristic facts regarding their differences. In the series just written down, it would suffice to know the terms 43, 4, 2, and 0, to enable any one to write out the whole apparently irregular series. It is on the ground of this principle that Mr. Babbage's Machine, and all others of a like kind, can compute extensive tables, such as tables of Logarithms (see ENGINE, CALCULATING). — The whole of this subject has grown into the *Calculus of Finite Differences*, an important and elaborate branch of the modern Analysis; first proposed by Dr. Brook Taylor, under the name of the *Method of Increments.* Of course, it is impossible to enter here, on consideration of the processes of this Calculus; but a few remarks are needed to enable the student to distinguish it clearly from that true Transcendental Analysis —viz. the method of *Infinitesimals.* It is not possible to regard the method of Finite differences as at all related to the great conception of Leibnitz. The fundamental conception of Taylor is the following—viz.: it is required to detect what changes will pass on a function, if to its variable *a definite increment be added.* Now, this definite increment can differ from the original variable only in relative magnitude: it is a quantity altogether *of the same sort as the variable;* and it is difficult to see in what way—by taking *it* as an intermediate quantity—we can establish equations that define the variable, more easily, than might be done by working with the variable itself. The immense facility, and therefore the immense advantage of the Transcendental Calculus, lies in the very fact, that the increments employed are quantities *differing in nature from the original variable*—that, in fact, they are *infinitesimals;* and therefore that we can neglect higher powers of them in comparison with lower powers, and higher differentials in comparison with lower ones, without, as has been shown (see CALCULUS), endangering the accuracy of the equation. Lagrange has pointed out this radical distinction, with the power usual to him: and he has shown that the pretended analogy sometimes asserted between the *Calculus of Differences* and the *Infinitesimal Calculus*, has therefore no existence; and that formulæ of the former calculus can never furnish, *as mere particular cases*, formulæ belonging to the latter, seeing that their nature is essentially distinct.—But though the Calculus of Finite Differences has thus no pretension to take rank with the Infinitesimal Calculus, and is comparatively powerless in the field of the triumphs of the latter, it is impossible to overlook its grasp and importance as a branch of pure Algebra. Of the whole doctrine of *Series*, it is undisputed Master; and it has given birth to the superb *Theory of Generating Functions* of Laplace. We also owe to it that valuable theory of *Periodic and Discontinuous Functions*, first introduced by Euler, and afterwards so greatly advanced by Fourier. See FUNCTIONS.

Differential; Derivative; Fluxion. See CALCULUS.

Diffraction of Light: Inflexion. The very remarkable and puzzling phenomena designated by the foregoing terms, have been known since the time of Grimaldi, who also made the earliest attempt to explain them, in 1665. They consist of curious appearances connected with the shadows of opaque objects, when these are placed within rays diverging from a simple line or point. For instance, if a small lens or burning glass of short focus be fitted into a hole in a shutter, it will concentrate the light passing through it, in a bright point; and from that point rays will diverge through the dark room. An opaque object placed within these rays will, of course, cast a shadow, on any screen; but instead of the *real* shadow corresponding with what may be termed the *geometrical* one, its edge will be found quite indefinite,—certain curious tints in the form of *fringes* of colour, or of alternating dark and light, extending to some distance and running parallel to that edge. These curious appearances were attributed by Newton (who held that theory of light known as the *Theory of Emission*,) to certain molecular attractions and repulsions between the *matter* of the *rays*, and the edge of the opaque body: but, irrespective of minor, although decisive objections, the following classical experiment of Fresnel's—by which appearances *of the same nature*, are produced under circumstances totally different—gave this explanation its death-blow. Having arranged a lens, A, so that rays of light concentrated at its focus, might thence diverge into a dark room, he placed opposite to it two plane metallic

rors M and M′, in the way shown in fig. 1. ese mirrors, on receiving the rays F G and F H, ect them downwards along G B and H B, so t they meet and concentrate in a *point*, or rather

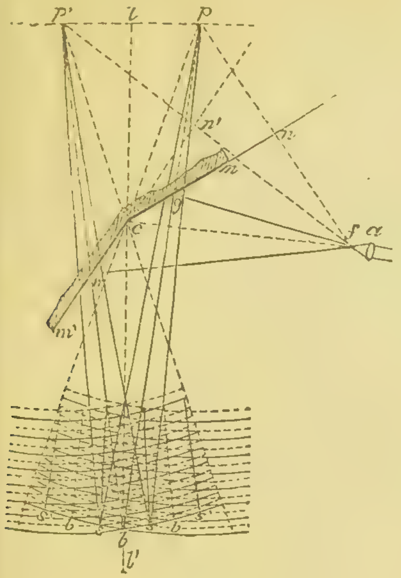


Fig. 1.

ne of light parallel to the intersection of the rrors. Now, the phenomenon, which is tho-ghly remarkable, lies on each side of the ght band B. At a short distance, S S, there a *dark band;* beyond which we find an-er bright band B′ B′; beyond that again a ond dark band S′ S′, and so on in regular alter-ion to a considerable distance from the central d B. Should one of these mirrors be obscured, this fringing vanishes; and the entire space is rly covered by a diffused light;—a striking experimental confirmation of the apparently paradoxical maxim, that *light added to light may produce darkness.* But as this curious fringing resembles, in almost everything, the phenomena attending the edge of the geometrical shadow of an opaque body, and as no hypothesis of mole-cular attractions and repulsions can explain the experiment of Fresnel, it is clear that such hypotheses must be abandoned in the former case also.—Both these experiments are ex-tremely simple, and may readily be performed by the *Amateur.* One explanatory remark, however, is necessary. To obtain alternate *dark* and *bright* fringes, the light concentrated by the lens, must be *homogeneous* light, *i.e.* it must consist of rays of one kind only. The ordinary white light of the sun is not simple light, but a sheaf of seven different elementary rays, only *one* of which—*e.g.* the *red* ray—must be employed, if the foregoing *pure* effect is desired. Should the usual, or composite beam be made use of, the results will be found much more complex, although likewise much more brilliant. By the process which originates the fringes, the white beam (*see below*) is *decom-posed;* and instead of alternate dark and light bands, a succession of fine *spectra* appears on each side of the central band B, in Fresnel's ex-periment—a similar series fading away from the edge of the shadow, in the other case. Nothing can well exceed the beauty of the appearances frequently evolved by this decomposition. Take, for instance, the case so profoundly analyzed by Frauenhofer. If a ray of ordinary solar light be transmitted through a *narrow slit,* and examined by a telescope adjusted to distinct vision of the slit, the phenomena indicated by fig. 2, immediately declare themselves. The slit A appears in the middle, of a white light,

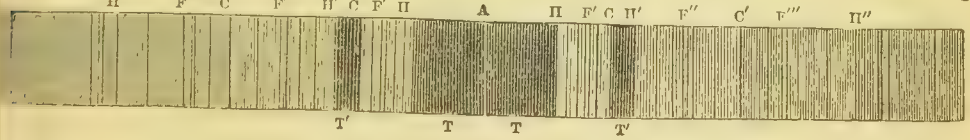


Fig. 2.

with edges perfectly defined and *clean.* An ure or black band, T T′, lies on each side of slit. Beyond this, symmetrically disposed, find a fine and complete *spectrum,* H C,—the et being at H, and the *red* at C. Another t interval succeeds; after which we have a ession of spectra, H′ C′, &c.—all, like the first, ing the violet nearest the slit; only, the *red* he *second* spectrum overlaps the *violet* of the *d;* and so on with the *fourth,* until the phe-ienon fades quite away. Those special *dark* s of *Frauenhofer* (*q.v.*) are found in all *spectra diffraction;* but their relative distances are red. The following appears to be a law;—cting any distinct line F′, its distance F″ A in second spectrum, from the image of the slit s double of F′ A; and F‴ A is triple of F′ A; &c.—These and all other peculiarities have been fully explained by the general theory whose outlines we shall now endeavour, under a few heads, to unfold.

(1.) *Diffraction; the General Principle on which it Depends.*—The *Undulatory* or *Wave Theory of Light,* and that special consequence of it—the principle of *Interference,* will be treated at large under several appropriate articles. Nevertheless, the reader must at present have clearly in view the nature of that Theory, as well as of the principle of Interference. Accord-ing to accepted opinions, Light is not propagated by particles emitted from the luminous body, at extravagant speed; but—as *Sound* is propagated—by waves excited in a highly elastic medium, by the luminiferous body. To use an illustra-

tion, more palpable and expressive perhaps, than rigorously *apt*,—unless the surface of this supposed elastic ethereal ocean, be agitated, or in a *wave* state, it does not communicate to our human organs the sensation of light. Let that surface be smooth—from whatever cause—and, to our eye, there is darkness. Now, the surface of an ocean may be smooth from two causes: either because it is agitated by no system of waves whatsoever; or because two systems of waves course along it, so related, that the *crests* of the one system always coincide with or *fill up* the *troughs* of the other. Imagine two systems of waves propagated through an elastic medium from two different points. If these points are so placed, that lights from them reach a third point, either in the same time, or at times differing from each other by one length of an undulation, or two lengths, or three lengths, &c.;—then, two wave crests must always reach that third point at the same moment, and the result must be *double* light: but if their relations in space are such, that lights from them shall reach the said point, at times differing by one-half an undulation, or by an undulation and a-half, or two undulations and a-half, &c.,—their *crests* and *troughs* will always arrive there together, and the *two lights must produce darkness.* In the latter case, the two *lights* or *systems of waves*, would evidently *interfere with* or *neutralize* each other; and their luminiferous energy would be *nil.*—Having caught firm hold of these principles, let us return to Fresnel's experiment, and the diagram, fig. 1. The complexity arising from intermediate action by the two mirrors may be got rid of, in this wise: their acts of reflexion have no other meaning than that they send light to the spaces in front of them, exactly as it would have been sent, had it been propagated by the two points P and P′—the two images of the bright focal point F. Suppose then, waves of light propagated downwards from P and P′, and let the *crests* of these waves be represented by the dark circular lines below, while their *troughs* are indicated by the intermediate dotted lines. Bisecting P P′ in H, and drawing L L′ perpendicular to P P′, we shall have a line L L′, along which, it is very clear that *crest* must always meet *crest*, and *trough* meet *trough*, because every point in it, is equidistant from P and P′. Through the point B therefore a *bright band* must pass. Take, on the other hand, a point, S, on either side of B, where, as marked in the diagram, *crest* meets *trough*,—in other words, where, P S—P′ S, is equal to one-half of an undulation,—that will mark a band, or fringe of *interference* or *neutralization*; or a *dark fringe.* Inspection of the diagram will now readily convince the intelligent reader, that there must be an alternation of dark and light fringes on either side of B; and that they must diminish in distinctness, if they are received on a plane surface.—The foregoing clearly understood, the special problems of *diffraction* receive an easy solution.

(2.) *Diffraction; Special Phenomena of, and Special Explanation.*—So soon as the reader has formed a distinct conception of the mode of propagation by *waves*, and of the notion of *Interference*, he will discern that disturbances in what may be termed the rectilineal propagation of light, may readily occur when an obstacle of any kind is placed in the way of a wave. A slight consideration indeed of the probable consequences of the interposition of an obstacle in front of a wave propagating through a pool of water, can scarcely fail to suggest the reflection which first revealed to Dr. Thomas Young the origin of the phenomena of *Diffraction.* Instead, however, of resting on merely general considerations, we shall at once expose the principle that led Fresnel to a sufficient and comprehensive theory of the details of the subject. "*The vibration or oscillation existing at any point of a wave, may*—according to the French geometer—*be considered as the resultant or the sum of all the motions propagated towards it, by every point of the wave in any antecedent condition.*" In other words, if F be the luminous point, X Z X′ a section of the wave at any moment,—the oscillation or velocity of the point P, may be supposed due to the motions, or velocities propagated towards it by every point in the wave X Z X′. This fundamental proposition is self-evident. It may seem a complex and artificial statement of a simple truth; but it is the mode of putting the case which best permits of the actual consequences being deduced. Further: Fresnel saw that all consideration of the effect on P, of points in the wave, *not in the immediate neighbourhood* of Z, might be dismissed at once. Any set of remote points—for instance A, M, C, &c., may always be taken so, that their successive distances from P differ by *half an undulation:* on which account the motion produced by them, or the waves transmitted by them to P, will evidently neutralize each other at P, or produce no positive wave. The actual oscillation of P may therefore, with rigorous accuracy, be referred solely to the oscillation of the point Z, and of points in its immediate vicinity: and the practical question is, in what manner will these points—under various circumstances—act upon P and its neighbourhood? The circumstances with which we are now concerned, are the inter-

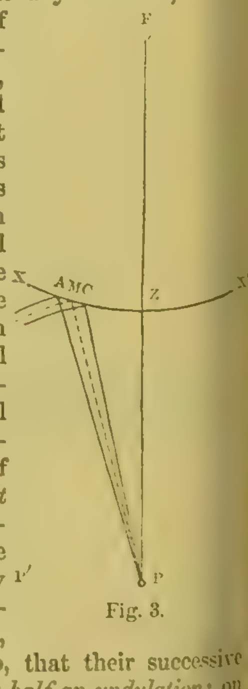


Fig. 3.

ition of obstacles in the way of the wave, or cutting off, in various manners, of the action certain numbers of the points near Z, upon its near P. Let us treat the phenomena, by anging them into classes. — I. Imagine a een, V Z, interposed, so that the whole of one of the wave X Z X′ be intercepted by it. It is plain that upon any distant point P′, no effect will thus be produced, because *its* oscillations must, as we have seen, be determined by the oscillations of the points close to the unaffected part of the wave at Z′. Fringes, or whatever else shall be developed by the interposition of the obstacle or screen V Z, cannot therefore extend far from P:—but what must be the condition of P″, a point quite near P? This will be readily understood by aid of our next figure. Suppose X Z X′, a small portion of the wave propagated from , — a portion so small all points of it act in producing the oscillaof P′; in other words, in illuminating P.

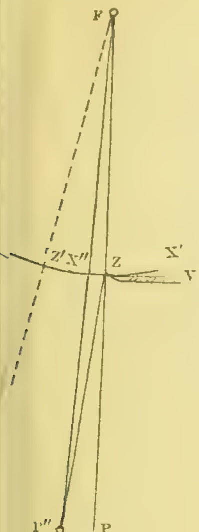


Fig. 4.

Let the total action of one-half of it be called 1; then the whole light received by P, or the total action of both sides of the wave, will be 2. Neglect at present the action of the side X Z′: and around P, as a centre, with the radius P Z, describe an arc Z K. Next take a set of points B, S, B′, S′, &c., so placed that B T, S T′, B′ T″, S′ T″, &c., shall represent the lengths of half an undulation, a whole undulation, an undulation and a-half, two undulations, &c. Two propositions will be accepted at once:—*first*, that Z B must be greater than B S; B S greater than S B′; S B′ greater than B′ S′, and so on:—and, *secondly*, that while the action upon P, of the points in every separate arc, Z B, B S, S B′, B′ S′, &c., may onsidered as *according*, the action of the Z B must be *discordant* with that of the B S; B S discordant with the arc S B′; discordant with B′ S′, &c.;—the arcs next other will be in *discord*, while alternate or those separate from each other by an mediate arc, will be in *accordance*, in so as their action on P is concerned. More definitely, if one set of these *according arcs* transmits to P the *crest* of a wave, the other set will, at the same moment, transmit a *trough*.—The total effect on P, or the condition of P with regard to light, must, if the foregoing consideration be correct, evidently depend on the *difference* of the sums of the actions of the two sets of accordant arcs: *i.e.* one set of actions—let us say, a wave's *crest*, being due to Z B, S B′, S′ B″, &c., and another set of actions, or a *trough*, to B S, B′ S′, B″ S″, &c., the actual oscillation of P will depend on the difference of the two: if the difference is *o*, P will be at rest: if the former set are more powerful, P will be in *crest*, and if the latter, P will be in *trough*. But since, as we have seen, these arcs are unequal in length—their lengths diminishing as they recede from Z—although the sum of Z B, S B′, &c., will be greater than Z B alone, the entire sum or the amount of both series, must be less than Z B alone:—in other words, the action of the entire arc, which we have valued at 1, *must be less than the action of the partial arc* Z B. The student will now be prepared to accept the following conclusions — (1.) Should a screen intercept the whole of the semi-undulation Z X′, the point P would be acted on, or illuminated by the semi-wave X Z′,—*i.e.* it would receive the entire light developed as 1.—(2.) If the edge of a screen is pushed inwards as far as B, the point P will receive *light* 1, and the light also from Z B, which, as we have seen, is *more than* 1. The entire light falling on P will therefore be *more than* 2; as P *will seem brighter than if the screen did not exist*.—(3.) If the screen is at S, intercepting S X′, the light received by P will be 1 + (Z B—B S),—the action of B S being *negative* or *discordant;* so that although the screen is farther from it than before, the point P will appear *darker*. An effect precisely opposite must follow when the screen is removed to B′:—in short, by gradually withdrawing the screen, by definite intervals or steps, the point P may be endowed alternately with a greater or less amount of light, than it would have if the screen did not exist. But if the screen be supposed immovable, and the point P to change its distance, the results will be equivalent: from which it follows, that beyond the geometrical shadow of any opaque body or screen, there must always be found a series of alternating dark and bright lines or fringes, parallel to the edge of that body;—which fringes are the first and simplest class of the phenomena of diffraction.—It will be observed that the explanation now given proceeds on the supposition, that we are dealing throughout with the same sort of undulations, or that the light diffracted is homogeneous. If the light be not homogeneous, but, let us say, the ordinary sheaf of rays constituting the solar beam, other results must occur. The position of the alternating dark and bright points, with regard to the point P, clearly de-

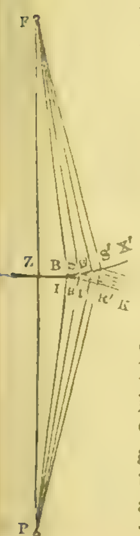


Fig. 5.

pending on the length of the undulation, it is manifest that undulations of different lengths will give rise *to separate series of dark and bright points, at different distances from* P. In other words, the compound solar ray will be *decomposed by diffraction;* and instead of an alternation of simple dark and bright bands, we shall necessarily have a *succession of spectra.* This is the feature which bestows on all this class of phenomena so peculiar a brilliancy: its cause having now been indicated, we cannot again refer to it, because of the limits of this article. — II. The next peculiar or critical phenomenon due to *diffraction* consists in this: *if a narrow opaque body be placed within a cone of rays, issuing from a bright point, a set of fringes will be formed, extending from either side* WITHIN *the limits of the geometrical or true shadow.* We shall easily reach the cause of this, by aid of the subjoined figure.—Let F, as before, be the bright point or focus of the rays, T T′ the opaque screen, G G′ the limits of the shadow, and X T T′ X′ a section or front of the wave proceeding from F; the question is, what will occur, or what motion will be propagated to P, any point within G G? There cannot be a doubt that the phenomenon is due to some operation akin to that already analyzed; for if one side of the wave T′ X′ be intercepted by another screen, the dark and white bands, or the fringes, will at once disappear. How then is an *interference* produced by the action of one side of the wave upon the other side of it, in as far as the point P is concerned?—On each side of the wave, take the points A, B, C, D, and A′, B′, C′ D′, so that the lines P A, P B, P C, P D, may differ from each other by half the length of an undulation, and that the same may be the case with P A′, P B′, P C′, &c. Let us now inquire what line may represent the *direction* along which all the motions propagated by the arcs T A, A B, B C, &c., might, if *compounded*, be supposed to act on P. Since the arc T A is greater than the arc A B, and since it also lies more *directly* or less obliquely in regard of P, it is clear that its positive effect will quite overbalance the negative effect of A B; so that their combined effect must be positive, or what is the same thing, they must together send a definite quantity of light to P. Similar considerations will show — since, viz.: the other arcs go on diminishing in size and increasing in obliquity—that the resultant of the action of all the arcs taken together must lie along such a line as P R within T A, and that the portion of light sent to P, along P R, will

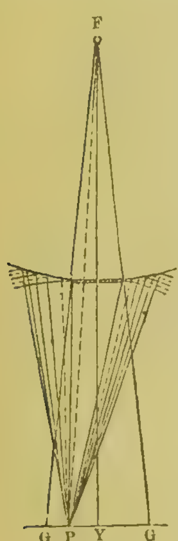


Fig. 6.

depend on the breadth of the screen, its distance from the luminous point, and the position of the point P within the geometrical shadow. Exactly in the same way, the total action of the arc T′ X′ may be represented by an undulation, transmitted from T′ A′ along the resultant P R′; so that the condition of P, as to undulation, will depend on the effect, on each other, of the motions transmitted along P R and P R′. (We omit, as too abstruse for this paper, any discussion of the *intensity* of the vibration transmitted along these resultants.) Thus much understood, the necessity of fringes, or of dark and bright bands, becomes readily apparent. *First,* at the point Y in the axis of the shadow, the lines P R, P R′— which then are Y R Y R′ — being equal, the transmitted undulation must be in *the same phase; i.e.* the *central* band of the shadow must be a *bright band. Secondly,* as P diverges from Y, towards either side, the lines P R, P R′ will become more and more unequal; at length a position will be reached by P at which they differ by *half an undulation;* in other words, the position of a *dark band.* A dark band therefore will be found on either side of the central bright band. *Thirdly,* as P diverges still farther, the difference of P R and P R′ will further increase, and must soon reach the *length of a whole undulation;* in other words, next to the aforesaid dark band on either side, a bright band or point must be found: and so on alternately, just as these phenomena actually appear.—It is a sufficiently singular consequence of this peculiar mode of *diffraction,* that if a small opaque circular disc be placed within a cone of rays, the centre of the shadow of the disc will be illuminated, as if the disc had been transparent.—III. Very remarkable phenomena, due to the same class of causes, occur when a beam of light passes through a narrow aperture, such as B B′. Our limits oblige us to leave the student, as to the operations in question, simply with the annexed figure; but the deductions already laid before him, furnish an easy clue. It must suffice that we state successively what the results are. The geometrical representation of the aperture on the screen ought, of course, to be bright: but there are *fringes exterior* to it, or penetrating within the dark space; and *interior fringes,* or fringes that variegate, and in so far obscure the bright space. (1.) When the aperture is extremely narrow, these exterior fringes are discerned; but as it often happens that

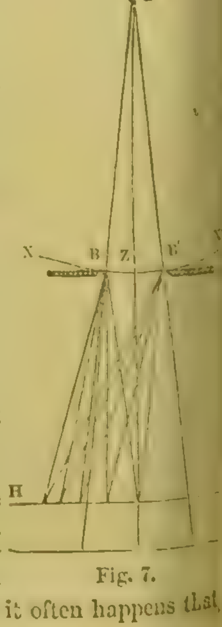


Fig. 7.

ae immediate neighbourhood of the aperture, get mixed up with the interior system, it necessary, in order to observe them purely, the shadow be received on a screen at a iderable distance. Their laws, which are ous, were first deduced from Fresnel's fundaal doctrine, by Biot and Pouillet. (2.) It ufficiently evident, that in all such cases, ior fringes must be produced; and the ry is beautifully verified by an experiment e reverse of the one alluded to in the pre- section—viz.: that a beam of light passthrough a narrow circular aperture, shows *rk spot in the centre of its projection on a n.*—In the cases now mentioned, as in s already alluded to, the purity of the ts depends on the homogeneity of the light oyed. In all other circumstances, coloured es and even spectra, often of great brilliancy, produced.—IV. Many interesting questions ected with diffraction have not been touched is article; for instance, the inquiry as to hyperbolic paths of these fringes in space, traced backwards from the edge of the ue body; and still farther, the case of interce by *reflexion*, or those phenomena of r produced by striæ on polished surfaces, hich the discoveries and researches of Sir d Brewster have bestowed so fresh and iar an interest.—The space that remains to must be occupied by a brief reference to a of those brilliant results so carefully ed by Frauenhofer—the effects of diffraction the ray passes through a delicate *net-work.* ng the simplest case of a system of narrow ngs, such as below, it is clear that various

Fig. 8.

sets of openings may be made out of it; for instance, two parallel sets crossed, will form an extremely delicate set of square holes, &c., &c. If the parallel system be employed, and the light which has passed through amined by a telescope, the eye of the spec-

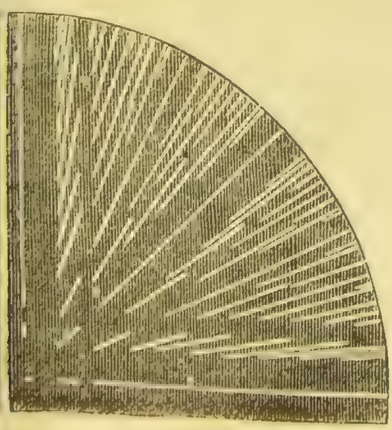


Fig. 9.

is dazzled by that series of magnificent spectra of diffraction already described (page 174). If, by crossing two parallel systems, a set of square holes is produced, the spectator beholds a circular figure, of which the preceding diagram is a quadrant (fig. 9), in which the white rectangular figures are spectra, more or less elongated, but exceeding brilliant. If, again, a set of small round apertures, close on each other, be employed, the subjoined still more remarkable appearance bursts on one's sight—

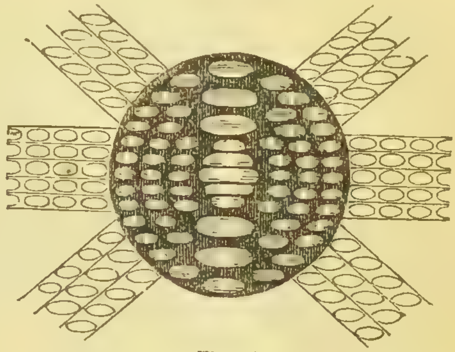


Fig. 10.

each oval space being, as before, a complete and dazzling spectrum. But experiments of this sort may be infinitely varied; and they never fail to bring new pleasures to the inquirer.—We refer the student further to articles on *thin* and *thick plates*,—see PLATES.

(3.) *Diffraction; Exemplified in Natural Phenomena.*—Certain practical and natural results of diffraction are too important to be passed without notice.—I. Several curious appearances in the foci of telescopes are attributable to this cause. (1.) If the telescopic image of a fixed star is examined by a magnifying power exceeding 200, we discern first a circular disc with perfectly clean circumference; and secondly, around this disc a system of dark and bright rings, with slightly coloured edges. Diminish the opening of the object-glass, by a diaphragm in front of it, and the image of the star augments until it assumes the aspect of a planet—the rings enlarging at the same time, and becoming more distinctly coloured, manifesting well pronounced shades of white, red, black, and blue. Arago observed, further, that if the eye-piece be gradually *pushed in*, the disc darkens at its centre, becoming at last quite black: as the eye-piece is pushed farther inwards, the black centre enlarges; then a luminous point appears at its centre; and this bright point, enlarging in its turn, comes to manifest a central dark point, which also goes through similar changes. Also, if the eye-piece is fixed for a short time at a position showing an image with a dark centre, and if this image be closely inspected, a brilliant point will ever and anon burst out within the obscure disc, and then suddenly disappear. This latter phenomenon, however, occurs only in the case of *scintillating* stars. See SCINTILLATION.—(2.) Sir John Herschel has, by ingeni-

ous experiments, added considerably to our knowledge of this curious subject: he placed diaphragms of *various shapes* in front of the object-glass, and obtained thereby the following curious results. *First*, with a diaphragm or opening shaped as an equilateral triangle, the annexed figure gives the image the star: *i.e.* the

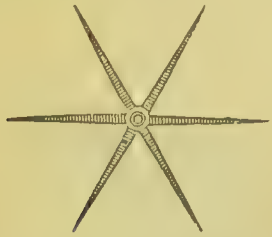


Fig. 11.

disc of the star surrounded by a dark ring, and six straight rays, thin, but sufficiently brilliant. Three of these rays point to the angles of the triangular diaphragm; three to the middles of its sides. If the eye-piece be slightly pushed in, the figure changes into this:—

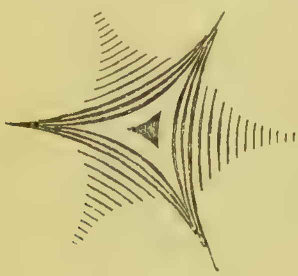


Fig. 12.

Secondly, an aperture formed by the interval between two concentric squares, gives the subjoined curious appearance:—

Fig. 13.

And, *thirdly*, from an assemblage of small equilateral triangles regularly arranged, an image

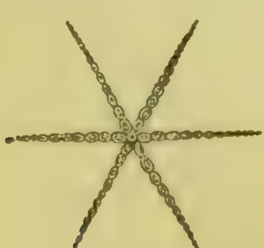


Fig. 14.

like that of fig. 14 is observed,—an image composed of a series of circular discs, arranged along six symmetrical and equal rays, which unfold the brilliant colours of the spectrum.—The whole of these curious phenomena, and many others like them, issue from interference,—the rays being diffracted by the edges of the diaphragm. — II. A very large class of the phenomena of optical meteorology, are referable to *Diffraction*:—we shall specify only *Crowns* or *Coronæ*, and *Anthelia*; phenomena already *described* under the proper articles. — These *coronæ* or *concentric coloured circles*, surrounding the sun and moon, present, in regard of their colouring, the characteristic of all phenomena of diffraction, viz.: the *exterior* part of the spectrum is *red*; the *interior*, *violet*. The diameters of the several circles increase, as the numbers 1, 2, 3, 4, &c.,—a law discerned, as necessary, by Young, and recently confirmed, by delicate observation, by M. Delezenne, of Lille. The doctrine of *interference* easily explains this law. The production of *coronæ* may be illustrated experimentally in the following easy way. Cover a piece of glass with dust of *lycopodium*, and look through it at the sun, the moon, or even the flame of a candle,—superb *coronæ* will immediately appear. Now, this is the exact process in nature:—natural coronæ being formed whenever we look at these luminaries through a great number of small spheres or globules of water, of a uniform or nearly uniform diameter, —globules placed between us and the sun by the agency of a peculiar cloud. The apparently paradoxical optical principle which these globules bring into operation, will now be readily understood: it is simply this; if, slightly beyond the line joining the eye and a luminous point, a small opaque body be placed, its effect will be precisely the same, as that of a similar *opening* illumined by the incident light; so that whatever the degree of the body's apparent opacity, in that same degree it produces an actual illumination. This indubitable and fertile principle readily explains a singular phenomenon described by Necker of Geneva. If the sun rises behind a hill covered with trees or brushwood, a spectator in the shadow of the hill, and *quite near the line* of the solar rays, sees all the small branches projected on the sky, not opaque or black, but, on the contrary, white, silvery, and brilliant, as if the vegetation were really composed of silver. The same theory of Interference also explains the aspect of those globules floating in the air when a ray of the sun penetrates a dark room; likewise the colouring of spider's threads, or thin metallic wires, &c. *Coronæ* really exist around the sun much more frequently than one imagines; but they cannot be seen in general because of the dazzling light of that orb. They are most easily found on looking at the image of the sun in a mirror blackened on one side. Every transparent cloud, except the *cirrus* and *cirro-stratus*, produces them; and the fogs form

in the valleys over night, and rising to-ds mid-day to the tops of mountains, yield earances of this kind of admirable brilliancy. Anthelia, as already described, are colours around the shadow of one's head, &c., when shadow falls on a cloud, on the grass, a of corn, or any other surface covered with , &c. Bouguer first observed this among Cordilleras. Lamartine, on the summit of anon, saw his head surrounded by a superb ry; the same thing has been seen elsewhere. le *Coronæ* are produced by the agency of *smitted* light, *Anthelia* are due to *reflected*, a *retrograde transmission*. But the theory e same: it is easy to pass from the pheno- of transmission to those of reflection or grade illumination. The same formulæ of *ference* apply in both cases.

iffraction, Dynamical Theory of. leading phenomena of *diffraction*, as con-d with the general idea of the undulatory ry, are explained above. But many other, much more difficult problems, start up on a consideration of this curious subject. For nce, that fundamental one,—in what man-ccording to what mathematical and dyna- laws, is each wave of a series broken up passes the edge of a diffracting body, or upon a small aperture? How shall we sent the exact condition of the secondary issuing out of it, and thus come to a com- understanding of all the details of the illu- on in front of that aperture? The inquiry een pursued in all its generality, in a able and very pregnant memoir by Pro- Stokes, published in the ninth volume of *ransactions of the Cambridge Philosophical* ,—a memoir containing, besides, exceed- interesting contributions to general dyna- It is not within reach of this cyclopædia more than make reference to such memoir; ere is one part of Mr. Stokes' investiga- earing so closely on an important question ence to the general theory of light, and resting at the present moment, that it ncumbent to treat it in some detail.—The who has at all looked into modern specu- regarding light, does not require to be ed that the vibrations of the molecules of wave are supposed to take place *trans-* to the length of the wave, or to its ng course: they are not like waves of out like the vibrations of a musical string n tension. In the same manner, in the a ray of *common* or *unpolarized* ray of hese vibrations take place at the same *all transverse* directions—just as when a moved from its repose, not by one pull it by a great number of pulls in all direc- In the case, on the other hand, of what d a *plane-polarized ray*, these vibrations ce in one transverse direction only—as tendent string is made to vibrate by being pulled aside in one direction only. The vibration of the molecules in a plane-polarized ray must thus be all parallel to some one fixed transverse line; and the question that remains is, to what line are they parallel? Are they parallel to the plane of polarization itself, or perpendicular to it? Now this is no mere curious question, but, on the other hand, closely connected with our notions of the habitudes of the light-ether under different circumstances. But the most eminent physicists differ in their conclusions—Fresnel and Cauchy holding ultimately by the theory of perpendicular vibrations, while MacCullagh maintained the opposite hypothesis; nor was any effort made to discover an *experimentum crucis*, previous to these researches on diffraction by Mr. Stokes. His method was simple. Suppose that a plane-polarized ray reaches a grating in a direction perpendicular to the plane of the grating, it is not difficult to discover the plane of polarization of the diffracted ray; and the plane of polarization of the incident ray is of course also known. Now the following theorem holds,—if the angles between the plane of polarization of the diffracted rays and the lines of the grating be less than the angle between the same lines and the plane of polarization of the incident ray, then the vibrations must be *parallel* to the plane of polarization; if the contrary obtains, the vibrations must be *perpendicular* to that plane. Mr. Stokes performed the experiment by aid of a fine glass grating, on which parallel lines were ruled of 1,300 to the inch; and the issue seemed decisive in favour of the latter hypothesis, or Fresnel's. But doubts so grave have since then supervened, that the important question must still be considered unsettled. One of the most experienced physicists, M. Holzmann, resumed the inquiry in 1856, on the ground of Mr. Stokes' theory—performing the experiment, also, with a slight modification. He did not use a grating made by diamond lines on glass—to which he objects, as having irregular edges—but one of Schwerdt's lamp-black gratings; and he maintains, as a determinate result, that the *vibration of light occurs in the plane of polarization*. It is the earnest wish of all physicists that Mr. Stokes should find leisure for resumption of the investigation. If the two conclusions are incapable of being harmonized, at least we may expect to discover the causes of their conflict. Nor can the unsettled state of such a question be regarded otherwise than as a serious blot on the undulatory theory, as it at present stands.

Diffusion of Light and of Heat; Diffusive Power. On analyzing what is termed the *reflecting* power of bodies, with regard to Light, it is found that a very important distinction requires to be made. A total or pure reflection of *light*, for instance, from the surface of any body, would convey to the onlooker no conception whatever, either of the size or colour

of the reflecting substance; the only result would be, a perfect presentation of the source of the incident beam,—its direction from the eye being all that would be altered by the reflection. But by all mirrors—even the most perfectly polished, more is done than that;—the eye which notices the reflexion, notices also the *form* of the mirror, and in some degree its *colour* also. Besides the pure geometrical reflexion, therefore, the incident beam undergoes other two distinct operations. *First*, there is an *irregular reflexion* in all directions, from the inequalities of the surface of the mirror, dispersing incident light on every side, and revealing the mirror's form; and *secondly*, there is a *diffusive power* which disperses also in all directions a portion of the incident light *after having altered it*, or after having impressed upon it that *special colouring* depending on the nature of the body. In article COLOURS, reference has been made to the difficulties connected with this question of the natural colours of bodies: it is sufficient, therefore, to give here a simple definition of the term *diffusion of light*.—Now, what occurs with regard to LIGHT, happens also with reference to HEAT: but certainly the difficulty of determining exactly how much of the efficacy which is not absorbed is dispersed by irregular reflexion, and how much by such *diffusion*, is not diminished in the latter case. The endeavour to reach such a determination has occupied the well known Melloni. It is quite established, for instance, that a white matted surface reflects much more (or *absorbs* much less) than a blacked surface: but the question is, in what manner is the light reflected sent away? Melloni conceives that there is at once a large irregular reflexion, and a peculiarity in the amount of that irregular reflexion depending on the nature of the incident ray of heat—in other words, a true *diffusive power*:—it requires, perhaps, to confirm his idea, that the nature of the dispersed rays be minutely examined, so that it be determined whether they correspond in nature with the incident rays, or have acquired new properties?—Melloni further speaks of a supposed diffusion of heat by transmission, that is, when the rays emerge through an unpolished surface, when they are issuing from a *diathermanous* or *thermanizing* substance. It does not appear, however, that the rays issuing in this way from a *rough* surface of rock salt or glass differ from the rays issuing from polished surfaces of these substances: *i.e.*, those issuing from a polished surface of rock salt have all the characters of the source from which they emanated, while those issuing from glass are merely *thermanized*, as they ought to be: no change, therefore, is impressed on them by the rough surface, except *change of direction*: but the astonishing fact is, that a rough face of rock salt blacked by smoke, *does not impress on the emergent rays any deviation whatever in point of direction*.—The student is especially referred to a remarkable memoir by Knoblauch, in Taylor's *Scientific Memoirs*. See also RADIANT HEAT; RADIATION.

Diffusion of Liquids, Gases, &c. A term applied to a curious phenomenon, discovered first by Priestley, brought again under notice by Dalton, and of recent years very fully investigated by Dr. Thomas Graham. It is virtually this:—let volumes of two liquids or two gases, having no chemical relationship whatsoever, be brought into contact with each other (if only by a narrow connecting tube) there will immediately ensue a flow or translation of the two into each other, until the two separate volumes become interfused,—the distribution or quantities of the two separate elements depending on their respective specific gravities. The student is especially referred to Graham's remarkable Memoirs in the *Transactions of the Royal Society of London*, republished in the *Philosophical Magazine* during a few years prior to 1851.—As to *Liquids*, Adolf Fick of Zurich has recently made an interesting attempt to develop a fundamental law. (*Pogg. Annalen*, vol. cxciv). Proceeding on the supposition that the law for the diffusion of a salt in its solvent must be identical with that according to which the diffusion of heat in a conducting body takes place—the law on which Fourier founded his celebrated Theory of Heat, and which Ohm applied in determining the diffusion of electricity in a conductor—he finds that the transfer of salt and water occurring in a unit of time between two elements of the same salt, must, *cæteris paribus*, "be directly proportional to the difference of concentration and inversely proportional to the distance of the elements from one another." Fick carries out his fundamental idea by aid of ingenious mathematical processes, which promise to co-ordinate at least the larger number of Graham's facts.—In reference to *Gases*, Graham conceives that the ratio of dispersion depends on the specific gravity of the gas—the lightest travelling fastest—the rate of diffusion being exactly as the square root of the density of the gas. Inquiries as to this point were instituted some years ago by Bunsen in conjunction with Professor Stegmann. The former has more recently resumed the subject, employing an apparatus and manipulation alike novel and ingenious. He thinks he has established that the pores of the gypsum diaphragms generally used to separate the gases in experiments, do not, as usually supposed, act as fine openings in thin plates, or as gratings, but as a system of capillary tubes: and he negatives the opinion formerly received, "that the volumes of two diffused gases, when interfused, are inversely as the square roots of the densities." The inquiry, however, is confessedly in an unfinished state. The student is referred to Bunsen's *Gasometry*, translated by Mr. Roscoe.

igester (Papin's). An instrument ori-lly employed to extract, as far as possible, nutritious matter from bones. It is not for however, that we mention it here. It sists of a strong iron vessel in which water be boiled, and the lid of which is so fixed n as to be nearly as capable of resisting force within as the rest of the vessel is. Heat g applied after the vessel is filled, steam is ed as usual. When water boils in ordinary mstances, the pressure of steam never rises e 14·7 lbs. per square inch (the atmospheric ure), nor the temperature of either the r or steam above 212° Fah. In the ster, if the lid be so constructed as to open r a little less than would burst the vessel— lbs. of extra pressure per square inch—we raise the temperature of the water and the erature and pressure of the steam very con-ably higher. The principle of all high ure engines is illustrated by Papin's diges- See SAFETY VALVE.

ilatation. Increase of bulk. See Ex-ION.

iophantine Analysis. That section of heory of unlimited problems, which attempts id rational and commensurable values an-ng to certain equations between squares cubes. Thus, it is properly a problem in iophantine analysis, to find all the values of l y, constituting $x^2 + y^2$ an exact square. shall subjoin methods of solution for one o of these unlimited or indeterminate pro-, in the way of example. No general rules atment are, or can be, laid down:—Let $7y = 29$, it is required to find correspond-*tegral* values for x and y.

Since $5x + 7y = 29$

$$x + y + \frac{2y}{5} = 5 + \frac{4}{5}$$

$$x = 5 - y + \frac{4 - 2y}{5}$$

in order that this may be a whole num- is evidently requisite that the fractional $\frac{-2y}{5}$ shall be a whole number. Let then $\frac{y}{\ } = n.$

$$\therefore 4 - 2y = 5n$$

$$2 - y = 2n + \frac{n}{2}$$

$$2 - 2n - \frac{n}{2} = y$$

this may be a whole number, it is necessary hat n shall be even. Hence, in order that l be an integral number, and x also, it is ary that $y = 2 - 2n - \frac{n}{2}$ and $x = 5 - y$ $\frac{-2y}{5}$ where n may be any even number. alues so found for x and y will satisfy the original equation.—Thus let $n = 2$, then $y = 2 - 4 - 1 = -3$ and $x = 5 + 3 + 2 = 10$, and $5 + 10 - 7 \times 3 = 29$. Again, let $n = o$, then $y = 2$, and $x = 5 - 2 = 3$, and $5 \times 3 + 7 \times 2 = 29$.—Next, let it be required to find a number, such that if divided by 3, 4, and 5, successively, the remainders will be 2, 3, and 4, respectively. Suppose that number, x.

Then $\frac{x}{3} = p + \frac{2}{3}$, where p is a whole number,

and $x = 3p + 2$

Also $\frac{x}{4} = q + \frac{3}{4}$, where q is a whole number,

$$\text{or } \frac{3p + 2}{4} = q + \frac{3}{4}$$

$$3p - 1 = 4q$$

$$p = q + \frac{q + 1}{3}$$

And if $\frac{q + 1}{3}$ be a whole number, p will be so.

Let $\frac{q + 1}{3} = r$, then $q = 3r - 1$ and $p = 4r - 1$.

Hence $x = 12r - 1$, where r is a whole number.

Also $\frac{x}{5} = t + \frac{4}{5}$, where t is a whole number.

$$\frac{12r - 1}{5} = t + \frac{4}{5}$$

$$12r - 1 = 5t + 4$$

$$2r - 1 + \frac{2r}{5} = t.$$

If therefore $\frac{2r}{5}$ be a whole number, t will be a whole number, and tracing back the process, x will be a whole number, and divisible by 3, 4, and 5, with remainders 2, 3, and 4, respectively. This will take place when r is any multiple of 5: *e.g.* let $r = 5$, then $x = 12 \times 5 - 1 = 59$, which number answers the conditions. Let $r = 10$, $x = 119$, another number also so answering, and so on.—*Thirdly*, let it be required to find two square numbers whose sum shall be a square, *e.g.*, to find two numbers whose square roots might represent the legs of a right angled triangle, in which the hypotenuse is commensurable to the legs.—Let x^2 and y^2 be the numbers. Then $x^2 + y^2 = z^2$. Let us suppose $z = x - ny$, a quantity which may be made anything we please by adjusting the value of n.

$$x^2 + y^2 = x^2 - 2nxy + n^2y^2$$

$$\therefore y = -2nx + n^2y$$

$$y(n^2 - 1) = 2nx$$

$$y = \frac{2nx}{n^2 - 1}$$

Given any value of x, then, y will be found by substitution in this formula, so that x^2 and y^2 shall be an exact square. Suppose that we wish to get x and y integers also as well as z, then $\frac{2nx}{n^2 - 1}$ must be an integer, as it evidently

will be if $x = n^2 - 1$ and n be an integer. In that case y will be $= 2n$ and $x = n^2 - 1$, there being no limitation regarding n but that it shall be an integer.

Let $n = 1$, then $y = 2$, $x = 0$, and $z = 2$.
Let $n = 2$, then $y = 4$, $x = 3$, and $z = 5$.
Let $n = 3$, then $y = 6$, $x = 8$, and $z = 10$.
And so on.

On this Diophantine analysis—so called from its discoverer Diophantus—has been reared the extensive subject of the Theory of Numbers by Legendre and Gauss.

Dioptrics. The two changes of fundamental importance, in Geometrical Optics, occur when a ray passes from a medium by which it is readily transmitted on to a medium by which it is not transmitted but thrown back; and when it passes from one transmitting medium into another, different in kind from the first, as, for example, from air into water. Geometrical Optics treats of the first, under the name of Catoptrics (or Katoptrics), (see CATOPTRICS,) and of the second, under the name Dioptrics. The subject of Dioptrics, therefore, is the transmission of rays of light from one medium into another, differing in kind. The laws, upon which the whole of Dioptrics rests, are the following:—When a ray of light passes from void space into any medium, it is bent from the straight line; but it proceeds onward in the plane which contains the perpendicular to the boundary of the medium at the point of its entrance, and the line of its original course. Thus, when a ray passes from void space, through a spherical piece of glass, if we draw a line perpendicular to the glass through the point where the ray strikes, and imagine a plane to pass through this perpendicular and the ray, the light will pass through the glass somewhere along that plane. The second law is this:—The sine of the angle which the ray makes with the perpendicular already described, bears a definite ratio, differing for every kind of medium, to the sine of the angle which the transmitted ray makes with the same perpendicular, produced. In other words, if S P be a ray falling upon a glass X P Y, and P S′ the direction of it while passing through the glass, and if Q P Q′

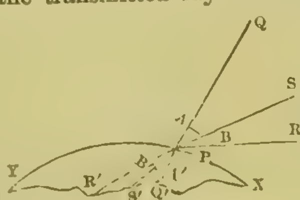


Fig. 1.

be perpendicular to the surface at P, then, when we take any point such as B, and a corresponding one like B′, equi-distant with it from P, and draw B A, B′ A′ at right angles to Q A Q′, A B and A′ B′ will bear, the one to the other, a certain ratio, which can be experimentally determined for each medium. Certain definitions enable us to state these two laws in shorter space. The *Angle of incidence* of a ray, is the angle contained between any given ray and the perpendicular to the bounding surface of the medium into which it passes, at the point where it falls upon it. Thus Q P S is the angle of incidence of the ray S P, and R P Q of the ray R P. The *Angle of refraction* is the angle made by the direction of the transmitted ray—as it passes through the new medium—with the perpendicular line produced. Thus Q′ P S′ and Q′ P R′ are the angles of refraction in the two cases respectively. The *angle of deviation* is the difference between the angles of incidence and refraction. Thus, the line S P being produced to Z, the angle Z P S′ is the angle of deviation. The

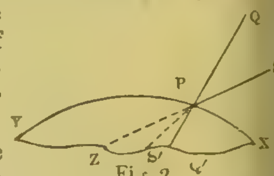


Fig. 2.

dioptrical laws can now be stated thus:—1. The incident and refracted rays are in the same plane with a perpendicular to the surface limiting the medium entered. 2. The sines of the angles of incidence and refraction bear a certain ratio, constant in the passage of any ray from vacuum into any given medium, at whatever angle the limiting surface is met by the incident ray. These laws hold absolutely with regard to homogeneous light. As to the latter law, however, the ratio of which we have spoken, differs for different kinds of light. Hence, if a refracting substance, such as a triangular prism, of very finely ground and very transparent glass be presented to a ray, so that it may freely pass through it, the different colours will be separated one from the other, and the ray

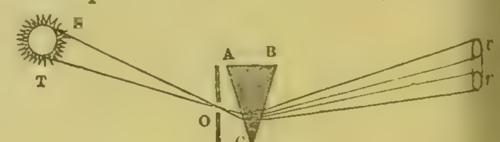


Fig. 3.

will be thrown along the space R F, presenting in detail an appearance like that below. The *lines* re-

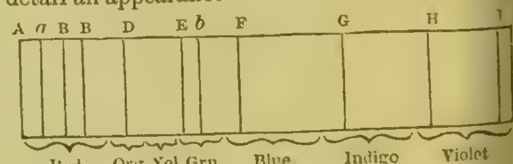


Fig. 4.

present spaces which appear dark in the spectrum, and the spaces marked red, orange, &c., are spaces where rays of those colours predominate. In order to give an idea of the differences in refraction to which the different rays are subject, we transcribe the subjoined table from Herschel's Treatise on Light. The numbers are the values of what is called the refractive index (the value of the fraction, $\frac{\text{sine of angle of incidence,}}{\text{sine of angle of refraction,}}$ or (in fig. 1, $\left.\frac{\text{B A}}{\text{B′ A′}}\right)$ for the different dark lines of the spectrum. There are many more of these lines, but those marked by the capital letters have their positions and refractive indices best known.

TABLE OF THE REFRACTIVE INDICES OF VARIOUS GLASSES AND LIQUIDS FOR SEVEN STANDARD RAYS.

Refracting Medium.	Spec. grav.	Values of μ (B)	μ (C)	μ (D)	μ (E)	μ (F)	μ (G)	μ (H)
Flint glass. No. 13	3 723	1.627749	1.629681	1.635036	1.642024	1.648260	1.660285	1.671062
Crown glass, No. 9	2 535	1.525832	1.526849	1.529587	1.533005	1.536052	1.541657	1.546566
Water......................	1.000	1.330935	1.331712	1.333577	1.335851	1.337818	1.341293	1.344177
Water, another experiment..	1.000	1.330977	1.331709	1.333577	1.335849	1.337788	1.341261	1.344162
Solution of potash	1 416	1.399629	1.400515	1.402805	1.405632	1.408082	1.412579	1.416368
Oil of turpentine............	0 885	1.470496	1.471530	1.474434	1.478353	1.481736	1.488198	1.493874
Flint glass, No. 3............	3 512	1.602042	1.603800	1.608494	1.614532	1.620042	1.630772	1.640373
Flint glass, No. 30	3 695	1.623570	1.625477	1.630585	1.637356	1.643466	1.655406	1.666072
Crown glass, No. 13	2.535	1.524312	1.525299	1.527982	1.531372	1.534337	1.539908	1.544684
Crown glass, letter M	2.756	1.554774	1.555933	1.559075	1.563150	1.566741	1.573535	1.579470
Flint glass, No. 23 .... Prism of 60° 15′ 42″	3.724	1.626596	1.628469	1.633667	1.640495	1.646756	1.658848	1.669686
Flint glass. No. 23 .... Prism of 45° 23′ 14″	3.724	1.626564	1.628451	1.633666	1.640544	1.646780	1.658849	1.669680

e full consideration of this refractive power rrying in the different coloured rays, belongs ysical optics. We shall, in this place, con- rays, as composed of perfectly homogeneous (*i.e.*, light possessing the same refractive rties, and having the same refractive index). irst consequence from these two laws is this: a ray is incident upon a refracting substance, ich the two surfaces (at which it enters, and which it emerges) are plane and parallel, the of the ray after emergence is exactly par- to its course before incidence. We shall a somewhat circuitous proof of this, for ke of avoiding the use of trigonometry. Let

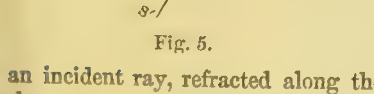


Fig. 5.

e an incident ray, refracted along the line and emergent at P′, from the substance, of A B C D is a perpendicular section, and in A B and C D are parallel lines, along the P′ S′, then P′ S′ is parallel to P S. Draw erpendiculars R R′, E E′, upon A B and C D, make P F, P′ F′, each equal to P P′. Draw nd F′ E′ parallel to A B and C D. Produce Q. It is necessary to premise that the law raction is utterly indifferent to the direction e ray of light, and that it holds equally when a ray passes from vacuum into a medium, or from a medium into vacuum. If one ray enters a medium from void space, and if another ray of the same kind of light passes through the medium in a line coinciding with that of the refracted ray, it will emerge into vacuum, and pass through vacuum along the original course of the first ray. According to the laws of refraction, the line R F will bear a certain proportion to R′ P′, and the line F′ E′ will bear the same proportion to E P, the side of the rectangle opposite to R′ P′, and therefore equal to it. R F and E′ F′ will then be equal. We have now two right-angled triangles, P R F, and F′ P′ E′, in which F P and F′ P′, being each equal to P P′, are themselves equal, and the sides F R, E′ F′ have been shown to be equal.—We have, therefore, S P Q and P′ S′ parallel.—The next consequence of importance is, that if a number of parallel rays fall upon a plane surface, the lines of direction of the refracted rays are also parallel; a conclusion we shall not stop to demonstrate.—It may have been noticed, that in the figures hitherto given, the refracted ray is bent *inward.* This invariably happens when a ray passes from vacuum into any medium, and the contrary happens when it passes from any medium into space. In the first case, the ray is bent towards the perpendicular which passes through the medium; and in the other, is bent from the perpendicular which passes through vacuum. The table already given illustrates this. The following table shows how this fact is expressed, in accordance with the refractive law; and at the same time may give an idea of the different refracting powers of different substances. The refraction is calculated for rays of mean refrangibility

—those between the violet and the red rays of the spectrum.

TABLE OF REFRACTIVE INDICES FOR RAYS OF MEAN REFRANGIBILITY.

Vacuum,	1·000000
Hydrogen, (32° Fahr., and 29·9218 inches barometric pressure),	1·000138
Oxygen,	1·000272
Air,	1·000294
Nitrogen,	1·009300
Ammonia,	1·000385
Chlorine,	1·000772
Ice,	1·3085
Water,	1·336
Vinegar,	1·347
Alcohol,	1·37
Fluor Spar,	1·436
Tallow, melted,	1·46
Oil of Turpentine,	1·48
Honey,	1·495
Dry Egg Yolk,	1·5
Glass, English Plate,	1·5
(Varying, in different specimens, from this to)—	
Bottle Glass,	1·582
Rock Crystal,	1·57
Amethyst,	1·562
Deep Red Glass,	1·729
Sapphire, White,	1·768
Ditto, Blue,	1·794
Sulphur,	2·
Diamond,	2·439 to 2·755
Mercury, (probably)	5·829

The meaning of the table is this:—in passing from vacuum to common air, the ratio of the sines is 1·000294; that is, if S P be the course through vacuum, and S′ P through common air, then S′ A′ = 1·000294. The ratio of S A to S′ A′ in the case of a passage from vacuum to any substance is given opposite its name. Two general results follow readily from the table;—*first*, the refractive index is greater in passing into denser, than into rarer media; and *second*, the same index is greater also in passing into the more combustible media.—Up to this point, we have proceeded on the idea, that the ray of light passes from vacuum into another medium, or from another medium into vacuum. But in the great majority of instances, the ray passes from one medium into another. This case, however, is met by the laws already noticed. Given the absolute refractive indices of two media (the index for the passage from vacuum into each of them), it is easy to find their relative refractive index (that for the passage from the one into the other). The relative index in passing from one medium into another, is the quotient of the absolute index of the second by the absolute index of the first. When the ray passes through one medium into a denser one, the relative refractive index will be measured by the quotient of one quantity (the refractive index of the denser) divided by another less than it (the refractive index of the rarer), and will therefore be greater than 1. The ray will thus be bent towards the perpendicular to the bounding surface. And the reverse must hold when the ray proceeds in the opposite direction.—It is worth noticing, that the refractive index of common air is very small, and that when we take the relative refractive index for air and a medium, very little alteration is required in the original absolute index of the medium; unless indeed, for the gases, whose indices, being likewise small, are very sensibly altered, in consequence. Thus the relative refractive index of a mean ray passing from air to glass is 1·4985, when that for such a ray, passing from vacuum to the same glass is 1·5.—Another very important proposition is this, that if we have a series of media, all of which are separated at their boundaries by parallel surfaces, any ray passes through one of the series, just as if it had entered it from vacuum. Thus, if we imagine, in fig. 6, two originally par-

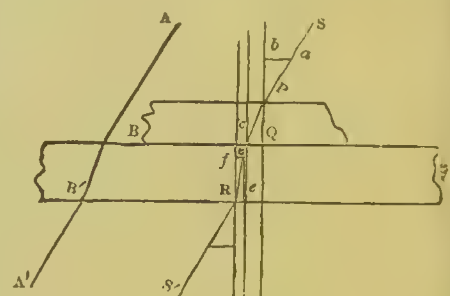


Fig. 6.

allel rays S P Q R S′ and A B B′ A′, coming both from vacuum, and passing through the medium B Q R B′, the rays will be transmitted through the medium in parallel lines.—This is a very direct and easily deducible consequence. The lines S′R and A′ B′ are parallel; so also, the lines B′ B, and R Q. But the latter has passed through a medium before entering B Q R B′ and the former has not.—It is important to notice a limitation requisite in the application of this statement, to the ordinary refraction of stellar and solar light in passing through the atmosphere. The

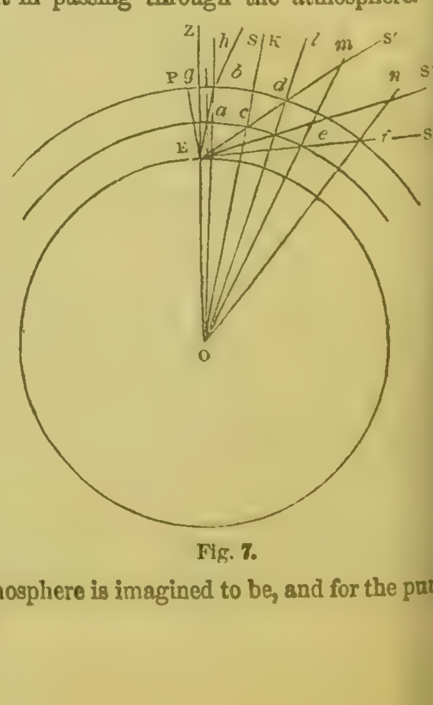


Fig. 7.

atmosphere is imagined to be, and for the purpose

calculation we consider it generally as com-ed of a series of layers of equal density passing round the earth, those of each density being at ual elevation. Let E represent the position of spectator at the surface and *a c e*, *b d f*, two acent atmospherical layers. O Z is directed vards the zenith. Let E *a b* S, E *c d* S′ E S″, *f* S‴ be luminous rays. Then the surfaces *f* and *a c e* are curved surfaces, and therefore is impossible to describe them as parallel. If, wever, a plane be drawn just exactly touching curved surface at any point, it is said to ve the same direction with the surface at at point. Now, in refraction, we have only do with a point of the limiting surface. It is ar that it would not, in the least, signify, al-ough we should have, instead of the straight e P Q, a hollow line Z, in the vertical section

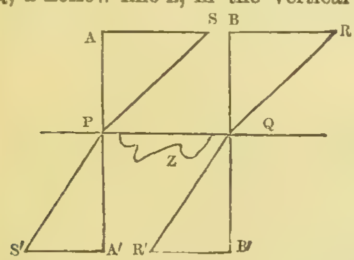


Fig. 8.

t the body. It is equally clear, that it does matter how near P or Q, such a hollow line y come, if it only leave room for S P to fall exactly the same spot as it would otherwise and do not interfere afterwards with the ections S′ P. Hence, in refraction, we have y to do with a very small part of the bound-surface, and that the part at which a ray is ident or emergent. We can substitute, there-e, instead of the surfaces *a c e*, *b d f*, three nes just touching them at *a*, *c*, and *e*, and at *d*, and *f*. The condition of which we have oken, that the uniting surfaces be parallel, in ler to the application of the propositions nonstrated, will be fulfilled, therefore, if the ne which touches the outer circle at *b* be rallel to that touching the inner at *a*. This ain will happen, if the lines *a g* and *b h* are rallel. For any planes are parallel, the per-ndiculars to which are parallel lines. Now se straight lines *ag*, *b h*, *c k*, *d l*, *e m*, *f n* all continuations of the radii through *a*, *b*, *d*, *e*, *f*. These lines *a g*, *b h*, *c k*, *d l*, *e m*, when brought to one point at O, are dily seen *not* to be parallel, and therefore planes of which we have already spoken not to be considered so either. Hence law which we have demonstrated, for rallel boundaries cannot be applied to at-spheric refraction. It is useful to observe, wever, that in the case of rays passing down far as from S near the zenith, the devia-n from the case which we have already con-ered is very small. The triangle E O *a* is very much elongated, and the angle E O *a*, by which the directions of the perpendiculars and therefore of the planes differ, is very small. Taking a radius of two degrees or so, round the zenith line, we may cut out a conical portion of the sky, like *p* E *b*, differing very little from a real cone, and having the luminous lines passing through A to E, passing into the layers of differ-ent densities *very* nearly at parallel surfaces. In fact, for this space, the deviation from the law stated could not well be estimated. In this case, then, in computing the exact amount of refrac-tion, we simply take the original ray as coming to the eye, in a line, the refractive index for the deviation of which from the original line is 1·000,294, subject to certain minute correc-tions for temperature and pressure. This is liable to no material error, for the refractive index for air has been very frequently observed. In the case of rays nearer the horizon, we have dif-ferent and far less simple laws, akin to those for prismatic refraction; and the correct applica-tion of these is dependent upon our knowledge of the physical constitution of the atmosphere; and is disturbed by any temporary disarrange-ment of that constitution, for example, by winds, earthquakes, meteors, &c.—We shall now pro-ceed to consider the case most analogous to at-mospheric refraction through the strata nearest the earth's surface. It is that of refraction through a prism. The general character of a prism, as we now consider it, is simply a body not bounded by parallel surfaces. When a transmitted ray strikes, at its points of incidence and emergence, on planes which are not parallel, or upon surfaces, the tangent planes to which at these points are not parallel, our previous conclusions do not hold, but become useless. We must consider this case specially. Conceive two planes, standing out at right angles to the plane of the paper, and making on that plane the figure B A C. Let S P be a ray of light from a luminous point, S, falling on the plane at P, and following the course S P Q R. The problem re-quires the discovery of the laws which regulate the transmission of the ray (supposed of homo-geneous light) S P Q R.

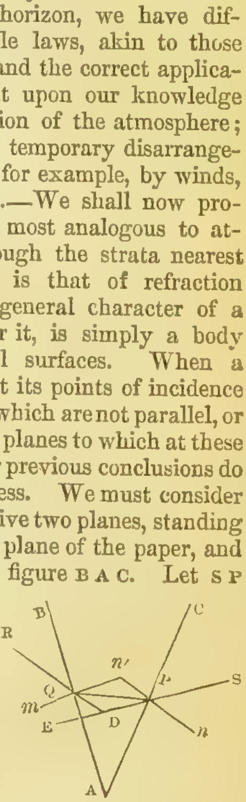


Fig. 9.

The first proposition we shall establish is this,—the algebraic sum of the angle of refraction at the first surface, and the angle of incidence at the second, is equal to the angle B A C, contained between the two planes. Draw *m n′* and *n′ n* perpen-dicular to the sides. Then *n′* P Q and *n′* Q P are respectively the angles of refraction at the first surface and of incidence at the second. It is asserted that their algebraic sum is equal to E A P. The angles *n′* Q A and *n′* P A are each right angles. The whole four angles *n′* Q A, *n′* P A, Q A P, Q *n′* P make up four right angles

(i. 32 cor.) It follows, therefore, that Q *n′* P and Q A P will make up two right angles. Again (i. 32) Q *n′* P and Q P *n′* with *n′* Q P make up also two right angles. The addition of Q A P, therefore, and of Q P *n′* with *n′* Q P to the same quantity Q *n′* P, gives the same result, and *n′* Q P and *n′* P Q must be equal to Q A P. We may imagine the line Q P becoming nearer and nearer to Q *n′*, until, in some cases, it comes to coincide with it, as in the figure, where P Q and the line Q *n′* are the same. In that case the angle P Q *n′* has vanished, and the proposition just proved, if true, would require that *n′* P Q should itself be equal to A. But as this is a limiting case, we cannot trust our conclusion without verifying it. Thus, therefore, Q P *n′* and Q P A make up the right angle *n′* P A, and Q P A and P A Q are equal to the right angle B Q P. It follows as before, that Q A P and *n′* P Q are equal. The proposition is, therefore, true in this case also. We have imagined that the line Q P crept up towards Q *n′* as the position of the ray S P changed. Might it not be conceived to have passed it and got to the other side? It is evident that it might, assuming the position of the figure below. But in this new case the result is the same. It follows from the above, that the deviation of a ray, caused by such passage through planes, is always towards the thicker and from the narrower part of the prism, if it be denser than the surrounding media, and in the opposite direction, if rarer.—We shall proceed to find expressions for the amount of deviation, in terms of the angle of the prism and of the relative refractive indices of A B C and the surrounding media. Let δ = angle of deviation (E D R, fig. 9, E P R, fig. 10, P D R, fig. 11). Let i be the angle of incidence at the first surface, i′ that of refraction; e the angle of emergence from the second surface, e′ that of incidence upon it, and α that of the two planes represented in section by A B and A C. Thus in fig. 9: δ = D P Q + D Q P.

$$= (\text{D P n}' - \text{n}'\text{P Q}) + (\text{n}'\text{Q D} - \text{n}'\text{Q P}).$$
$$= (i - i') + (e - e').$$

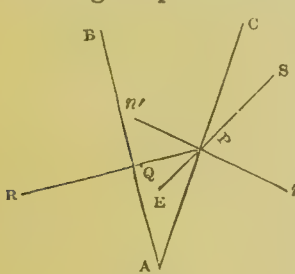


Fig. 10.

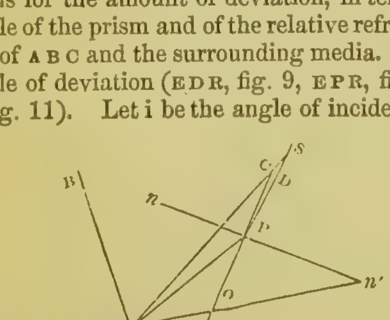


Fig. 11.

$$\delta = i + e - (i' + e')$$
$$= i + e - \alpha,$$

because α, the angle made by the planes, is equal to the algebraic sum of the angles of incidence at the second and of refraction at the first surface. The same proof would show that the statement holds in all the three cases, e becoming $-$ e, when the direction of R Q changes with respect to *m* Q′, as in fig. 11. Then

$$\sin i = \mu \sin i',$$
$$\text{and } \sin e = \mu' \sin e',$$

where μ and μ' are the relative refractive indices for a mean ray passing from the two surrounding media into A B C. Take for the sake of simplicity, *e.g.* the more usual case—media on the two sides of the same kind (μ and μ' being equal). The more general investigation proceeds on the principles which we are about to apply, but demands the introduction of higher mathematics.

$$\therefore \sin i = \mu \sin i'$$
$$\sin e = \mu' \sin e'$$
$$\alpha = i' + e'$$

In these three equations, we have got three unknown quantities defined, i′, e, and e′, and it would be possible by the application of not very recondite geometrical principles, to get these expressed in terms of the various known quantities, i, μ, μ' and α. We must confine ourselves still further, however,—viz.: to three special cases, capable of resolution without difficulty. Suppose i to be very small. But the sines of very small angles are nearly proportional to the angles themselves. Hence we may substitute for the expressions sin i and sin e, &c., i and e, &c. And we obtain

$$i = \mu i'$$
$$e = \mu e'$$
$$\alpha = i' + e'$$
$$\delta = i + e - \alpha = \mu i' + \mu e' - \alpha = \mu(i' + e') - \alpha$$
$$= \mu \alpha - \alpha = (\mu - 1)\alpha$$

Applying this formula, to the case of a prism of glass, with an angle of 60°, we will have

$$\delta = (\mu - 1)\,60^\circ$$
$$= (1{\cdot}5 - 1)\,60^\circ$$
$$= {\cdot}5 \times 60^\circ = 30^\circ$$

—The second special case we shall consider, is that in which the angles of incidence and emergence are equal. Taking our standard formulæ —

$$\sin i = \mu \sin i'$$
$$\sin e = \mu \sin e'$$
$$\alpha = i' + e'$$

We shall then have i = e; whence, from the first two formulæ i′ = e′. (This merely asserts a proposition already established, viz.:—that two parallel rays are refracted parallel, in the same circumstances.) Instead of having the rays parallel, conceive that the sides B A and B C after having been together, are carried away separate. Since i and e were equal when they were together, the rays were parallel then, and the angles of refraction were therefore equal then. But if the system—the glass and the ray—be merely removed, with no change in their relative

sitions, the refraction must remain quite as fore.

$$\text{Hence } i' = \frac{\alpha}{2}$$

$$\text{Now } \delta = i + e - \alpha = 2\,i - \alpha$$

$$\text{But } \sin i = \mu \sin \frac{\alpha}{2}$$

$\therefore$ i is the angle whose sine is $\mu \sin \frac{\alpha}{2}$

nd $\delta = 2 \times (\text{angle whose sine is } \mu \sin \frac{\alpha}{2}) - \alpha$.

Our third special case, is more general than ny of the others. It does not assume that the edia, on the two sides, should be the same: fact, it takes in the most general application t the case conceivable, when the ray emerges rpendicularly. In that case $e = 0°$; whence is also equal to 0°.

Hence, taking our formulæ—

$$\sin i = \mu \sin i'$$
$$\sin e = \mu' \sin e'$$
$$\alpha = i' + e'$$

e have $\alpha = i'$

$$\sin i = \mu \sin i' = \mu \sin \alpha$$

$\therefore$ i = angle whose sine is $\mu \sin \alpha$

nd $\delta = i + e - a = i - \alpha = (\text{angle whose sine is } \mu \sin \alpha) - \alpha$

pplying these two last formulæ to the case of glass prism of 60° at A, (taking μ, the relative fractive index of air and glass, as 1·5), we ve, when the angles of incidence and emerence are equal, (in this case, by the way, the viation is proved, by the higher mathematics be the least possible)

$$\delta = 2 \times \text{angle whose sine is } \cdot 75 - 60°$$
$$= 2 \times 48°\cdot 35' - 60° = 37°\cdot 10'$$

d when the ray emerges perpendicularly to the unding surface, which might, in this case, be iced in water, (the other boundary surface ing kept above it),—we have δ = angle whose e is 1·299037—60°. A most anomalous relt, for no angle has a sign greater than 1. e inference is, that it is impossible to transmit ray through a prism with a refracting angle, that the emergent ray be perpendicular to the face of emersion. This result is sufficiently eresting; but it becomes more so, when it is ewed as an inevitable consequence of the rective law; and when another class of analogous ults, depending on the same principle, are ought up to confirm it. The refractive index m a rarer to a denser medium is always greater an 1. Suppose it μ. But in every case of raction, the formula $\sin i = u \sin i'$ applies. now $\sin i'$ be greater than $\frac{1}{\mu}$ (and whatever μ, since $\frac{1}{\mu}$ must be less than 1, some angle may found whose sine would be so), we should ve $\mu \sin i'$ greater than 1; but $\mu \sin i'$ is equal to sin i, and sin i is therefore greater than 1, which is impossible. Either, therefore, the refractive law is incorrect, or some false hypothesis has been made. That hypothesis is, that light could be refracted into a medium at an angle of refraction whose sine is greater than $\frac{1}{\mu}$, or, which is the same thing, that if passing through such a medium, any ray could in these circumstances, *be refracted out.* We arrive, therefore, at the conclusion, that light is not refracted into a medium at an angle of refraction whose sine is greater than $\frac{1}{\mu}$. For example, all light, passing into water, will be refracted at angles of refraction whose sines shall not be greater than $\frac{1}{1\cdot 331}$ (1·331 being the least refractive index, for the red ray, through water, and the rays so refracted will be red; violet rays will not be refracted at an angle whose sine shall be greater than $\frac{1}{1\cdot 344}$. (See table 1, already given.) The red rays, therefore, may fall within 48° 42′ (angle whose sine is $\frac{1}{1\cdot 331}$), and the violet rays within 48° 4′ (angle whose sine is $\frac{1}{1\cdot 344}$), of the perpendicular to the surface at the point at which they enter. The 38′ between the two would be very beautifully tinged with the prismatic colours. Hence to an eye below water all the objects at the surface of the earth will appear as if grouped into the space of 97° 24′, instead of appearing, as they do, at the surface under an angular space of nearly 180°. The eyes of fishes have very nearly the same density and refractive index with the water in which they live; there is, therefore, no material change in this effect to them.—We have thus obtained, then, by the above processes, expressions for δ in terms of μ and α. It remains to deduce similar expressions for μ in the terms δ and α. Sometimes we have peculiar specimens of refracting substances, whose exact power we wish to discover, and in order to this we measure δ for a prism with a given refracting angle. The expressions which we shall deduce refer respectively to the three cases already specified.

In the first case we found $\delta = (\mu - 1)\,\alpha$

$$\frac{d}{\alpha} = \mu - 1$$

$$\frac{\delta + \alpha}{\alpha} = \mu$$

Thus, in a prism of 60° refractive angle, if δ be found to be 33°, we obtain $\alpha = 1\cdot 55$; from which, it is probable that the glass is a low flint glass, containing lead in composition. (*Herschel on Light.*)—In the second case

$$\delta = 2\,i - \alpha, \text{ and } i' = \frac{\alpha}{2}$$

$$\mu = \frac{\sin i}{\sin i'} = \frac{\sin\frac{(\delta + \alpha)}{2}}{\sin\frac{\alpha}{2}}$$

Take similarly $\delta = 30^\circ$

Then, $\mu = \frac{\sin 45^\circ}{\sin 30^\circ} = 1{\cdot}414$

In the third case,

$$\delta = i - \alpha$$
$$i' = \alpha$$

$$\therefore \frac{\sin i}{\sin i'} = \mu = \frac{\sin(\delta + \alpha)}{\sin \alpha}$$

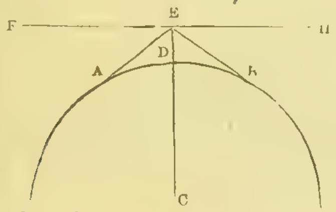
Let $\delta = 30^\circ$, here also

$$\mu = \frac{2}{\sqrt{3}} = \frac{2\sqrt{3}}{3} = 1{\cdot}151.$$

—The subject of the refraction of light at the surfaces of various curves, and of the convergence and divergence in the refracted rays, will be fully treated under article LENS. Another subject, closely connected with dioptrics, and depending upon dioptrical principles, is the appearance which bodies make to the eye behind refracting surfaces. This will be treated of elsewhere. See LENS and TELESCOPE. We shall just state the propositions upon which it rests. Its principle is very simple. Every line or surface, luminous either in itself or by reflection, is supposed to be made up of a number of luminous points placed closely together. The rays from these appear, after passage through the prism, as if they came from points entirely different from those from which they are really emitted, according to the laws already demonstrated in this article. For each point, therefore, we will get another corresponding point, from which the rays, really emergent from the first, will apparently emerge. The whole of these corresponding points, put together, will make up a line or a surface, as the original body made up the one or the other, and the problem which we now just indicate is this—Given a line or a surface made up of luminous points, required to find its position, as that appears through a refracting body? This may be the case while the eye remains in one medium and the body in another, only one of which terminates between the body and the eye. A fish seen in water is such a body. Or again the body and the eye may be separated by a medium completely terminating between them, as a stick seen through glass. We have shown in this latter case, that the point from which the rays will seem to an eye on the other side of the medium, to emerge, will be at the distance $u + \frac{t}{\mu}$ from the surface of the medium—approximately. If, therefore, we have an object *given*, and draw from its points various perpendiculars to the farther surface of the plane medium of which we speak, and mark off, from the points of incidence of these perpendiculars, distances equal to $u + \frac{t}{\mu}$ respectively, (μ being the relative refractive index, and μ the perpendicular distance of each point of the object from the plane surface), we shall obtain the image required. It would be easy to show, for example, that the image of a straight line so obtained will also be a straight line, parallel to the first. If, as in the other case, the medium be not terminated between the object and the eye, we should have $u' = \mu\, u$ as the formula to be applied for each point, where u' is the distance of the point from which the rays appear to emerge, from the bounding surface, and u the distance of the point from which they actually come. In this case also, the image of a straight line would be a straight line. It is to be noted carefully, however, whether the eye is situated in the denser or in the rarer medium. As this changes, μ and $\frac{1}{\mu}$ change places. Thus, also the relative refractive index of water and glass is μ, when that of glass and water is $\frac{1}{\mu}$. In the case of the fishes seen in water, the value of μ is less than 1, because equal to the reciprocal of a number greater than 1, and fishes will therefore always appear nearer the surface to an eye above it, than they really are. The refractive indices for various kinds of water vary, but that given in the second table, 1·336 may be taken as the standard value. The distance, then, of a fish from the surface of still water will be 1·336 times more than the apparent distance. In the case of the virtual images (images occupying the points from which the actual light appears to proceed) formed by prisms, the principles are more complex. We shall take only one case—that of a prism with a small refracting angle. Suppose that the rays by which the image is really formed are all perpendicular to the two prismatic surfaces. It is then easy to calculate the apparent position of the object. Imagine now the rays from that image falling on the second surface, untouched by the first, and calculate the position of the new image by the same law. This position may be taken as sufficiently near for practical purposes to the real position of the image.—The foregoing paper, referring only to the leading phenomena of Dioptrics, does not pretend to exhaust the subject. The advanced student is earnestly referred to a recent memoir by Gauss, reprinted in *Taylor's Scientific Memoirs.*

Dip of Horizon, is the angle which the line drawn from the eye to the most distant visible object on the earth's surface makes with the rational horizon. It is impossible to observe the dip on the surface of the ground, because of its inequalities. It is readily observed, however,

at sea. The dip of the horizon is affected by refraction. See REFRACTION. As far as it is not so influenced we shall explain it here.—A spectator situate at E, above the surface of the earth, will see any object above the lines E A, E B. He cannot see below these lines. The angle then which either of these lowest lines makes with F E H, which is drawn perpendicular to E C (C being the centre of the earth), and which is so, parallel to the horizon of D, the point immediately beneath E, on the surface, is called the dip of the horizon. Thus, in the figure, either A E F or B E H, is so.—It is easy



to calculate its amount. The angle F E A is equal to E C A, and when we know E D, it is not difficult to find the value of E C A. If we add E D to the length of the radius of the earth at the spot, and divide that same length of the radius, by the sum, we obtain a decimal, which will be found in trigonometrical tables of cosines, opposite the angle we seek.—If, as is more frequently the case, we wish to know the length of space B A, over which we can range at a given height, we may obtain a very near approximation by multiplying E D by four times the diameter of the earth, and extracting the square root. This will give the diameter of the circle of vision. It is to be noted, however, that the approximation is the less accurate the larger E D becomes, and that if it exceed a mile or two we must employ other expedients.—It is necessary to remember that results thus obtained, must be corrected for refraction.

Disc. (*δίσκος, a quoit.*) An astronomical term signifying the part of the surface of a heavenly body visible at any given time. Disc is also employed in optics, although rarely, to signify the width of aperture of telescope glasses.

Discharge. In the article upon Conduction of Electricity we described the several modes of electric discharge, or of the transference and annihilation of the electric forces. One of these modes, the disruptive discharge, will be specially considered in this article, as it is that to which the simple term Discharge is most usually appropriated. If an electrified conductor is insulated, and brought near to another conductor, it electrifies the latter by induction, and has its own charge determined with greater or less intensity towards that part of its surface which lies nearest to the induced charge. Up to certain limits, the intensity of this electric action may be increased by several means, as by bringing the surfaces nearer to each other, by increasing the charge upon the conductor, or by changing the forms of the surfaces: but the increase, however effected, has a limit in every case. In this limiting state of the inductive actions, a spark appears between the surfaces, accompanied by sound and other effects. and the electric forces are found to be wholly or partially destroyed. In other words, discharge has occurred *across the air;* and it is called the disruptive discharge because of the sudden and violent commotion that it produces in the matter occupying its path. The conductors may be separated by other substances as well as air, for the production of these effects. Discharge between their surfaces may be effected across any kind of interjacent matter, gaseous, liquid, or solid, which possesses a certain degree of insulating power. When the interjacent matter is solid, it is shattered, or cracked, or pierced, by the disruptive action, so as to be permanently unfitted, by its loss of insulating power, for the repetition of discharge through its mass. On this account, solid media of discharge cannot be employed to any great extent in the investigation of the laws of this electric action. The laws of discharge in air have been investigated with much attention and success by Sir W. Snow Harris. A full account of his methods and results is given in the *Philosophical Transactions* for 1834. The principal subject of his inquiry was, the quantity of electricity requisite for the production of discharge in given circumstances: and of his valuable results we may mention two, which are remarkably simple and definite, and which have been verified under such varied forms of experiment, as to be well entitled to the name of general laws. If two electrified conducting balls are made to approach one another in air, they discharge, to each other, across the air, when their mutual distance has diminished to a certain value. This amount is called the *striking distance.* In constant circumstances, the striking distance is constant. It varies with several elements; particularly, with the forms and magnitudes of the conducting surfaces, with the amount of their charges, and with the condition of the surrounding air. In the two laws above mentioned, the striking distance is related mathematically to the amount of the charges and to the density of the air. For the same balls, the striking distance varies exactly in the direct ratio of the charge, and in the inverse ratio of the density of the air. Thus, let one ball be connected with the ground, and let the other be charged by the unit jar with one, two, or three units of electricity; then the striking distances between the two balls across the air will be in the three cases as one, two, and three. Again, if the two balls be enclosed in an air pump, and the air be rarified, the striking distance for a constant charge increases, and precisely in the same proportion as the density of the air diminishes; and in these circumstances a constant striking distance is maintained by diminishing the charge

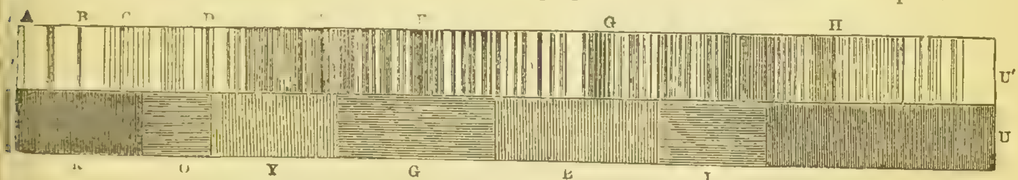
in the same proportion as the density. Faraday has made numerous experiments upon disruptive discharge, as may be seen in the twelfth series of his *Experimental Researches.* One inquiry of great interest which he has attempted in those experiments is, the dependence of the striking distance upon the specific nature of the gaseous medium interposed between the conductors. It had been commonly supposed that the power of air and gases in restraining discharge, up to the limiting intensity of inductive action, was due to statical pressure upon the conducting surfaces. This view, however unsatisfactory, was not without support in fact, as may be seen from the second law above stated. But Faraday's results have rendered it more than questionable, by proving the existence of specific restraining powers in the several gases. Thus, the striking distance in muriatic acid gas is about one-half of that in common air in similar circumstances, and barely one-third of that in hydrogen. The restraining power of muriatic acid is therefore three times that of hydrogen. This is for the same pressures; and similar differences, though not so extensive, are observed among the other gases. These differences cannot be attributed to specific gravity, for the orders of the gases in respect to these two properties are not the same. The differences are founded, according to all appearance, upon specific electric distinctions among the gases in relation to discharge. This fact appears the more remarkable, when we consider that all the gases have sensibly the same inductive capacity: but a similar contrast has long been established in the case of air at different densities. Faraday's experiments on disruptive discharge appear to have brought out another specific difference among the gases. When two charged balls of different dimensions are brought near to each other in air, the discharge is restrained more powerfully when the *smaller* ball is that positively electrified,—other things being equal. A similar law holds in *hydrogen*, *olefiant gas*, *carbonic acid*, and *muriatic acid;* while the reverse appears to hold in *oxygen*, *nitrogen*, and *coal gas.* Some of these results are still questionable; but a *specific difference* in this respect, has been certainly established. For full information upon these points the reader is referred to Faraday's *Experimental Researches*, series twelfth and thirteenth; where the bearing of the facts upon the theory of electricity is also brought out pretty fully. We should not omit reference to the variety of forms assumed by disruptive discharge in the gases. These are described by Faraday under the names of spark, brush, glow, and dark discharge. The first three of these are sufficiently indicated by their names, to all who have witnessed the common optical effects produced by electric machines in action. Their theoretical connection is simple enough; for the brush has been actually analyzed into a quick succession of spark-discharges, and the glow may be supposed to consist of discharges in still closer succession. The dark discharge, as far as it has been yet observed, occurs in connection with the glow. When two balls, for example, discharge to each other through rarified air, there is exhibited, in certain circumstances, a perfectly dark interval in the space between the balls, and this while the whole space appears to be traversed by the discharge. It is even considered probable that there are dark discharges analogous to the spark and brush, but not luminous in any part. In the old theory, the phenomena of discharge in air, were explained as the result of the mutual attractions of the electricities, opposing and overcoming the resistance of the air. The latter resistance was generally identified with the statical pressure of the air upon the conductor. This theory was a natural interpretation of the more elementary facts; but it is now giving place to the theoretical views of Faraday. One of the great excellences of the latter theory is the strictness of connection that is established among the varied phenomena of electricity, a strictness so well sustained even in the present undeveloped state of the theory, that it is difficult to give separate explanations of electric phenomena without tedious reference to other phenomena. Induction, according to Faraday, is the result of a forced mechanical condition of the particles of a dielectric. When a body is placed between two electrified conductors, its particles are thrown into this forced condition, by the electric forces resident upon the conducting surfaces. The intensity of this condition is exalted by the approximation of the conductors, until the particles *throw off* their forces, by communication to each other; and this constitutes discharge in general. Disruptive discharge differs from conduction according to this theory, not in kind but in degree; the limiting intensity of the forced inductive condition of the particles being very great in the former case, and very small in the latter.

Dispersion of Light. It is shown, under Refraction, that the *sine* of the angle of the rays *incident*, upon a refracting medium, is proportional to the *sine* of the angle of *Refraction;* in other words, that the ratio

$$\frac{\sin i}{\sin r}$$

is constant for the same medium. This ratio is termed the *index of refraction* for that medium. But as it is well known that the ray of light consists of parts, or is a *sheaf of rays*, unequally refrangible, and that on this account it becomes, after refraction, the coloured *spectrum*—it is clear that the angle of refraction above spoken of, is merely the *mean angle.* Each colour, indeed, has its own *index of refraction* for every separate medium or refracting body; nor have we been hitherto enabled to trace the physical connection between the indices

refraction of the different colours, and other ıalities of the refracting substance—one colour ›pearing contracted, and another remarkably ·veloped, when the ray is subjected to the fringent action of different media. The *dis-rsion* of light, strictly speaking, is the differ-ce of the refracting indices of the two extreme lours—*red* and *violet*—of the spectrum pro-ıced by any substance: and—(the ratio of its *spersion*, to its mean index of refraction minished by unity)—what is called the *dis-rsive power* of that refringent substance, is easured by means of that. But, as every one ıo has seen a spectrum knows, the various colours are spread indefinitely over a considerable space,—so indefinitely that it is not possible to state accurately, either the beginning, the end, or the middle of any colour. The exact measurement of *dispersion* would thus have been impossible, but for the remarkable discovery, by Frauenhofer, of the *dark bands*. The character of these is fully explained under FRAUENHOFER'S LINES, as well as the circumstances under which they may be readily discerned. The actual spectrum, as seen through a telescope, would be represented correctly, if the upper portion of the accompanying figure, or R′ U′, were superposed on its under or coloured part. These



ırk bands belong to the different colours over hich they are placed in this figure, and whatver the refringent or dispersive medium, they tain their place in relation to these colours. stead, therefore, of having to deal with an definite coloured space, we can deal with a finite dark line belonging to that colour; and rauenhofer, as well as all subsequent inquirers, ve adopted for this purpose the lines B, C, D, F, G, H. By aid of these rays, the *indices of fraction* of the different colours, in reference to refracting media, may be formed and tabued; and the dispersive power of such media nputed. Such observed and computed tables given at length in all good writers on optics. suffices here to have stated their origin and ture.—Nor can we omit to remark, that such erminations are essential to the scientific comtion of ACHROMATISM.

ispersion Epipolic or Superficial; Disrsion Internal; Fluorescence. Terms riously applied to a class of phenomena of ich the explanation has recently been given Professor Stokes in several memoirs, that conute an epoch in the history of our knowledge the affections of light. Indications of the stence of an action on light, not known before, substances of various kinds, had not escaped notice of Sir David Brewster and Sir John rschel. Sir David clearly pointed out that had noticed these as early as 1838; and Sir ın Herschel entered more fully on the subject 1845. What attracted Herschel was as folrs:—Solution of quinine, which, when viewed transmitted light, is colourless as water, does in certain aspects, and under certain incices of that light, exhibit a *celestial blue colour*. is colour, according to Herschel, comes only n a stratum of fluid, of small but finite thicks, adjacent to the surface at which the light ers. Brewster asserts that he has seen the ıt considerably farther from the surfaces; and he identifies it with a property belonging to fluor spar—(hence naming it *fluorescence*). He refers it to a common, or rather an extensive class of facts, included under the name of "internal dispersion." The light thus transmitted is found capable of being dispersed by the prism, and to consist of rays of various refrangibilities —none of them, however, belonging to the red end of the spectrum. Herschel could discern no traces of polarization, on examining it by the tourmaline; Brewster, on the other hand, on using a rhomb of calcareous spar, conceives that he certainly detected traces of this condition. The investigations of this latter eminent inquirer will be found in the *Transactions of the Royal Society of Edinburgh* in 1846, and again in the *Philosophical Magazine* for June, 1848.—Matters remained in this state until the year 1856, when Mr. Stokes took up the problem. Struck by the fact, that while the rays that enter the fluid cannot pass through, or affect more than a trifling thickness of the fluid, the dispersed rays themselves traverse many inches of that fluid with perfect freedom, he felt himself shut up to the conclusion, that the *rays producing dispersion* are in some way of *a different nature from the rays dispersed.* Now, in what quality or qualities can two sets of light-rays differ from each other? Under the modern theory of light, a ray is distinguished or individualized by two circumstances—its period of vibration and its state of polarization; or, as we may rather say, its degree of refrangibility and its state of polarization. Pursuing the latter hypothesis in reference to its power to account for the phenomenon now before him, Mr. Stokes shows it to be utterly untenable, and thereby found himself driven to suppose that the difference of nature between these two sets of rays is a *change of refrangibility.* But from the time of Newton it had been believed that light retains its refrangibility through all the modifications which it may undergo. "Nevertheless," says

Mr. Stokes, "it seemed to me less improbable that the refrangibility should have changed, than that the undulatory theory should have been found at fault. And when I reflected on the extreme simplicity of the whole explanation, if only this one supposition be admitted, I could not help feeling a strong expectation that it would turn out to be true. In fact, we have only to suppose that the invisible rays beyond the extreme violet give rise by internal dispersion to others which fall within those limits of refrangibility between which the retina of the human eye is affected, and the explanation is obvious. The narrowness of the blue band, observed by Sir John Herschel, would merely indicate that the fluid, though highly transparent with regard to the visible rays, was nearly opaque with regard to the invisible. According to the law of continuity, the passage from almost perfect transparency to a high degree of opacity, would not take place abruptly; and thus rays of intermediate refrangibilities might produce the blue gleam noticed by Sir John Herschel, or the blue cylinder, or rather cone, observed by Sir David Brewster. We should thus, too, have an immediate explanation of a remarkable circumstance connected with the blue band, namely, that it can hardly be seen by strong candle-light, though readily seen by even weak day-light. For candle-light, as is well known, is deficient in the chemical rays situated beyond the extreme violet."—That a ray of Light should be definitely changed in its refrangibility, by its passage through a medium, was indeed a new truth in science; and it opens a boundless sphere of investigation as to the relations of the Ethereal Medium with the molecular constitution of the bodies that so influence its vibrations. Nothing in modern times has been substantiated which is so fertile, and so sure to evolve, ultimately, momentous results. Various inquirers, and some speculative thinkers, *e. g.*, the Abbé Moigno, in his fundamental objection to some conclusions by Sir David Brewster—(see SPECTRUM) have imagined the possibility of such a change; but the honour of having established it, belongs to Mr. Stokes.—We add one of the most general of the conclusions of his first Memoir.

"The phenomena of internal dispersion oppose fresh difficulties to the supposition of a difference of nature in luminous, chemical, and phosphorogenic rays, but are perfectly conformable to the supposition that the production of light, of chemical changes, and of phosphoric excitement, are merely different effects of the same cause. The phosphorogenic rays of an electric spark, which, as is already known, are intercepted by glass, appear to be nothing more than invisible rays of excessively high refrangibility, which there is no reason for supposing to be of a different nature from rays of light."

Divergence: Divergent Series. Lines which constantly recede, one from the other, are termed *divergent.* For instance, two adjacent radii of a circle *diverge.*—A *divergent series*, in Algebra, is an infinite series of this sort:—if, on stopping at any term of it, and taking the series as there terminated for the whole series, we cannot say that the error committed lies within a definite amount or limit,—that series is a diverging or divergent series.

Diving Bell. See BELL, DIVING.

Divisibility. All ordinary bodies are supposed to be made up of a considerable number of parts, which may be separated one from another. Every body, however small, must have a certain volume; and as this *volume* can be divided in abstraction infinitely, we may imagine an infinite number of lines drawn on the surface of the body, or through it, marking the boundaries of its different cohering particles. If these particles be not joined by an infinite force, there must be some finite force capable of separating them. If, therefore, we cannot conceive an infinite force, we are driven to believe in the infinite divisibility of matter; and such a force we cannot conceive.—But although we are led to a belief in the infinite divisibility of matter, it by no means follows, that we can divide it infinitely. The smaller atoms may be held together by forces perfectly sufficient to resist the most powerful that we are able to apply. The metaphysical question admits of metaphysical argument; but no actual result is ever likely to throw light upon it.—Passing from it, therefore, we next ask, do we ever find in practice a limit higher than the mere imperfection of the senses, and therefore of instruments constructed for, and adapted to, them, where the division of matter stops? The answer is clear and decisive —we do not. Our progress in dividing bodies seems only limited by the imperfection of our senses and of our implements.—Thus, in the case of liquids and gases, there must be evidently a very great divisibility—for water is easily separable from water at any point, and yet the most powerful microscope is unable to detect interstices. No inequalities can be perceived upon their surface, either by the eye or the touch.—All experiments, in fact, tend to show, that the divisibility of matter is almost, if not altogether, infinite. A fibre of silk has a thickness of $\frac{1}{5000}$th of an inch, a fibre of merino is about twice this in the thickness of its diameter, and the fibres of the ordinary hair in animals, with the finer furs, are between these. Yet each of these very thin fibres contains a complete system—with parts distinct in nature and in properties—which we do not find to adhere powerfully, where we can test that adherence.—A tube of glass presented to the blowpipe, and drawn very carefully out will come to be as fine as this silk fibre, while retaining its character as a tube, with a distinct exterior and interior surface. The polish which metals take, is another striking instance of their divisibility. It is produced

rubbing them with hard grains, which make ndefinite number of little holes at the sur-, instead of a smooth surface. But these are minute, that the same touch by which a fibre ilk is easily felt, cannot detect them. Par-s, therefore, as small, or smaller than would these holes, have been detached from the s. When a soap bubble is blown, and not wed to detach itself, just before it bursts e is a place at the top where a black spot ears—the rest of the bubble displaying all colours of the rainbow. This is $\frac{1}{2500000}$th n inch in thickness. We can blow glass so , that this same phenomenon will be visible; then the glass must be of this same thick-. Divisibility is strikingly illustrated by thinness of wires. Wollaston placed a pla-um wire, $\frac{1}{100}$th of an inch in diameter, in the re of a silver wire $\frac{1}{5}$th of an inch. He then them through a drawing plate, and the re-was that, as the platinum constantly main-ed its proportion to the silver; when the r was $\frac{1}{1500}$th of an inch, the former was 00th in diameter. The silver was then lved away by nitric acid, which left the inum wire. Divisibility is well illustrated he case of odours. A room, the supply of n which is regularly renewed, will be per-ed for months by a little musk exposed in while yet no loss can be detected in the ht of the musk. This odour is caused by floating in the air of odoriferous particles ersed through it. The most striking in-ces of divisibility are found in the animal vegetable kingdoms. Wherever we find bodies, differing in nature, in juxtaposition, onceive that the mass made up of the two visible. Proceeding upon this principle, let onsider a drop of blood. This has been dis-red microscopically to be made up of a very t number of globules, differing in size and , according to the species. In man, and he other mammalia, for instance, they are rical; and in birds and fishes, elongated. ian, again, they are $\frac{1}{3700}$th of an inch in eter; in the gnat, in which they are small-about $\frac{1}{8500}$th of an inch, and they vary be-n this and $\frac{1}{3100}$th of an inch. Now in all globules there is a distinct chemical con-tion; all of them are made up from distinct s of the body, and therefore all of them are ately subdivisible.—Finally, there are ani-s as small as these globules of blood. We ot prove that they have organs. But we them move, and this requires a complicated nism. They are capable of directing them-s to the accomplishment of ends—of shun-obstacles—and even of vanquishing them. these properties, microscopically discovered, e the existence within them of a compli-d organism, and therefore the smallest object h we can perceive must be yet indefinitely ible. Great and little, in truth, seem in creation alike terms expressing merely relation to us, and vanish in the universe of the Infinite God.

Division, in *Arithmetic*, an operation the reverse of multiplication. Its symbols are b) a (: $\frac{a}{b}$: or a $\div$ b.

Division, in *Astronomical Instruments*, signifies that graduation of their arcs or circles, by which equal parts—the measures of equal angles—are marked out on them. The practical art of *division* is now carried to very high perfection. The late Troughton greatly advanced it by his exquisite dividing engine; and continental artists have certainly not lagged in the race. In this country, astronomical circles are usually divided primarily into every *five* minutes of arc: on the continent they carry primary subdivisions farther—as far as *two* minutes of arc. These primary intervals are of course again subdivided, in either case, by aid of *Verniers* or *Reading Microscopes* (*q.v.*) Notwithstanding the extreme accuracy of the mechanical divisions, however, the observer never trusts absolutely to that accuracy. The errors of its division, are among the errors which he conceives inseparable from his instrument; and he does not delay to investigate their amount,—employing the results of his inquiries in accordance with the *theory of corrections*. The investigation is a tedious one, and will be found explained in special treatises on astronomical instruments.

Dome. The spheroidal or spherical top of a building. It is very frequently placed above a circular tower. The *dome*, as distinguished from the *cupola*, signifies the exterior surface; while the *cupola* signifies the interior. The principles of the dome are very much those of the bridge and the arch (see ARCH), and are fully explained in ordinary architectural works. —See VAULT.

Dominical Letter. It is often convenient in historical research to know upon what day of the week a particular event may have taken place, when its date (*e.g.* 19th January, 1563) is specified. For the purpose of determining this, tables of *dominical letters* are constructed. Their meaning and method of employment is as follows. The seven first letters, A, B, C, D, E, F, G, are connected with the first, second, third, &c. days of every year, A being again connected with the eighth, B with the ninth, &c., A with the fifteenth, B with the sixteenth, C with the seventeenth, and so on. The dominical letter is that letter which falls, according to this arrangement, to the first Sunday (*Lord's* day—*dominus*—*dominical*) of each year. In leap year there is no letter connected with the twenty-ninth of February, but one is missed over for that day. It is evident that the same result will be obtained by taking the letter next to the dominical letter of the year (*e.g.* E for D) as the dominical letter after that date. Every leap year has two domi-

nical letters, one for the first two months and one for the last ten. If then we have the dominical letter for each year, and tables are constructed, according to the old style and to the new style, which give this immediately, we turn to another table wherein we have the year divided into 52 weeks, thus—

Same day of week for each line.

Jan. 1, 8, 15, 22, 29, Feb. 5, 12, 19, 26, Mar. 5.
Jan. 2, 9, 16, 23, 30, Feb. 6, 13, 20, 27, Mar. 6.
Jan. 3, 10, 17, 24, 31, Feb. 7, 14, 21, 28, Mar. 7.
Jan. 4, 11, 18, 25, Feb. 1, 8, 15, 22, Mar. 1, 8, and so on.

Then if our first table gives the dominical letter C, the letter corresponding to the third day of the year—the first Sunday of that year is on January 3d. All the days along the horizontal line commencing with it are Sundays also; and according as the commencing days of the other seven lines (Jan. 1st, Friday; Jan. 2d, Saturday; Jan. 4th, Monday; Jan. 5th, Tuesday; Jan. 6th, Wednesday; Jan. 7th, Thursday) are Mondays, Tuesdays, &c., so are all the dates marked along these lines. Should the year be a leap year, instead of C for the dominical letter we shall have C and D; the first, until the end of February; after which, the second, till the end of the year. It is evident that E would be the letter for next year, F for the next, G for the next, and for the fourth A, B. For in each successive year there are just 52 weeks with one day (two in leap year) remaining over, wherefore the letter is put forward. In our usual tables the dominical letter is printed in capitals, and the other letters—corresponding to the other commencing days—in small characters. The dominical letter was introduced by the primitive Christians in place of the *Nundinal* letters of the Roman calendar.

Double Stars. See Stars.

Draco (*the Dragon.*) One of the old constellations. Its principal stars lie between Ursa Minor, Cepheus, Cygnus, and Hercules. The star, γ Draconis, of the second magnitude, is celebrated as the one used by Bradley in the discovery of aberration. γ and β Draconis are nearly in the line joining Deneb and Arcturus. The extreme star, λ Draconis, between the third and fourth magnitude, is situate very nearly between the pole star and its two pointers. The star, γ Draconis, was so used by Bradley, because it was the largest star passing near his zenith, and therefore fitted to secure at once distinctness of observation, and freedom from refraction.

Ductility is that property of bodies, in virtue of which they may be drawn out in length, while their diameter is diminished, without any fracture or separation of their parts. This property is peculiarly noticeable, and important in the case of the metals. The degree of ductility in bodies is affected—increased or diminished through the agency of heat. Metals are ductile, generally speaking, at any temperature. The degree of ductility appears always, however, in their case, to be influenced by temperature. Some metals, such as brass, and a kind of iron, called red short, are more ductile at ordinary temperatures than when hot. The only general rule which we can apply is, that the heat at which a body is ductile, is less than that at which it is fusible. While the body remains ductile, there is a force joining the particles together, but when it has become fused, all force has ceased to constrain the junction of particles. Ductility is produced in certain masses, as certain *clays*, by their intermixture with water: when they are in the medium state between solidity and fluidity, these bodies are ductile. They form, then, a pretty thick paste. The order of the ductility of the metals is very nearly the following, beginning at the highest:—Gold, silver, platinum, iron, copper, zinc, tin, lead, nickel, palladium, cadmium. This is nearly the same order as that of their malleability. There, is, therefore, some connection between the two properties, but the relation does not hold universally. Iron, for instance, is extremely ductile, but not very malleable. The connection and the difference, are usually explained thus: That in ductile bodies the particles are interwoven, or rather, consist of short fibres, placed side by side, while in malleable, they form little plates, the one kind sliding by their flat surfaces, the other from extremity to extremity. We shall give one or two examples of ductility. When gold is treated by the goldbeater, Reaumur found that a grain of it could be beaten into a plate containing about 36 square inches. So far, it is malleable without difficulty. It is next spread round an ingot of silver, about an inch and a-half in diameter, and then passed repeatedly through the draw-plate, until the thickness of the thread of silver is not above 1-9000th part of the thickness of the ingot. All this time, the gold remains on the silver, and is spread over the surface, without leaving a single spot uncovered, even when viewed microscopically. The grain of gold is therefore spread over a square of 15 ft. in the side. By pressing even this very thin wire between large cylinders, it is very considerably extended. It is to be noted, however, that if beaten violently with a hammer for some minutes, it will become comparatively brittle; and it is then impossible to extend it much farther. The ductility of glass, at a red heat is very remarkable. Threads of the greatest fineness can be made of it. If, for instance, we take a piece of glass, and heat it over a furnace, we may catch some of the heated mass with an iron hook, and draw it away, still adhering to the glass. Lay it now on a wheel and turn this rapidly, when a number of coils of very fine glass thread will encircle it. The form of these threads is elliptical, one diameter being three or four times greater than the other. The flexibility is almost as great as that of the spider's web fibres (which is, by the way, itself a

cellent instance of ductility), and it has been ·n thought possible that articles of apparel ·ght be manufactured from glass fibres, in con- ·uence of their flexibility increasing with the ·nness.

Dynameter. A beautiful little instrument, ·structed for the purpose of determining with · greatest possible precision the effective mag- ·ying power of a telescope. It is well known ·e TELESCOPE,) that this can be obtained by ·iding the focal distance of the object-glass ·the focal distance of the eye-piece. Now, if · look through the eye-glass at the image of ·object-glass, formed at the common focus, the ·meter of the image will bear the same ratio ·he diameter of the object-glass, as the focal ·ance of the eye-glass does to that of the object- ·s. If, therefore, we divide the diameter of ·object-glass, by that of the image so formed, ·shall have the magnifying power. Now, the ·ctive diameter of the object-glass, is one of ·data of any telescope, and readily measured. ·hen, we can obtain an instrument capable of ·suring its image, we shall obtain the magni- ·g power of that telescope. Such an instru- ·t is the dynameter. The simplest method, ·the one which most readily occurs, is to ·e a slip of mother of pearl, carefully gradu- · in, say, hundredths of an inch, near the ·mon focus, adjusting it carefully for distinct ·n, and reading off the number of divisions ·veen the top and bottom of the luminous ·ge. This construction does not, however, ·re a very rigorous determination of the ·nifying power desired. Ramsden was the ·ntor of the double-image dynameter, and ·ond improved his construction. Fig. 1 ·esents the instrument in a side view, and ·2 represents a section of it, through the box ·C D. The graduated circle F, is attached to ·screw-head E, and turns with it. The screw ·nd the screw I both enter the nut, but are ·one right and the other left-handed. They · the same number of turns to the inch, and ·inside screw I moves, within the tubular ·V H, in a thread adapted to it. When, there- ·we turn the nut E, the one screw moves ·ard and the other backward. They separate,

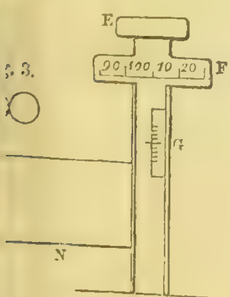


Fig. 1.

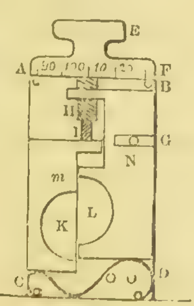


Fig. 2.

·fore, by double the motion of either. Now, ·se the one screw attached to one plate and the other screw to another plate. Each of these plates contain a small semicircular lens, K and L, and when these plates are together, the lenses are together. The movement of the screw separates the centres of these lenses, and the amount of separation is measured by the number of turns, and parts of a turn of the graduated circle F. As, however, it would be troublesome to keep account of these, a very small mother-of-pearl plate, G, is attached outside to one of the inside plates, and is graduated so that one division upon it is passed over in one turn of the screw.—When the dynameter is used, it is held in such a position near the eye-glass whose power we wish to test, that the image of the object-glass may be distinct within it. The screw of the micrometer is then carefully turned, until the two discs which immediately begin to show themselves just touch at their outer extremity. Their position being noted, the same operation is repeated by again turning until the two just touch at the other side, and the number of turns and parts of turns are again noted. As the discs move equally, the space passed over during each motion, will correspond to the magnitude of the disc, and the mean of the two will be that magnitude, corrected for errors of the scale. As it is of great importance that the semicircular lenses should only separate at the line of their centres, not letting that line change in direction, a horse-shoe spring, O, is adapted so as to press against the plates, and to keep them properly adjusted. This micrometrical method admits of very great accuracy. The dynameter is applicable to any telescope on whatever principle constructed.—Although not strictly a *dynameter*, the method proposed by Gauss for determining the magnifying power of a telescope may be mentioned here. If we invert the telescope, and direct the eye-piece toward some distant object, then, on looking through the object-glass, the image of this object will appear as many times reduced in size as it would be magnified by the telescope, if we observed through the eye-piece. The telescope then is directed so that two objects can be distinctly seen through the object-glass in the middle of the field of view, or at equal distances from the optical axis. A theodolite is now directed towards the telescope, so that the optical axes of both coincide, and the angle a, formed by the two foregoing objects, is measured by the theodolite. Next, remove the telescope, and measure with the theodolite the angle A really comprehended between the objects themselves. The required magnifying power, m, is—

$$m = \frac{\tan. \frac{1}{2} A}{\tan. \frac{1}{2} a}$$

or, if A and a are small, it is simply $= \frac{A}{a}$.

Other modes of accomplishing the same object are given in all works on practical optics.

Dynamics. That portion of abstract or purely deductive Science which takes account of the habitudes of a point or body as to Motion, when that body is acted on by any System of Forces, has with great propriety been designated RATIONAL MECHANICS. But the habitudes in question are of two distinct kinds:—either the System of Forces acting on it, will keep the point or body at *rest*, or it will impress on it a definite motion. To determine the conditions and relations of Forces that produce rest, or which are internally *in equilibrio*, is the function of STATICS (*q.v.*): to determine the direction and amount of motion which any System not in equilibrio, will impress on a point or body under any given circumstances, is the province of DYNAMICS. At the foundation of both departments of Inquiry, rest certain recognized results usually termed the LAWS OF MOTION,—a most important subject, discussed in this dictionary under an article with that name. Departing from these Laws, the entire fabric of *Rational Mechanics*—alike *Statics* and *Dynamics*, is essentially deductive.—Without attempting to enter on details, we shall now briefly sketch, under a few heads, the leading contents of *Dynamics*.

(1.) *Dynamics, fundamental conceptions of: Different kinds of Motion; their characteristics and the mode of expressing them.*—There are two grand classes of Motions—*uniform* Motions; and *varied* Motions, or Motions whose rate is *accelerated* or *retarded* according to some function of the *Time* during which they exist. The characteristic of the former class is simple, and easily expressed,—consisting merely in the fact, that *the spaces described by a body in uniform motion are proportional to the times of description:* and the whole habitudes of a body under the influence of a motion or of motions of this description, are immediate results from the LAWS OF MOTION, and their COMPOSITION (*q.v.*) The only point of difficulty, then, connected with the conception and expression of *Motion*, has reference to the case of *Varied, i.e.* of *accelerated* or *retarded* Motions; and it is to this subject alone—one in which we owe the first and all-important step to Galileo—that our present remarks are addressed. These remarks are restricted, further, to the case of accelerated motions; for, since a retardation may be considered as a *negative* acceleration, it is manifest that the laws of the one class are virtually included under the laws of the other. One other preliminary statement: A body, however accelerated, will generally be found moving under the influence of two forces or causes of motion, a force causing an uniform motion, and a force causing a varied motion. These, however, may be separated: *i.e.* the space which the body would pass over in a given time in obedience to the uniform force, may be calculated apart; and also the space it would move over in the same time in obedience to the accelerating force: the sum of these spaces will plainly be the entire space through which the body has moved in that time. The question thus simplified may now be enunciated as follows: Suppose a body to become subject to an accelerated motion, how shall we express or compute its habitudes—viz., its velocity at the end of a given time, and the space it has traversed in that time? The simplest case is that of *uniformly accelerated* motion, or that motion which receives an *equal increment* in an *equal time;* one, however, already so fully discussed under article ACCELERATED MOTION, that we can dispense with further elucidation here. The laws, as there unfolded, are certain, definite, and manageable, so that in so far as this fundamental conception of Dynamics is concerned, or the modes of expressing and dealing with it, there remains nothing to be desired. It is the case, as the sagacity of Galileo discerned, of *falling bodies;* and, indeed, the only form in which accelerated motion is presented by the phenomena of Nature. All other and more complex descriptions of accelerated motion,—motions in which the acceleration takes place according to other functions of the *time*,—are, therefore, purely hypothetical; but should consideration or discussion of any such ever be required, the *Infinitesimal Calculus* will be found quite as capable of representing and discussing them, as it is with regard to the actual case just spoken of. No difficulty, whatever, therefore, now encumbers this foundation of general Dynamics; so that we may proceed to survey briefly the nature of the solutions provided by that Science, for all problems concerning the habitudes of bodies subjected in any way to such motions.

(2.) *Theoretical Dynamics. Part I.—The Motion, free or constrained, of* POINTS.—On entering the inquiry concerning the habitudes of Points in Motion, it is easy to discern that it contains two problems—a *direct* and an *inverse*. It may be required, for instance, to determine the shape of the path, which a point must describe when partaking of various given motions; or, the shape of the path being given, it may be required to determine those separate motions or forces under influence of which the point may describe that path. An instance of the first problem is this,—given the impulse which a projectile receives, and assuming the laws of falling bodies, to determine the curve described by the projectile? An instance of the second problem is presented by the phenomena of the solar system, viz: the planets describe *ellipses* around the sun,—required the forces that cause them to describe that path. Throughout the whole of the Science of Dynamics, this division into direct and inverse problems necessarily prevails; but as the inverse solution depends for its methods on the conceptions contained in the *direct* one, we shall restrict ourselves here to consideration of the latter. It ought to be remarked, however, that it was in laying the foundation, and in so far completing

solution of the inverse problem, that Huy- ;ns and Newton obtained their unwithering lau- :. A Point, under the influence of continuous :es, may either be *free* to move through space :hese forces shall direct it, or it may be *con- ained*, *i.e.* it may be restricted to motion on ıe plane or curved surface, to which it is at- hed, (for instance, it may be free only to slide ng the surface of a table, or the convex sur- : of a globe, &c.) Hence two classes of In- ries regarding the motion or the path of a nt. Dynamics furnish the complete solution ooth.—I. The method of treating the case of ree Point, which is now generally adopted, lue to Euler. Taking each force separately, *instantaneous action* of that force on the point molecule, is decomposed—by the theorem of decomposition of Forces—into three others, ıg the three usual rectangular co-ordinates: by adding together the corresponding com- ents of all the actual forces, we reduce them to :e continuous forces acting on the molecule, ıg these co-ordinates. If, then, the total force ıg the axis x be named X; the force along axis y, be named Y; and that along z, Z; immediately obtain, in accordance with the s of accelerated motion, the following equa- for any time t,

$$\frac{d^2x}{dt^2} = X. \qquad \frac{d^2y}{dt^2} = Y. \qquad \frac{d^2z}{dt^2} = Z.$$

l also—

$$v^2 = 2 \int (X\,dx + Y\,dy + Z\,dz),$$

ı which equations, the co-ordinates which *e the nature of the curve*, and the law of the city of the molecule in it, may be deduced nere analytical methods. In the more com- : cases, the analytical difficulties (like every- g depending on *integration*,) are often very t: indeed, it has been, by the efforts requi- to overcome such difficulties, as presented dynamical problems, that Analysis itself ıly advanced:—the purely-dynamical por- of the problems rarely opposes serious ob- les to the inquirer.—II. If the point is not but so attached, let us say, to a curve sur- that it cannot depart from that surface, the lem becomes more complex, although there ss difference between the two cases than at appears. We have only, indeed, to account *ne* of the continuous forces, acting on the ecule, the *total resistance* offered by the given ce: and the one problem then becomes iden- with the other. The difficulty resides in the t analysis of that resistance. Now this consists vo parts quite distinct, and which may be dis- uished as the *Statical* resistance, and the *Dy- ical* resistance. The statical resistance is that h would take place if the molecule remained ionless; it is the resistance of the surface to pressure of the molecule upon it at any instant; and, therefore, must be effectively equal and opposite to the energy with which the continuous force that acts on the molecule, presses it down on the curve at that instant—an energy easily expressed by aid of the theorem of the Composition of Forces. The *dynamical* resistance, on the other hand, has quite a different origin; it is engendered by the *motion* of the point; and results from the tendency of every body to abandon the curve it is forced to describe, and to follow, at every instant, in obedience to the First Law of Motion, the direction of its *tangent.* This peculiar resistance is, of course, always and necessarily manifested, when the point passes from one element of the curve surface to the next element; and a few simple considerations enable us to arrive at the theorem, that the *centrifugal force* is always equal to the square of the effective velocity of the moving point, divided by the radius of curvature of the curve surface at the instant in question. The *direction* of this special form of resistance is as easily obtained as its *intensity:* and when it is compounded with the *statical* resistance described above, we have that total new force, to which, because of its connection with the surface, or because of its *constraint*, the point or molecule is subjected. Joining that force of resistance to the other continuous forces, we reduce, as already stated, the case of a *constrained*, to that of a *free* Point.

(3.) *Theoretical Dynamics.* Part II.—*The Motion of a rigid system of Points or of a* Solid Body.—The problem just analyzed is plainly nothing more than an abstract one; and its value consists in its being the necessary foundation of all actual or real inquiries. Nothing exists in nature, similar to the motion of free or constrained *points.* We have to do, in real exigencies, with *bodies*, or rigid systems of points; and it is quite clear that no *body* in motion can be treated as a simple point, just because the mutual actions and reactions of the several parts or points of which it is composed, must necessarily modify, and—it may be—greatly change the motion that would be assumed by any of these points taken separately, and affected by the continuous forces at work upon the bodies. To rise from the abstract problem to the real one is, therefore, the aim of this second section of Theoretical Dynamics; and the honour of establishing a general and adequate method is due to D'Alembert. Previous to the labours of the great French geometer, each new set of problems belonging to this class was treated by aid of a separate general principle—as it was called—some principle peculiarly applicable to the questions under consideration; and this was the origin and object of all those separate *principles* in Dynamics, which are in reality only general *theorems*, well adapted to the treatment of the kind of problems in which they primarily originated. D'Alembert put an end to the reign of these special theorems, by taking account directly of the dynamical meaning of the action

and reaction of moving rigid systems of points, and expressing that in a general equation. See ALEMBERT, D', PRINCIPLE OF. The character of this famous principle may be stated once more. "When, by the reactions which several points exert on one another through effect of their connection, each of them takes on a motion in obedience to external forces, different from what it would have done, were it single and *free*, this *free* movement may be considered as composed of two others—the movement which actually takes place, and the movement which, through that connection or constraint, has been destroyed. Now these destroyed movements will be found, in every system, to be of a contrary character,—some of the points actually moving swifter than they could have done *freely*, and others more slowly. D'Alembert's principle is that these destroyed quantities of motion—the one set *positive*, the other *negative*—necessarily balance each other." The student will easily recognize that, by aid of a principle so simple and comprehensive as this, all problems involving the action and reaction of a system of rigid points, are at once reduced—in as far as these reactions or that rigidity is concerned—within the sphere of problems of equilibrium; and Lagrange accordingly, in his immortal work, the *Mecanique Analytique*, has combined D'Alembert's theorem, with the Statical *Principle of Virtual Velocities*,—thus raising the science of Dynamics to its highest logical perfection. We may conclude, therefore, with regard to this second part of Theoretical Dynamics likewise, that it now presents no speculative difficulty.

(4.) *Theoretical Dynamics.* Part III. *Motions of Rotation.*—A system of rigid points may be subject to two descriptions of motions—to one or the other, or both,—that in themselves are altogether distinct. It may undergo *translation* through space; or it may *rotate* around some point within itself; or, while being *translated*, it may also *rotate*. The habitudes of the body as to both motions, may indeed be investigated by one general method; but the consideration of these motions *apart*, has recently tended greatly to the simplification and rationalizing alike of Statics and Dynamics; and the method of doing this is unquestionably due to M. Poinsot. The way to this signal advance in method, originated with the great and ever-fertile Euler; so that, after his time, the complete analysis of the movement of any system, acted on by any forces, consisted in determining, *first*, the velocity of its centre of gravity and the direction along which it is being translated; and, *secondly*, the direction, at each instant, of the spontaneous axis of rotation passing through the centre of gravity, and the velocity of the rotation of the system around that axis. To effect this latter task *directly*, Poinsot proposed his theory of *Couples*, and thereby gave a positive and real significance to three of the six general equations of Equilibrium. The real bearing and significance of Poinsot's doctrine are exposed fully under articles COUPLES and STATICS, *q.v.* And the student is especially referred to recent memoirs on Rotation, by that perspicuous writer, for evidence of the great simplicity with which a subject, once so difficult and even intractable, is now invested.

(5.) *Dynamics, General Theorems in.*—Reference has been made above, to those theorems—*special* theorems they ought to be termed—which in the course of the progress of science have been welcomed and rightly valued because of their applicability to various classes of researches. The following are the chief that belong to Dynamics:—1. The principle of the *Conservation of the Movement of the Centre of Gravity*,—discovered by Newton, and demonstrated at the beginning of the *Principia*.—2. The principle of *Areas*,—the first idea of which we owe to John Kepler, and which, as generalized by D'Arcy, must be accounted one of the bases of our Celestial Mechanics.—3. The principles of *Moments of Inertia*, and of the *principal Axes*, discovered by Euler.—4. The *Conservation of Living Forces* (*virium vivarum*), suggested by Huyghens and completed by John and Daniel Bernouilli.—5. The principle of Maupertuis, erroneously named the *Principle of the least Action.*—And, 6th and last, The principle of the *co-existence of small oscillations*, a very admirable discovery by Daniel Bernouilli.—All these, as well as certain statical theorems of the same order, will be found explained under appropriate heads in this volume.

(6.) *Dynamics, Practical Problems of.*—The student has now been presented with an outline of the principles and methods of pure or abstract Dynamics. We shall merely enumerate, in conclusion, a few of the actual problems to which these methods are applied. *First*, we have the subjects of *Impact* and *Collision of Bodies*. *Secondly*, the theory of *Projectiles*. *Thirdly*, such problems as those of the descent of bodies in *Planes* and *Curves*, the characteristics of motion and oscillation through *Cycloidal* arcs, the *tautochronous Curve*, the *Curve of Equal Pressure*, &c., &c. *Fourthly*, the entire domain of the Celestial Mechanics, which abounds, in every portion of it, with illustrations of the inverse as well as the direct problem of *Central Forces*. *Fifthly*, problems connected with oscillation and motion about a fixed axis, such as the *Compound Pendulum*; the *Centres of Oscillation* and *Percussion*; permanent *Axes of Rotation*; the *Figures of Rotatory Bodies*; Axis of *Spontaneous Rotation*, *Impulsive Action on a Rigid Body*; the action of *Resisting Media*, &c., &c. And, *Sixthly*, the most difficult and imperfect of all—the subject of *Hydrodynamics*. It cannot be doubted that the general formulæ of pure Dynamics are applicable to the movements of any set of particles, whether these constitute a fluid, a gaseous, or solid body; and that, to insure their practic

ılication, we only require to know and to be e to express aright, the relations connecting se particles among themselves. The real per-xity of the case of fluids and gases rests in ling an expression of this sort that will be at :e correct and *manageable;* and it must be fessed that, when an attempt is made to ex-ss fully the condition of liquid particles, the alting equations are so unmanageable that meters have been fain to resort to hypotheses resenting the fluid or gaseous condition so perfectly, that no faith can be placed in the ılts obtained. The general science of Hydro-namics remains, therefore, considered as a ctical one, still in its infancy; and we are iged to deduce its theorems and laws, by pro-ses that are purely *empirical.*—See the various cles indicated above. The student who desires oursue the subject is recommended in the first ce to the *Mechanical Euclid* of Dr. Whewell, treatise on *Statics* by Poinsot, the two ex-lent and perspicuous volumes on *Statics* and *amics* by Mr. Earnshaw; and the *Mecanique* Duhamel. Other valuable works on the sub- as a whole, as well as on its several parts, und alike in French, and in our English ntific Literature.

Dynamics, Vital, is the application of the ıciples of the Dynamical Theory of Heat HEAT) to organisms endowed with life, ether animal or vegetable. Animals are re-ded as machines for the economic develop-ıt of energy; plants, as storehouses of energy. natural tendency of the mind is to view ng creatures as original centres of force—ter being permeated by a peculiar, indepen-t power, flowing from life. But life is rather spenser than a source of force. Left to itself, ınguishes and ultimately dies out. Its stores extraneous, in the food we eat. So long this is supplied, the animal developes force, ther of heat or of work, and in exact pro-ion to the supply. When we move a leg ırm, we use only the common mechanical ıngement of bands and pulleys, by which we ıld communicate a similar motion to a piece ead matter. As we can trace the motion of straps and wheels of a factory to the chemi- action going on in the furnace beneath the er, so we can see the origin of the force which es the bands and pulleys of the animal hine in the chemical action to which the is subjected. In the former case, however, y step in the transformation of energies is r; in the latter, the mysterious conservative er of life interrupts our analysis. This che-al action seems divisible into *general* and *ial.* General chemical action results chiefly eat, and is independent of the will: special hat which accompanies muscular exertion, is usually confined to the seat of this exer-. The relation of general and special chemi- action to each other and to the evolution of force is not so well ascertained. Animals form one of three media by which chemical force may be evolved—the other two being the electro-magnetic engine and the various forms of thermal engines. To produce ultimate useful work by the electro-magnetic engine, chemical force assumes the form of electric force; while in thermal engines it becomes primarily heat. Does the energy of food act through either of these media, electricity or heat? or does it pass directly into the *vis viva* of animal and external bodies? There is nothing in the known pheno-mena of muscular action to support the latter supposition; and, tried by scientific principles, it is in no way probable. We have no right to introduce a new force, because we cannot clearly see how acknowledged forces can intervene. This would be a return to the profitless system of occult qualities. Is it, then, energy of heat or of electricity which is evolved from animal food, and placed at the disposal of the animal will? "There are at present known," says Thomson, "two, and only two, distinct ways in which mechanical effect can be obtained from heat. One of these is by means of the alterations of volume which bodies may experience through the action of heat; the other is through the medium of electric agency." (*Transactions of Royal Society of Edinburgh*, vol. xvi., p. 542.) But there is no arrangement for thermal expansions and con-tractions in the body. Muscles generate instead of destroying heat by contraction. Similar rea-sons may be adduced against the existence of thermo-electric currents in the tissues; and one objection bears against both, that all experiment proves that the chemical forces never develope a *thermal* equivalent in the body. We are, there-fore, in the present state of our knowledge, shut up to the conclusion that electricity is the form which the potential energy of food assumes before appearing as visible mechanical effect. Are there any physiological facts in support of this rather abstract conclusion? We know that in every part of the body chemical processes are advancing. We know also, from Faraday's electric theory of chemical combination, that there must therefore be large quantities of electricity present; but whether there is a physical arrangement for "loosening the electricity from its habitation for awhile, and conveying it from place to place," is a question not so easily answered. A full consideration would lead us into all the details of animal electricity, but we shall confine our-selves to such remarks as are necessary for pre-sent application. (1.) That the existence of the conformation of bodily parts requisite for the liberation of electricity is quite consistent with the animal economy, we know from its actual presence in various animals, *e. g.*, the *torpedo*, *gymnotus*, &c. Matteucci found in the torpedo a regular electric series of cells, with positive and negative poles; an electric lobe in the brain, from which nerves ramify over these cells, placing

the time and extent of their action under the control of the creature's will. The activity of the electric function is proportioned to that of the circulation and respiration. True, this electricity is applied by the torpedo, and similar animals, to purposes of attack and defence, and not to motion; but still the discharge is entirely under their control, and an electric current may, in other animals, produce muscular contraction and external work. But (2.) free electricity does undeniably circulate in all muscular tissue. We cannot certify ourselves so thoroughly of its source, but the fact of its presence is certain. We are apt to forget the fact in the fanciful and strained theories sometimes set forth in explanation of it. Its amount varies with the mechanical duty of the muscle, and with all the circumstances of circulation, respiration, external temperature, &c., which modify muscular action. At the moment of contraction the current almost entirely ceases, as happens when an electro-magnetic engine begins to work. In each case electricity ceases to exist as such, and passes into the form of mechanical effect. (3.) It is also found that no artificially applied agent excites muscular tissue so strongly as electricity. Matteucci has proved that a given quantity of zinc, consumed in a battery, can be made to do much more work by occasioning muscular contraction, than by turning an electro-magnetic engine. Opinions may differ as to the import of these facts; but while an inquiry into the mode in which chemical becomes muscular force, is necessary to the completeness of a theory of vital dynamics, it does not involve its accuracy. This depends on the establishment of an equivalence between the potential energy of the food and the actual energy of animal heat and work done. In thermo and electro-dynamics, it is easy to exhibit the equation; but here we do not deal with a mere engine, but with a self-adjusting variable organism. The act of overcoming external resistance quickens breathing and circulation. Thus the formula for work done by an animal in any given case becomes complicated.

$$W = J \frac{\theta\,(a - b) - H}{\theta\, a}$$

where a is the oxygen consumed during motion, b during rest; H the increase in animal heat following motion; θ the thermal equivalent of the combination of a unit of oxygen with the food; J the mechanical equivalent of a unit of heat. That special chemical action $(a - b)$ is almost entirely productive of work, Matteucci proved in the case of the frog. Estimating its mechanical value in the contraction of the gastrocnemian muscles of that animal, he found the work done bear to it the high proportion of ·878. The calculation of the element H would have completed the equation. The first application of these principles, though not of this formula exactly, which is difficult to follow in actual experiment, was made by Messrs. Scoresby and Joule in 1846. (*Philosophical Magazine*, 1846.) According to Watt, the value of one horse-power for one day is 24,000,000 foot-pounds. To sustain a horse in working condition, 12 lbs. hay and 12 lbs. corn are necessary per day; or one grain of this mixed food enables him to raise 143 lbs. to the height of one foot. These gentlemen found that when burned, one grain raised the temperature of 1 lb. of water 0·682° Fahr., a thermal effect dynamically equal to 527 lbs. raised to the height of one foot. The horse, therefore, converts ·27, or nearly one-fourth, of the entire *vis viva* of his food into useful mechanical effect. From the absence of any definite standard estimate of man-power, calculations as to its economy are not so determinate. Thomson seems to have made the first attempt. (*Philosophical Magazine*, S. 4, vol. iv.) He thinks it probable that a man, by walking up-hill eight hours a-day, may turn one-sixth of the mechanical value of his food into work, and one-fourth by such labour as pumping for six hours per day. In the former case, the work is foot-pounds of his own body; in the latter, foot-pounds of water raised. The general conclusion of Scoresby and Joule is, "that the animal frame, though destined to fulfil so many other ends, is, as an engine, more perfect in the economy of *vis viva* than the best of human contrivances." The student will find materials for more elaborate calculations in Gavarret's treatise, *De la chaleur produite par les etres vivants.*—It is proved from these investigations that the chemical forces of food are the source of the energy of all animals. But whence this chemical force? Since the ultimate food of all animals is vegetable, this leads us to the *Vital Dynamics of Plants.* The growth of plants consists chiefly in a decomposition, against the force of chemical affinity, of carbonic acid, water, and ammonia, imbibed from earth and air. This is, in fact, an *unburning* of the fire which has burned on our hearths, in our furnaces, and in the bodies of animals—a restoration of the materials there consumed to their former state of combustibility. The potential energy of carbon, hydrogen, and oxygen, is returned; or, in other words, actual energy has been expended in separating these elements. Actual energy of what? *Vegetable respiration*, as this process is called, the converse of animal, has been proved by the experiments of Ingenhouz, Senebier, and other investigators, to be a process which depends on light. It ceases on the coming on of darkness, and there supervenes a slight reverse action, one of combustion. It is therefore actual energy of sunlight which overcomes chemical force, and becomes potential energy, to be released as actual energy once more in our fires and bodies. When we decompose water, *i. e.*, separate oxygen and hydrogen by force of electricity, we exchange electric power for the tendency of these gases to unite (potential energy), and this tendency is

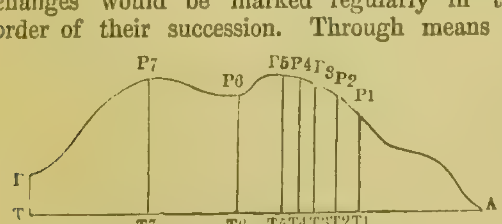
tisfied by applying a light to the gaseous mix-re. An explosion ensues. We get water as fore, but besides, heat the equivalent of the ctricity. So the force of a small fraction of e infinitesimal luminiferous wavelets which ginate in the sun is made latent in the affi-ies of separated oxygen, hydrogen, and carbon. hen these reunite, we get carbonic acid, and ater as before, but besides, heat, or electric or al force, the equivalent of the light-power ex-aded. An ingenious calculation of the amount sunlight and heat which is thus economized s been made by Professor Thomson (*Phil. ig.*, S. 4, vol. iv.) Estimating the mechanical lue of the annual produce of an acre of forest d from Liebig's data, and the mechanical ue from Pouillet's data, of the sunlight falling that space at the latitude of Giessin, he finds at "probably a good deal more than $\frac{1}{1000}$ of solar heat which actually falls on growing nts, is converted into mechanical effects." re certainly is poetry enough thrown around commonest objects—our coal fields, our street ps, our study oil, our home hearths, our aaces; for the sunshine which fell on an un-pled earth lights us and works for us with sunshine of to-day.

Dynamometer. An instrument constructed neasure the work done in overcoming a given stance and causing a given motion. The name nifies a measure of power or force. The instru-nt is rather a measure of effort or work. We ceive a force as being something instantaneous, ny moment changeable, beginning or ceasing. the other hand, we conceive of work, as the ation—through spaces of time—of forces. If pull a body, for example, along a level road, exercise a certain force every moment that we inue our work; but the work, though de-ding on the force, does not depend on it alone. or instance, we pull another body with only the force, over twice the distance, we do the e amount of work. Work, therefore, depends the force applied and the space through ch the body on which it is performed moves; the dynamometer proposes to measure the k, in any case of resistance overcome and ion produced. Assuming the obvious prin-, that work will be measurable by the pro- of the force, if kept uniformly acting, into space through which it acts, we may come to ipon what dynamometers must depend. They ire to provide a measure of the force, and easure also of the space through which the acts. If the force continue uniform during whole action, we should find the problem suf-ntly easy. The space traversed by a moving is easily measured, and very many con-inces might be easily applied to such a body ake it self-registering as to this point. If, we are able to measure the force with which oves, the desired result will be obtained. If we e it to act, for example, in pulling out a spring, we have a measure, in the distance to which the spring is drawn out, compared with the amount of *gravity* capable of drawing it out to the same distance, when a weight is hung upon it. We have then simply to multiply the two values just discovered one by the other, for the required result. The original dynamometers (as Reg-nier's) limited themselves to determining the value of the force, and left out the other element as unimportant. And, in fact, it might be consi-dered to be so pretty fairly, were the usual case such as we have described. If the force remained stedfast, we should have merely to measure the whole space and the one force. But in ordinary mechanical cases, our working forces do not re-main entirely, in many not even approximately the same, throughout intervals during which we desire to measure the work. We have, therefore, to take the small spaces during which they do remain comparatively stedfast, and to multiply these by the forces operating during motion through these spaces, and sum up the amounts of work obtained in each, in order to arrive at the total value. If all these small spaces, during which the force may be considered constant, were equal, we might still adhere to the measurement of forces merely. We could take the average of all the forces successively measured, and multiply the average by the total space. In fact, calling F', F'', &c. the forces, and s the small spaces, ($M\ s$ being the whole space, which is divided into M parts all equal), we might estimate the work as already shown by $F'\ s + F''\ s + F'''\ s +$ &c. $= s\ (F' + F'' + F'''$ &c.) The method just indicated takes for this measure $M\ s \times$

$$\frac{F' + F'' + F''' + \&c.}{M} = s\ (F' + F'' + F''' \ \&c.)$$

which is, therefore, equal to the true value of the work. But as $M\ s$, the whole space, is easily measur-able separately, these original inventions, confined to measurements of F', F'', &c. would therefore have given very simply the results required. Leroy's construction of dynamometers was exactly like our present letter weights. The force whose measure was required, was made to push a spiral spring inside a tube, downwards; and then com-pared with the force of gravity acting on known weights able to compress the spring to the same ex-tent. Regnier's construction was an elliptical spring, to be pushed into greater curvature or drawn into less by the force applied; and latterly, two plates of steel, one of which was kept fixed, and the other moved from or to it by the force. These constructions—though perhaps both susceptible of mechanical improvements, which they never at-tained—become immediately incapable of mea-suring work when the spaces through which the inconstant forces move become themselves incon-stant. The expression for the total work—call-ing s', s'', s''', &c. the successive unequal spaces, becomes $F'\ s' + F''\ s'' + F'''\ s'''$,&c., an expres-sion which requires, for the determination of its total value, a simultaneous measurement of the

quantities indicated by F', &c., and S', &c. And a new difficulty likewise occurs. In ordinary cases, the variations of these two inconstants are very rapid—so rapid that we could not read them off, even if noted by the instrument, without inconceivable labour. It is therefore essential that our instruments should themselves be made to keep a register, afterwards to be examined at leisure, of the values which they render perceptible. If, for example, we can succeed in making our dynamometer put a mark down of a certain length proportional to the force acting at the time, for every force which it measures, and if we can get it to move the paper, or ourselves move it, so that these marks will be placed in the true order of succession of these forces, we will have a sheet, when the paper is taken out, exhibiting the series of the F quantities. If, still further, we have a simultaneous arrangement which shall mark the spaces through which motion has taken place, we shall draw out a sheet similarly marked with the successive values of the quantities S; and taking the two series together, we shall obtain $F' S' + F'' S'' + F''' S'''$, &c., or the whole amount of work done. We shall endeavour to indicate in a general way how various dynamometers accomplish these objects. The ordinary spring dynamometer consists of two rims of steel of the parabolic form—the one kept fixed in the instrument—the other fastened to it at the two extremities, but drawn out from it to certain distances by the successive forces. The distance of the middle points of the two springs, when the one is pulled out in this way, indicates the amount of force. If a pencil be attached to the centre of the moveable spring, it will move with it, and indicate this distance. But this would give us only a pencil moving up and down a straight line; and neither the various successive values of the forces, nor the order of their succession, could be at all distinctly indicated. If, however, the paper was made to move beneath the pencil, the distance between the two points might then be given continuously; and all changes would be marked regularly in the order of their succession. Through means of



such an arrangement, we might have such a figure described by the pencils as above. The curved line would be described by the pencil attached to the moveable, and the straight line by one attached to the straight spring. The perpendiculars $P^1\ T^1$, $P^2\ T^2$, $P^3\ T^3$, &c., represent the successive forces, and the distances $T^1\ T^2$, $T^2\ T^3$, &c., the distances for which they approximately act. If we take a very narrow step like $T^1\ T^2$ for the space, the force may be considered as either $P^1\ T^1$ uniformly, or as $P^2\ T^2$ for these lines, and the inner ones representing the forces will differ excessively little from one another. The area, then, which we may call $T^1\ F^1\ S^1\ S^1$, may be expressed by $P^1\ T^1\ P^2\ T^2$. Similarly, the area $F''\ S''$ might be expressed by $P^2\ T^2\ P^3\ T^3$, and so on. Hence, the whole sum of the work done in any given space would be the sum of the similar rectangles, &c., throughout that space. It is very clear that the smaller we take these spaces, and the more of them, therefore, the more correct will our appreciation be of the total effect, and that each approximation will come, therefore, nearer to giving a sum equal to the value of the area enclosed between the line T A and the curve A P. The area of the curve will, therefore, give an equivalent for the whole work, and the area between any two of the quantities P T will give the similar equivalent for the portion corresponding. Sometimes it is of more importance to get the average force wrought with during a given time than the total work done. In this case, the method found useful is to put a chronometrical arrangement in connection with the machine, so that the paper may be drawn uniformly. And it might be without difficulty demonstrated, that the area of the curve divided by the length A T would give the mean force for the whole action. This curve would now, however,—unless there had been uniform motion before—be a different one from the curve A P, though similarly described; and, therefore, except in this case of uniform motion, the value of the force, or even of the average force, would not be a sufficient index of the total work done during a given space. Such is the principle upon which the dynamometer always depends. Peculiar modifications of the arrangements described would be necessary to fit it for application to rotatory motions.—The parabolic form is used for the spring commonly employed in the dynamometer, in order that the force may be as equally as possible distributed over the curve, and that the resistance may be proportioned to the force.—Watts' Indicator is a species of dynamometer of special application, which we shall describe under that word.

Ear. Little is known regarding the physical nificance of that singular and apparently comx apparatus which constitutes the *Ear.* In n, as with most animals gifted with hear-, it consists of an outer organ, that may ve some such purpose as an ear-trumpet, viz., collect sound; next, of a tube through which vibrations pass inwards; then, of that elastic nbrane, or drum of the ear, which would seem sitive to all impulses; and lastly of a cell and nain of small, singularly-shaped, and unintelble bones, conducting towards the place where es are first found of the presence of the audi- nerve. Anatomy and Physiology have debed this remarkable apparatus, but they have wn no light whatever on the relation and pose of its various parts; nor can physical nce at present offer them the slightest aid. in whatever way its constituent parts may it cannot be doubted that the human ear is nstrument of remarkable delicacy and great pass. All sounds, it is well known, travel ugh space with the same velocity. They r only in the lengths of the waves or undums that occasion them—the lengths increasing ne *graveness* of the sound increases, and *vice a.* Now, it has been distinctly established id of Cagniard la Tour's Siren, and Savart's ngement of *toothed wheels*, that the gravest west sound which the human ear can ordily discern, is one made by a body vibrating *fourteen* or *fifteen* times in a *second;* while *acutest* audible sound demands for its produc- no fewer than *forty-eight thousand vibra-* in the same space of one *second.* The imse interval between these numbers, is the sure of the range of the phenomena of sound, hich our organ of hearing can take cogniz-. The interval is vast indeed; and within e the causes of those sounds which Nature s, and all the innumerable Harmonies and odies evolved by Scientific Art. Still, that h we discern is only one domain of Sound. Ear penetrates so low, that few grave sounds exist, of the recognition of which it is incap-; but, at the other end of the scale, how t and even interminable the range from h it is shut out! Dr. Wollaston was cerly correct, that the Ear, fine as it is, as well, aps, as every other human sense, is adjusted ne discernment only of one finite and allotted within an infinite Universe.

arth, The, ♁ —the globe we inhabit; the l planet in order from the Sun; and the est within the belt or ring of the Asteroids. equatorial diameter is 7,926 miles; its polar, 8 miles; and in density, it is 5·67 times ier than water. The *mass* of the Earth, pared with that of the Sun, is ·0000028173. mean distance from the central luminary is about 95 millions of miles—a space through which light occupies 8 minutes 13·3 seconds in travelling; but, as the eccentricity of its orbit is 0·0167751, if this mean distance be termed 1, the distance of the Earth at aphelion will be 1·0168, while at perehelion it is only ·9832. The quantities of light received by it in these opposite positions are in the proportion of 0·967 : 1·034. The period of the revolution of our globe in its elliptic orbit is 365·2563744 mean solar days, or 365 days, 6 hours, 9 minutes, and 10·75 seconds. Its *tropical* revolution, or the period elapsing between vernal equinox and vernal equinox (that is, the period of the revolution of the *seasons*) is 365·24222 mean solar days, or 365 days, 5 hours, 48 minutes, 47·81 seconds. The Earth rotates on its axis in one *sidereal day*, or 24 sidereal hours; that is, in 23 hours, 56 minutes, and 4 seconds of *mean time.*—We proceed to refer briefly to the points of chief interest concerning our Planet's *motions*, its *magnitude* and *figure*, its *density*, and its *temperature.*

(1.) *The Earth, Motions of.*—The chief of these are the annual *Revolution* in an orbit, and the diurnal *Rotation* on an axis. The Earth's velocity in its orbit varies, according to Kepler's Law of the Conservation of *Areas*, modified by certain *perturbations*, of which an account will be found in detailed works on Astronomy. The angular velocity of Rotation is absolutely uniform. Besides these, certain motions affect the Earth's axis itself, the nature of which is explained under PRECESSION and NUTATION. The point to which we desire to draw attention here is this—How is the reality of these motions established? Living on the surface of the Earth, and, therefore, moving through space along with it, we cannot *see* these motions *directly.* How, then, are we assured of their existence as a *fact*, as distinguished from a serviceable *hypothesis?* If these motions are real, doubtless they will impress *apparent motions* on all bodies external to the Earth. For instance, the rotation of our globe on its axis would produce an apparent diurnal revolution of the celestial vault in the opposite direction; and so, with our motion of revolution. Such external motions undoubtedly exist. That grand and familiar daily and uniform revolution of the whole sidereal host, is the very phenomenon which would manifest to a spectator on the Earth the rotation of his own globe. Nevertheless, how can it be ascertained that the revolution of the stars is an *apparent* one?—by what kind of considerations are we entitled to attribute it, to an actual but unseen and unfelt Rotation of the Earth as its *cause?* When the immortal Copernicus first attacked the time-consecrated conception, that our globe is motionless, and the prime centre of all things, he could indeed propose his own view only as the *more*

probable hypothesis. It was easy to show that the position of our world as a planet moving around the Sun, demolished at one blow the whole of that artificial Ptolemaic machinery—a machinery that in the hands of Purbach had acquired a complicacy with which imagination could never cope; and surely it seemed easier to conceive one little globe turning daily around an axis, than that all these myriads of stars should partake of one and the same motion, with a regularity so rigorous and unbroken. The demonstration of the *fact*, however, was not within reach of science, in these early days, but rather has been reserved in the main for our own. —I. The first actual verification of the orbital motion of the Earth is due to Bradley. His capital discovery of *Aberration*—a discovery already described in this Dictionary (ABERRATION),—furnished a proof almost decisive of the Earth's planetary motion; at least phenomena were detected, and in this way finely accounted for, that could be explained in no wise by the conflicting theory. One difficulty—the difficulty started by the eminently *practical* Tycho—alone remained. If the Earth shifts its position in space, so that at one season of the year it is removed by the immense distance of 190,000,000 miles from its position at the opposite season, why—as the illustrious Observer of *Uraniburg* inquired—do the fixed stars always appear in the *same direction* from us—why have these orbs no *parallax?* The objection could not in any case have been conclusive, because the allegation on which it rests, had no title to be received as absolute. Tycho had no right to assert that the fixed stars *have* no parallax: all that he knew was that his instruments did not enable him to detect any. But the actual discovery of parallax—that greatest triumph of modern observation—(see STARS, FIXED)—has established a positive argument on behalf of the orbital motion of our Planet, of irresistible force. In so far as this motion is concerned, then, the Copernican theory rests on mere hypothesis no longer.—II. In reference to the Earth's *Rotation*, that too has received, although only in very recent times, a demonstration quite as full, and even more palpable and convincing. The existence of a *diurnal aberration*, of which there can be no doubt, might have seemed equally applicable in this case; but it never could be rendered satisfactory, because of its very small amount.. All that is needed, however, has recently been amply attained, through a consideration of the necessary *influence of the Earth's rotation, on the apparent motion of bodies situated on its surface.* This has been made manifest in three ways:—(1.) Were the globe motionless, or not rotating, it is clear that a body let fall from the summit of any perpendicular altitude, would fall to the surface, exactly at its foot. But if rotation exists, this will not hold. A body at A, for instance, must, when let fall, continue animated during its fall to the surface c′ by the horizontal velocity which it possessed while attached to the top of the height c A: in other words, if A A′ be the space through which, in virtue of the Earth's rotation, the point A would move in the time that a detached part of it would take to fall through A c, that detached part will retain, though detached, its original velocity, and when it reaches the surface will have moved over c B, a space equal to A A′. But c, the foot of the perpendicular height, will (as is evident from inspection) have moved in that time only through c c′: so that, when the body falls, it will not be at the foot of the tower, but in advance of it by a quantity c′ B. The necessity of such a deviation towards the east has long been discerned; but the credit of establishing its reality, is due to M. Reich of Freyberg. Taking advantage of a mine of the depth of 520 feet, he repeated experiments in the most careful manner, and quite established the existence of the deviation; and the satisfactory character of the result appeared further from this, that the *amount* of the observed easterly deviation differed from the theoretical amount only by the inconsiderable quantity of ·03 of an inch.—(2.) Still more striking, however, are the most ingenious methods of M. Foucault. The sensation is not yet forgotten that was created by his famous *pendulum experiment.* Its principles are as follows: Suppose a pendulum suspended *over either Pole* of the Earth from a point detached from the Earth and set in oscillation. The rotation of the Earth beneath it, can in nowise affect or alter the *plane* of that oscillation; but as a spectator on the Earth, carried round by the rotation of the Earth, would pass first under one end of the pendulum, then under the other, the plane of oscillation would necessarily appear to him to *make a revolution from east to west* in the precise time in which the Earth really rotates from west to east. It is not necessary that the pendulum be suspended from a point detached from the Earth. The attachment of the point of suspension to the Earth, provided it be over the Pole, would only twist round the wire, or cord forming the rod of the pendulum, and make the *ball* itself rotate on its axis in the course of a sidereal day. *Next*, imagine the Pendulum suspended *over the Equator*. It is equally clear that no apparent revolution or change of the plane of oscillation could take place in this case, just because the spectator at the Equator could not, by being carried round, be brought nearer one end of the plane of oscillation than the other. *Lastly*, let us examine the case of a pendulum at a

Fig. 1.

·rmediate Latitude A, E O being the Equator, P O the polar axis of the Earth. The actual

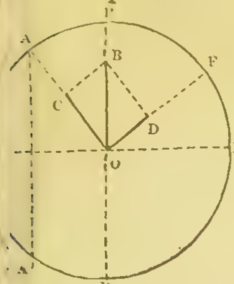


Fig. 2.

rotation of the Earth around P O may be imagined the resultant of two rotations, into which two, it can (see COMPOSITION OF FORCES) be decomposed. For instance, if B O be taken to represent the true velocity of the Earth around its axis, we imagine that velocity and direction, given t by two rotations—one with the velocity around F O, and the other with the velo- C O around A O. But the rotation around , which is the polar axis of the point A, not affect the apparent motions of a pen- um at A, which is the *Equator* of P O. These arent motions will only be affected, therefore, a motion of the amount C O around A O f. This latter case is the first one investi- ed, viz., when the Pendulum is suspended the *Pole;* so that the plane of oscillation of endulum at A must, in consequence of the tion of the Earth, seem to make a revolution period of time determined by the velocity The time in which that plane will seem evolve at the place A will be to a *sidereal* as CO : BO—*i. e.*,

ne of app. revolution = *Sid. day* × *Sin. Lat.*

Foucault verified his conclusions on a grand e by suspending a pendulum from the interior he cupola of the Pantheon; nor does it re- e more than ordinary precaution, and the use long suspension, to enable any one to bring he same manner, under the notice of any iber of persons, this palpable evidence of the ation of the Earth.—(3.) M. Foucault has e then imagined a still more ingenious device the same end—embodied in his GYROSCOPE .) If a mass be set in rotation freely in e, it will—unless disturbed or constrained— *erve absolutely the plane of its rotation;* and, fect this, it will even overcome slight obstacles. the mechanical contrivances constituting the *oscope*, a heavy ring of metal *a a*, is so freely ended, that it is almost at *entire liberty* to in any direction. That ring is, while de- ted—as in the small figure—set in very ra- rotation, and then placed in its frame—a ie so nicely constructed that the heavy ring continue to rotate for a considerable period, , as we have said, enjoy, all the while, fullest rty to assume the position suited to its mecha- il condition. Suppose now that a graduated on the edge of the apparatus is examined ough the telescope *m;* it is clear that if the h be at rest, the same graduated line will tinue under the spectator's eye at the tele- e. But if the earth is rotating, and carrying the Gyroscope along with it, the revolving ring cannot remain in its original relation to the tele-

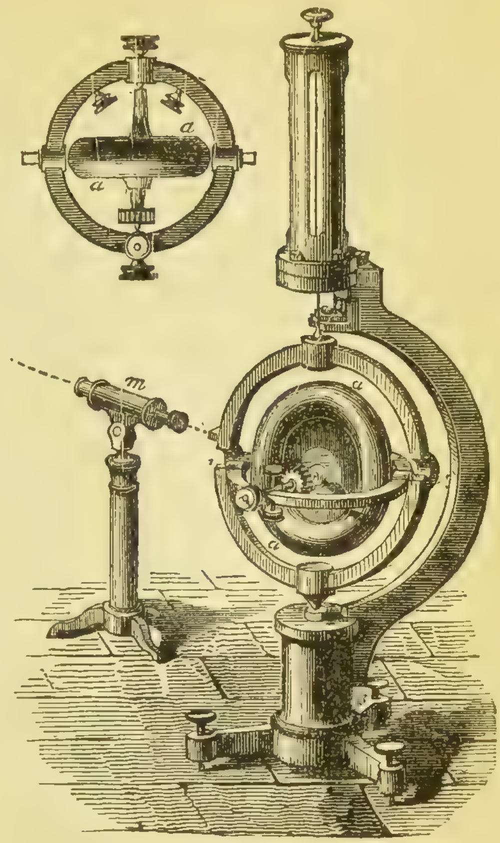


Fig. 3.

scope, just because its displacement by the Earth would, if it did so, *change the plane, in space, in which the ring revolves.* To retain itself in that plane, therefore, the ring will cause the graduated slip to move round under the telescope; and the observer will discern the different lines of graduation, passing regularly under his eye, exactly as a star moves across the field of view of a transit instrument. The mechanism peculiar to the *Gyroscope*, we shall describe under a special article: it is sufficient here to have shown in what manner it fulfils its very interesting object.—Neither the rotation, nor the orbital motion of the Earth, then, can longer be treated as admirable hypotheses—they are established *facts.*

(2.) *The Earth, Magnitude and Figure of.*—The subjects now referred to, have given rise to as large and interesting a series of researches, as those that relate to any other point of our cosmical mechanics. We shall endeavour to notice their chief characteristics.—It is needless to dwell on those familiar considerations that indicate the general shape of the Earth. Multitudes of common phenomena, such as the aspects of bodies seen from a distance—the regular apparent increase and diminution of the altitudes

of stars on the meridian, when the observer travels northward or southward—and, above all, the shape of the Earth's shadow as seen in Eclipses of the Moon,—all concur in proving that our planet's form is globular, approaching, indeed, to a perfect sphere. The *Magnitude* of that sphere is easily determined—at least the methods of determining it may be readily apprehended. Take, for instance, two points on the Equator, so remote from each other, that the one point (because of the Earth's *rotation*) will pass exactly underneath a star, (or have a star *on its meridian*,) exactly one sidereal hour after the same thing has happened to the other. These two points being determined by Astronomical Observation, their actual *distance* in miles may be measured by processes of Geodesy or general Surveying. But as the first of these points moves through the whole Equatorial circuit in twenty-four hours, (the period of the Earth's rotation,) it is plain that the measured distance must be exactly a twenty-fourth part of the entire girth of the globe at the Equator, in other words, a twenty-fourth part of the *circumference* of the terrestrial *sphere*. It is not necessary, however, that the precise space just mentioned should be measured, nor that it lie on the Equator; the accurate measurement of *a degree*, or any part of a degree, of Astronomical Longitude or Latitude, at any portion of the Earth's surface, will answer the same end: nor is there any difficulty in the process, except that which belongs to the exact determination of the distance between the two selected points—a process unquestionably demanding all the resources of practical Geodesy.—The general problems regarding the Shape and Magnitude of the Earth being thus resolved, it remains to inquire whether, with minute accuracy, the *spherical* form can be attributed to our planet. Observation of the figures of our companion planets do not encourage an affirmative conclusion, as their discs are in no case perfectly circular: and, immediately after the discovery of gravitation, Newton discerned that no globe *in rotation* can be expected to be spherical. Suppose, that in such a planet as ours, a canal were cut from the Pole to its centre, and another canal at right angles to it, from centre to Equator; it is clear that, if the planet has assumed a stable form, a fluid filling these two canals ought to be in *equilibrio; i.e.*, the weight of fluid in one of the canals should have no tendency to overbalance or push out the fluid in the other. In a planet at rest, these canals would, under such circumstances, be equal in length; and the planet a perfect sphere: but it is different under the condition of a globe's *rotation*. The centrifugal force, affecting in this case the canal from Equator to Centre, and not affecting the one from Centre to Pole, would virtually diminish the *effective weight* or the *density* of the fluid in the former; so that, to produce equilibrium, the *Equatorial Canal* must be *longer than the Polar*. Newton concluded from these considerations that the Earth cannot be a sphere, but an *ellipsoid*, whose shorter diameter is the polar: and he determined that the actual Polar and Equatorial diameters of the Earth must be to each other as 229 : 230. However valuable, as a first contribution, Newton's labours could not be expected to issue satisfactorily, either as regards the foregoing numerical determination, or the general shape he attributed to the Earth. Having taken no account of the fact that the density of the Earth in all probability increases, as we proceed from surface to centre, the ratio he gave could not be the true ratio: and although he proved that the Equatorial diameter must be longer than the Polar, he did not prove that the contour of the Earth *must be that of an* ELLIPSE. This latter proposition, indeed, has not even yet received a competent demonstration; nor are Geometers entitled to assert that, in its full generality, it is true. But one positive proposition, of which it is a converse, *in excess*, has been thoroughly established. The excellent Scotchman, Colin Maclaurin, laid down a general theorem, which will always be held a landmark. Clairaut, building on this, proved that in the case of a rotating ellipsoid, a proportion between the major and minor axis, could always be determined, so that—whatever the law of augmentation of density towards the centre—all the particles of fluid filling a canal of any form within it, should be at rest. The successors of these eminent men, advanced—by generalizing and simplifying—their analysis; but, among all their remarkable efforts, it were wrong to omit notice of the classical labours of our own James Ivory. As inquiry proceeds, however, indications increase, that we are not entitled to infer the correct ellipticity of the Earth, or any other rotating planet, as a logical *e converso*: nay, recent investigations by Jacobi of Berlin, tend to the conclusion, that other forms of a rotating body, may *also* be forms of equilibrium.—But turning from abstract Dynamical Theory, let us now briefly notice the means employed to ascertain the exact figure of our planet by actual measurement and observation. These are *three*. —(1.) The geodetic measurement of a degree on the meridian, suffices, as we have seen, to determine the Magnitude of the Earth; the measurement of two degrees or portions of degrees, in Latitudes considerably apart, is enough to reveal its exact shape or *eccentricity*. If P E Q E′ be an ellipse, it is easy to see that its curvature is much greater about the ends of the major axis than at the ends of the minor—near E or E′ than near P or Q. In other words, the osculating or equicurve circle near P will have a longer radius than the same circle in the proximity of E. Let *m m′* and *n n′* represent spaces, in those different localities answering to the same fixed number of *degrees*, ascertained astronomically, by the determination of the latitudes of *m*, *m′* and *n*, *n′*

meaning of which is, that the angle N S N'—
ɔeing the centre of curvature of $n\,n'$)—is equal

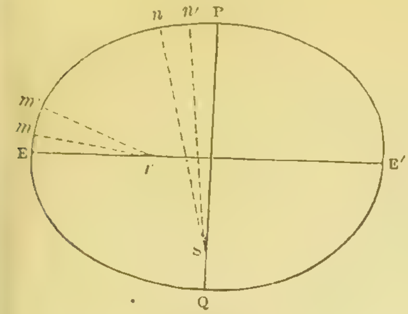


Fig. 4.

he angle $m\,n\,m'$,—n being the corresponding
·re for the arc $m\,m'$. But if these two angles
·qual, it follows that the line or distance $n\,n'$
·reater than the line or distance $m\,m'$, just
ıuse n s is greater than $m\,n$; that is to say,
·ided the earth be an ellipsoid whose minor
: is the polar, the *length* of degrees of Lati-
·—measured geodesically—must increase from
·ator to Pole: and the *ratio of this increase*,
·ending on the ratio of the axes of the ellipse,
if accurately determined, *reveal that ratio.*
measure such arcs in different latitudes has
ı the object of many great Surveys, under-
·n on a scale, whose costliness could be met
· by the resources of great Nations. England,
ıce, and Russia have all contributed munifi-
ly in aid of the gigantic work; and the mean
verage result of all the measures is, that the
·ticity of the Earth is 1-299th, or that the
: are to each other as 298 : 299. Corres-
ling results may be derived from the mea-
ment of degrees of *Longitude*, at different
tudes, but the explanation of this method
t not detain us here.—It falls to be observed,
·ever, that the various measurements effected,
ıot quite harmonize; nor is it possible to
ain the discrepancies otherwise, than by
buting them to unknown irregularities in
inner structure of our globe,—irregularities
·h affect the level or the plummet, and must
·fore deprive all such researches of the attri-
of ultimate accuracy.—(2). The ellipticity of
Earth is also indicated by that invaluable
sical Instrument, the *Pendulum.* We shall
ose that the length of the Pendulum vi-
·ng *seconds*, has been measured at various
·udes, with extreme nicety, and corrected
·he influence of the varying density of the
n which it oscillates—a process of no trifling
·acy and difficulty. Now theory tells us
g, or the force of attraction, under which
·pendulum oscillates, is determined by the
le formula:—

$$g = \frac{\pi^2\,l}{2\,t^2}$$

the force of gravity depends on the shape of the Earth; or on the distance of the place from the Earth's centre. That distance, then, at Pole, or Equator, or intervening positions, will follow from the length of the second's production. The average of a multitude of observations in this case, gives the ellipticity at 1-285ths.—(3.) The remaining method of determination, depends on the relation between the Earth's protuberance, and certain important phenomena in Celestial Mechanics. It is the action of the Moon and Sun on this protuberance, which gives rise to the phenomena of PRECESSION and NUTATION (*q.v.*), and that protuberance reacting on the Moon, produces inequalities in her motions in Longitude and Latitude. It is impossible to evolve the ellipticity from these motions, without an assumption as to the law of the increase of the Earth's density as we approach the centre; but an assumption that cannot be far from the truth, gives 1-305 as the result.—Upon the whole, then, it may be taken as a conclusion worthy of reliance, that the Earth is an elliptical spheroid, rotating around its shorter diameter, and that its ellipticity is about 1-300;—which signifies that the Equatorial diameter is longer than the Polar by about four and a-half times the height of *Mont Blanc.*

(3.) *The Earth, Density of.*—The achievement which most of all astonishes the young student in Astronomy, is probably its accurate determination of the weights of the celestial bodies. It will be explained elsewhere in what manner the weight of every constituent in the solar system can be compared with the weight of the Earth and measured by it: it is our present purpose to explain the methods by which our own globe is weighed, and that weight expressed in *pounds avoirdupois.* We shall notice three methods—all depending on the same principle, viz., a comparison of the attractive force of the Earth with the attractive force of some smaller body whose actual weight we can estimate.—(1.) The first method, known for upwards of a century, consists in observing how much a mountain mass *deflects a plummet from the vertical position.* This deflection, accurately measured, indicates the relative powers of the Mountain and the Earth: so that, if the actual weight of the Mountain could be calculated, that of the Earth might be inferred. Bouguer, long ago, noticed the deflecting or disturbing effect of *Chimborazo;* but it was reserved for Maskelyne to conduct experiments with the express view of carrying out this inquiry. He chose the Scottish mountain Schehallien—a large mass, stretching east and west, and alike steep on the north and south. The deflection of the plummet amounted to from 4″ to 5″: and, assuming a certain density or weight of the mountain, it was easy to infer from such a deflection, that the mean density of the Earth is nearly five times greater than that of water. The chief element of uncertainty is evidently this,—supposing the observations to be

scrupulously accurate, how is it possible to determine unmistakeably the weight of the mass of the mountain? Still Maskelyne's conclusions were entitled to a certain authority; and they undoubtedly constituted the first reliable step in a remarkable inquiry.—(2.) The second method is much more accurate; for although the conduct of it demands an unusual nicety, it does not require us to deal with any indefinite quantity whatsoever. It is the method known as the *Cavendish Experiment*,—performed originally, on the suggestion of Michel, by the well-known Henry Cavendish, and recently repeated by Reich of Freyberg, and Mr. Francis Baily. The general character of the apparatus, as used by Mr. Baily, is represented below.

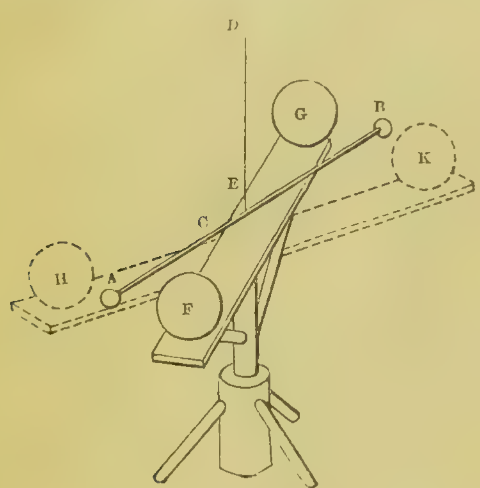


Fig. 5.

Two balls, A and B, of about two inches in diameter, and carried on a rod AB, are suspended by a wire D E; and their position observed under every precaution, by aid of a telescope. This position determined, large balls of lead, twelve inches in diameter, placed on a turning-frame, are brought near them, in so careful a way, that they could produce no effect on the small ones, *except through the force of their attraction.* On the approach, the small balls moved towards the large ones by a very trifling quantity, and this quantity was carefully measured. By aid of the turning-frame, the position of the large balls is then reversed, and the deviation again noticed. From multiplied experiments, (Mr. Baily made upwards of 2,000,) the amount of deviation or deflection was accurately determined. This element ascertained, the calculation is brief. It is no difficult matter to compute what *dead pull* the large balls must have given the small ones to produce such a deviation. But if lead balls, twelve inches in diameter, exert such a force, what would be their force, or their *dead pull*, were they as large as the Earth? A question easily answered. Now, the attractive force of the Earth is known; that being the *weight* of such balls, or their tendency to fall:—the attractive power of the Earth, therefore, may now be compared with the attractive force of lead; and since the density of the latter is known, that of the former must thereby be determined. It turns out, from Mr. Baily's results, that our globe is 5·67 times heavier than an equal volume of water;—a conclusion almost accurately divined by the memorable sagacity of Newton—"Verisimile est quod copia materiæ totius in terra, *quasi quintuplo vel sextuplo* sit quam si tota ex aqua constaret." (*Principia*, iii. 10.)—(3.) A third mode of determining the Earth's mean density has recently been put in practice most successfully, as far as the experimental portion of it is concerned, by Mr. Airy, *Astronomer Royal.* It consists in comparing two invariable pendulums, the one on the Earth's surface, and the other at a considerable depth below that surface. The difference of their rates will give the difference of gravity for that depth; and from this the mean density may be computed. The experiments were made at the close of 1854, at the surface, on the banks of the *Tyne*, and at the bottom of a pit of Harton Colliery, one of the deepest coal mines in this country. This low station was no less than 1,260 feet under ground. The ready means of communication now afforded by *Electro-Telegraphy*, put it in Mr. Airy's power to render his comparisons perfect; and, as a matter of course, he overlooked no requisite correction. The pendulums differed in rate $2\frac{1}{4}$ seconds per day; from which it follows that the gravity for that depth was increased by the 1-19190th part. The density of the Earth deduced from this result, is between *six and seven times* that of water; but Mr. Airy thinks that, as yet, he has not taken full account of the *hollow* of the *Tyne*, of the basin named *Jarrow-slake*, of the scoop indicated by the sea, and of the real and observed specific gravity of the rocks covering the mines of Harton. The necessity of bestowing values on considerations like these, shows sufficiently the *weak* part of this mode of determination:—on the whole, we had rather trust to the Cavendish experiment.

(4.) *The Earth, Temperature of.*—The question as to the actual heat of the surface of the Earth—as the momentary equilibrium of various heating forces—falls in the main within the sphere of Meteorology, and will be discussed under one section of article TEMPERATURE. The forces referred to, are the Calorific Action of Space, the Calorific action of the Sun, and the proper Heat of our globe itself: the last only, rightly belonging to the Physics of that globe. That the Earth has a proper or special temperature,—or that large portions of it have so—is evinced by many phenomena, of which the following is the chief: As we descend beneath the surface, *diurnal* variations of temperature grow less and less palpable, and at length they cease. The temperature of this stratum varies only with the *seasons.*

·cending still farther, the variations of the ons, in their turn, show less and less ampli- ·, and also practically cease to be indicated: temperature of this stratum is the average perature of the year, and may be called the t of solar effect. Now, below this line or tum, phenomena occur, in all respects highly ·arkable. The temperatures of inferior strata, ead of diminishing, as they recede from the ·re of the influence of the Sun, augment ac- .ing to a very perceptible rate:—the fact of ch augmentation is undoubted, being proved e by the phenomena of hot springs, and the ease of the temperature of the strata in deep ·es. The only point at all in question has re- ·nce to the *universality of this increase.* It is ainly still open to doubt, whether the ob- .ations have been extensive enough to entitle o assert that the fact may not affect large, perhaps peculiar districts alone. The ·age rate of the observed increase, is that of f Fahrenheit for a descent of between 40 and ·eet; or of about 100° per mile. At the ·h of *fifty* miles, supposing this rate to con- e uniform, a temperature of 5000° would .ail; and as no solid element that we know ould resist fusion at such a temperature, the ·ence—granting the accuracy of the pre- ·s—would be, that our globe is only a thin . crust, environing a liquid molten mass. : conception of a *central heat*, has been the urite one. Fourier takes it as the basis ·is remarkable theory; and Humboldt and ·r geologists of widest view, have discerned in ·central Heat, and in the action and reaction ·een the liquid centre and a thin solid crust, cause of volcanoes, earthquakes, and our d secular elevations. With every respect to : illustrious names, we must interpose, that ·eference of those superficial convulsions and ·avings—momentous to us, but trifling in rd of the whole Earth—to such a cause, is ·inly, the explanation of a small effect by a endous premise: but, what is much more ·rtant, Mr. Hopkins appears to have defi- y shown, that the Astronomical and tho- hly ascertained phenomena of *Precession* (see article,) are dynamically irreconcileable with such thinness of crust.—Another solution has offered recently by Poisson. He imagines through some cause, the various regions of e may have different temperatures; and that, ie sun sweeps on, in virtue of his movement *ranslation*, the planetary system will be ged, now into a cold region, and again into .rm one. If we have left a warm region, entered a cold one, the body of the Earth : now be cooling, and the increase of tempe- ·e to a great depth below, becomes explicable, t from the hypothesis of a central Heat.— ere needless to go into other hypotheses. habitudes of the Earth, and the entire sub- of the sources of Heat, are as yet too little known, to entitle us to take up dogmatically, any existing positive theory whatever.

Earthquake. The change which, within these few recent years, has passed over our knowledge of this most stupendous and alarming of terrestrial phenomena, happily constrains notice of it, in a dictionary of physical sciences. The ability of Mr. Mallet, of Dublin, alike in his investigation of physical causes, the critical discussion of historic records, and his experimental verification of bold and unlooked-for deductions, appear to have dispelled, in regard to earthquakes, that mystery and misconception with which every subject affecting our wonder and apprehension continues surrounded, until inquiry has revealed its laws. For two reasons, we shall make no attempt to narrate the recorded sights, sounds, and motions that are said to accompany earthquakes:—*first*, because the general impression regarding them is nearly as correct, as such records, in the state in which they are presented to us; and we have no space to follow Mr. Mallet's searching and convincing criticism: *secondly*, because the actual facts will be much better understood from a simple statement of their explanation.—Earthquakes are the consequence of a shock or impulse of some sort inflicted on the solid portion of the earth, at some point below its surface. This shock is sometimes accompanied by permanent elevation or subsidence of portions of the surface: with this effect or concomitant, however, we have nothing at present to do: the earthquake, properly so called, results from the shock alone. Now, this shock may be given right under some continent, or within part of the solid earth lying beneath the ocean. This latter is the more complex case, and also the more usual one: we shall trace, therefore, only *its* consequences. Such a shock, it is evident, will affect *three* of the constituents of the globe,—*the solid mass* above the seat of the shock—the *ocean* lying over the seat of the shock—and the *superincumbent atmosphere.* Let us attend to these three effects separately.—(1.) A shock, whatever it be, communicated to any point of an elastic solid mass, is propagated by two kinds of *vibration* or waves. In the first place, there is an *elastic wave of compression*, that moves onward on all sides in circles, proceeding from the point of shock as a centre, and carrying to a distance a sense of the trembling, or of the original shock. Such waves of trembling or undulation go in all directions, vertically as well as horizontally—but much farther horizontally than vertically. The state of the neighbourhood when a heavy railway train passes through a *tunnel* illustrates both. On the top of the tunnel there is a vertical movement of the soil, however slight; and at a far greater distance, horizontally, a trembling or agitation is felt. Slightest causes propagate such oscillations:—we have heard that the sudden shutting of the outer gate of Green-

wich Observatory has sufficed to make a star appear to start out of the field of view of the transit-telescope. It is not possible, then, that the sudden upheaving of a part of the solid earth, and perhaps its immediate and violent fracture, should not propagate waves of compression all around it: and on the solid *surface* these will have the exact appearance of low and broad waves at sea, only rushing on with an extreme velocity. The velocity of their propagation depends, of course, on the elasticity of the earth's surface; but as every part of that rocky crust is highly elastic, when compared with water, the earthquake wave through the solid portion of the earth, will greatly outstrip the primary sea wave in swiftness. The rate of its transit is probably not less than 2,000 feet per second on an average; and it is to this swiftness of propagation, and not to *internal fissures*, &c., that we must attribute such phenomena as the agitation of our Highland lochs by the earthquake of Lisbon. This solid wave, presenting all the forms of a low wave—with such a velocity of propagation—is the cause of the more fearful of the consequences of earthquakes: and Mr. Mallet has abundantly shown, not only that prevalent conceptions regarding *vortical* motions are as unnecessary as untenable, but that every established fact of concussion or demolition may be referred to the action of such a *wave*. And he has shown further, that inasmuch, as through the varying elasticities of different rocks composing the crust of the earth, this wave cannot be propagated circularly any more than the ocean waves—the deviations from uniformity of propagation will amply explain all established phenomena apparently at variance with this simple physical theory. — A second wave, however, is transmitted through this solid crust, viz., the wave of *sound*. On occasion of a fracture of the strata, from whatever cause, such a wave will instantly be propagated, and with an immense velocity, possibly from 8,000 to 10,000 feet per second. Hence the rumbling noise often preceding the shock of earthquakes.—(2.) The next wave, in physical importance, is the great sea wave. This must be distinguished from the wave or sea agitation, which the solid wave in the bed of the ocean must, as it were, *carry on its back;* and which must always have caused that phenomenon of the recession of the sea, noticed so frequently as coincident with the shock of earthquakes. The primary ocean wave—a large but low one—propagated on all sides, and modified as it proceeds by the depths of the channels over which it flows, must alternately dash on all coasts in the neighbourhood of the primary shock, and produce effects of its own. But as the progress of the solid wave and that of the liquid one is not determined by the same condition,—that of the former, depending on the elasticity of the solid strata; that of the latter on the mere depth of its channels,—it cannot but be expected that discrepancies, and even apparent disconnection should arise. A peculiar wave of sound must also be propagated through the ocean lying above the locality of the shock. —(3.) The *third wave*, is produced by the agitation of the *atmosphere*. If the original agitation has been sufficiently abrupt to cause an adequately swift vibration or oscillation in the superincumbent atmosphere, a great wave of sound will be propagated in the usual way. This must be low in note, and must reach the ear considerably after the shock has been experienced.—Such are the causes of the phenomena. Mr. Mallet has not stopped with a mere general appreciation. And important experiments, on a large scale, directed by him, have been recommended by the *British Association*.—See its valuable *Reports*.

Ebullition. The phenomena of ebullition and spontaneous evaporation differ in this material point, that in ebullition the vapour is formed within the mass of the liquid, while in evaporation, it is formed at the exposed surface. In what circumstances can vapour be so formed? Only when it obtains sufficient tension to resist the pressure round it. Vapour will not be formed within the liquid, where the pressure is 15 lbs., per square inch, provided no force is introduced capable of giving that vapour an opposite tension at least equivalent. The force ordinarily employed in producing ebullition is heat. Galvanic currents, and other agencies, also produce it; and they likewise do so by giving sufficient tension to the vapour to enable it to resist the surrounding pressure; that obtained, it is immaterial what way it may come. The boiling point of a liquid depends, indeed, upon the pressure to which the liquid is subjected; and the condition of boiling is very simple—namely, that the heat or other agency be such as to induce a higher tension in the vapour than the pressure can keep down. The consequence of this ought to be that, if we ascend a mountain, on the slopes of which the pressure of the atmosphere is less than at the surface, liquids ought to boil at lower temperatures than at the surface. They actually do so. The following table will illustrate this:—

	Yards high.	Inches of pressure.	Boiling point
Farm of Antisana,.........	4488	17·87	187·34
Quito,......................	3176	20·74	194·18
Mexico,.....................	2490	22·52	198·14
Convent of St. Gothard,....	2302	23·02	199·22
Briançon,...................	1428	25·39	203·9
Baths of Mount Dore,.......	1136	26·26	205·7
Madrid,......................	665	27·72	208·04
Phombieres,..................	459	28·39	209·12
Moscow,......................	328	28·82	210·2
Lyons,........................	177	29·33	210·92
Vienna,.......................	144	29·41	211·1
Paris,........................	71	29·69	211·46
Sea Level,...................	0	29·92	212

The same law holds with regard to all liquids. But these boil, under the same pressure, at *differ*

temperatures. We give below the tempera- at boiling of several well-known liquids:—

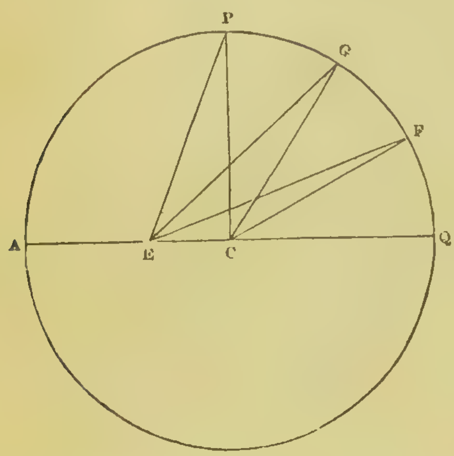
Sulphuric ether	100°·04
Sulphuret of carbon	116·6
Alcohol	175·64
Water	212
Essence of turpentine	314·6
Phosphorus	554
Sulphur	570·2
Sulphuric acid	590
Linseed oil	600·8
Mercury	662

ppears, then, that different liquids boil at dif- nt temperatures, and that any given liquid er lighter pressures will boil at lower temper- es. The opposite of the latter result will good; *i.e.* bodies boil at higher temperatures er heavier than ordinary pressures. Hence, Papin's DIGESTER. We can easily conceive ow much importance this is, when one wishes pply a very regular heat to a body—a deside- m impossible of attainment by ordinary com- on. The proper process is to generate steam e temperature required—regulating the pres- under which the water is placed, according e temperature—making a partial vacuum of intended to be low, and forcing in high pres- air, or enclosing it simply in a strong vessel Papin's Digester, with a valve to open at the er time, if the temperature is to be high. boiling point also depends somewhat on the e of the enclosing vessel. Thus in glass, boils more slowly than in metal pots. The mess of the surface helps it—just as there is ction on amalgamated zinc, in a battery rough zinc is powerfully decomposed. ably there are little galvanic currents excited assist the formation of the steam. If we in a few scraps of metal into a glass pot, eam forms about as quickly as it does in a one, and it first forms on these scraps; or, make a scratch with a diamond on the of such a vessel, we see the same formation am first round it. The boiling point is only, ver, very slightly altered by the insertion iquid of masses of solid matter, and not at the sides of the vessel be rough and the r be not dissolved. Thus, if we throw sand pot of heated water, it will boil neither nor later for that. If, however, we insert matter which is soluble, we obtain what lity is a new liquid, and the boiling point hange. It is worthy of note that, if there ery small aperture permitted to the steam ig from water to escape by, the temperature steam varies with the proportion borne by erture to the whole vessel. The quantity our emerging in a given time is, generally, nuch the same; but there seems to be a d heating effect in the mere rushing of the through the aperture. The issuing steam to give back part of its heat to the remain- ass; and the more, the smaller the aperture.

Temperature to which the water rises.	Proportion of surfaces of orifice of vessel.
212°.	1-1,000 and upwards.
221°.	1-5,000 „
239°.	1-10,000 „
280°·4	1-20,000 „

A very curious phenomenon, first noticed by Leidenfrost, has been recently investigated with great care by Boutigny. It is this: if a drop of water, or any volatile liquid, be put upon a very hot metal vessel, there will not be, as might have been expected, any immediate vaporization. The globule will, instead of that, float on the metal, as a needle does on a vessel of mercury, without wetting it. The heat, when so violent, produces a repulsive force, which keeps it from touching the metal at all. If the metal gradually cool, there will be a wetting of it, and then an immediate evaporation. The character of the liquid and of the metal go, together, to determine the temperature at which the vaporization takes place. The play of repulsive forces, M. Boutigny informs us, induces the globule always to take an ellipsoidal or spheroidal shape. He mentions 340° as about the lowest temperature at which the result described is produced for water. A drop takes a spheroidal shape—a larger quantity becomes flattened in shape—always retaining the form of an ellipsoid with its axis vertical. A somewhat remarkable result will be obtained, by darting a jet of sulphuric acid upon the watery globule. It adheres to the heated body readier, and is suddenly volatilized by the violent heat. The heat of volatilization comes also however, from the water as well as from the heated body; and there is so much thus abstracted as to *freeze* the water at the bottom of the red-hot vessel in which it is contained.—A very interesting application of the theory of ebullition may be made in determining by means of it the height of a mountain. Taking Regnault's values of the tension of vapour of water as correct, water which would boil at 90° centigrade, or 194° Fahr., would indicate a tension of 525·45 mill. (20·66 inch) of mercury—the standard being 760, and the weight of the air is diminished exactly in this proportion. If, therefore, at the top of a hill you get water to boil at 194° cent., you have a pressure $= \frac{525{\cdot}45}{760} \times$ atmospheric pressure at surface. If now, observations be made, or calculations, which may show us how much the weight of the atmosphere diminishes as we rise, and what is its weight at each point above us, we can, comparing this weight with it, obtain the height of our observed point. The simple rule—perhaps as correct as any that can be given for moderate heights, is that the boiling point is lowered 1 degree for every 550 feet of elevation. Thus water boils at 184° on the summit of Mont Blanc, giving it an elevation, by this rule, of 15,400 feet, while its actual height is 15,730.

Eccentric. (Ptolemaic Hypotheses.) When the ancient astronomers found that certain of the heavenly bodies, as the Sun, do not move in an exact circle, which, as they fancied, is the perfect figure, or at a uniform rate, they adopted a new hypothesis to save their old one. This was the problem. The sun (*e.g.*) appears to do something, which seems not moving uniformly, or else not in a perfect circle; but as he must move at once uniformly and in such a circle — required the explanation of this false appearance? They supposed that the standpoint from which we view his motion, viz., the earth, is not in the centre of the circle which the sun describes, but nearer the circumference (as



E within P Q A). Now, if the sun do move in a circle, and at a uniform rate, his movement in a given time will be along P G. In the next time of the same length he will describe G F, equal to P G. These lines, P G, G F, will subtend equal angles at the centre C, because equal arcs; and to an observer at C, therefore, who measures their motion by the angular space passed over, they will appear to indicate uniform circular motion. But an observer at E, sees the angles G E P and F E G to be described in equal times, and these two angles manifestly vary in value. Hence the body moving along P G F will appear to describe unequal spaces in equal times, to the spectator at E. This is exactly what the sun does, to the inhabitants of the earth, (in fact, his motion can be very closely approximated to, in this way, by an appropriate choice of the position of E—see ANOMALY); and it follows, therefore—since the postulates already advanced are indisputable, according to the old astronomers—that the earth is in the position E. The circle is called an *eccentric.*

Eccentric (Steam Engine) is a circular plate fixed in the rotating axle of a steam engine, so that it may pass at some not very large distance from its centre. By its means the valve gear and pumps of the engine are wrought very easily. It is an engineering contrivance of Murdoch's, and is almost universally adopted in engines.

Eccentricity. In such circular motion as the one above described (where there is a uniform motion in a circle, in the centre of which the spectator is not,) the excentricity, or eccentricity, is the ratio which the distance E C bears to the radius. In an ellipse, it is this same ratio—namely, that of the distance between either focus and the point of bisection of the line of foci (the centre of the curve), and the major semi-axis of the curve. In the older works on astronomy the term is also applied to the value of this line itself. (*Technical.*) The mathematical expression for the eccentricity of the ellipse is $\sqrt{\frac{a^2-b^2}{a^2}}$, and of the hyperbola $\sqrt{\frac{a^2+b^2}{a^2}}$.

Echo. We have seen (ACOUSTICS) that, in all ordinary cases of sound, there is vibration of the air or other elastic media; and that to this, the impression of sound is due. We have supposed, there, that the sound has been propagated from its source to the ear directly, and that its source was a body in which, by direct action, vibration had been produced. Suppose, however, that such a body sends out a pulse of vibrating air, which strikes upon some one that will no permit its passage. Here we have a mere repetition of such an impact as may produce vibration. The body which has interposed has such motions excited in its surface; and these again send back the air in similar pulses. This phenomenon is called that of the reflexion of sound, and to it is due what is called an echo. When light is reflected, we have seen (CATOPTRICS) that the returning ray moves in such a direction that the angles of incidence and of reflexion are equal. The same law holds in sound. In that case, however, we were permitted to consider light as composed of a number of independent rays, which could be reflected also independently, and which, in their course, do not communicate their own vibration necessarily to the luminiferous ether round that section of it through which they pass. But here we must consider that every vibrating point sends out vibrations in all directions round it; and that, when a surface is set to vibrate, the various parts of it originate vibrations which may, to a considerable extent, interfere the one with the other. There is not very much importance to be attached to this effect, however, in the simpler cases, and, in consequence, the same laws may be conceived to hold for sound that have been proved to hold for light. (CATOPTRICS.) Thus, if a sound be made at one focus of an ellipsoid, a series of sonorous vibrations will be excited, all of which will be sent back from the ellipsoidal surface, according to the law of reflexion, to the other focus. Hence, if a person stand there, all of them will strike on his ear, and a sound much more violent than could be heard by a person listening to the direct sound at any point of the ellipsoid will be the result. The person standing in the focus will

-rst hear the direct sound, and then the reflected ›ound, in one volume. The velocity of the sound ·ave is the same after as before; so that, as the ım of the lines drawn from the foci to any one ›int in an ellipse is constant, all the waves ·flected from each elliptic section of the surface ill converge at one moment—traversing equal ›aces in equal times. This species of vault, there- re, will give the most powerful echo. A circular ›me will illustrate this also, if we place ourselves : the centre. A circle is just an ellipse where the ci have come to meet in one point. Hence, we ill hear the sound which we create, and its flexion from all the circumferences also. The ro sounds in an ordinary dome will appear as if e same. The sonorous wave travels 1,090 feet one second, so that the abstract difference of ne in a dome of 54½ feet in diameter would be ›out 1-20th of a second only. An echo in this ay can be always produced from an elliptic sur- ce, without taking into account the distance of e foci. Any surface may, for a small space, considered as part of an ellipsoid—one which ›uld just touch it there—and so from any sur- ›e we might have an echo. This echo, how- er, would not be at all the same as that where e whole elliptic surface—catching the whole ›lume of emitted sound, reflects it on to the other ›us. There is only a little portion which sends ›ck the sound accurately to that point, and the ›o is, therefore, much more faint. Moreover, the iptic surface so formed to correspond with the tual surface—*i.e.* touching it at the specified int—may not have its focus at the origin of ınd, and most frequently has not. The echo ich we have just considered, is that of sound ›urately reflected to the ear. Diverging first m its point of origin, it meets a surface ich makes it converge back to a new point, ere the ear is situated. But it is not all faces that can make sound so converge. Re- ring back to the case of plane mirrors (CA- PTRICS), we shall find that there is produced, in › reflexions of light, a series of rays which do t at all appear to converge, but appear as if ›y had come diverging from a point different m that where they actually originated. So it often with sound. Reflected waves go off m the impending body in such a direction that ›y appear as if they had come from a different int. Though this point is behind the mirror, eye situated behind the mirror would receive impression of light, because the rays in fact not pass through; but an eye before it will. nilarly an ear situate before a plane reflecting face, will catch reflected sound, and be im- ›ssed with it as if it came from a point be- ıd the surface. The ear may blend the two unds together—if the distance from the surface not great; or if great, there will be a dis- ct echo, as if from a place farther away. When see a body and its reflexion in a mirror at once, is impossible, from the velocity of light, for us in any case to distinguish between the time of the impressions. Hence the ear, not trained nearly so well as the eye to discover distances from impressions upon it, will confuse the two sounds, if they do not come at intervals quite distinctly perceptible. If, however, our distance from the sounding body and the point where a body seems to have been sounded, in consequence of the reflexion, be any considerable portion of 1,090 feet, there will be a perceptible interval elapsing between the two sensations, and we will distinguish a decided enough echo. If, as is very usual in echoes, or rather, as is most usual in those which have been observed, we hear the returning sound at the same place where the sound is made —when we speak, for example, and are answered by an echo—there must exist a reflecting surface perpendicular to a line drawn to it from our position; only that wave of sound which goes from us, perpendicular to the reflecting body, can be sent back to us accurately. We found, in the article on CATOPTRICS, that this property of causing divergence of luminous rays was not confined to plane mirrors, but exists also in convex ones of all sorts. The same property holds with regard to convex reflections of sound. There, however, as in the mirror, the focus from which the reflected wave (of light or sound) seems to come is, generally, nearer the centre of emission of the sound than is the case of the plane mirror. There is an echo, therefore; but the tendency to confuse the echo with the original sound becomes more decided; so much so, indeed, that it may become almost as if the point of emission were at the reflecting surface itself. Hence, these echoes are not so readily noticed as in the case of plane reflection. Very often, as we have seen, concave reflectors make the sound actually curve back before their surfaces, so as to pass through a point at which the auditor may be placed. In plane reflectors, it seems always to pass from a point behind. We may imagine, therefore, reflectors of different concavities which may permit the rays to appear divergent, as if they came from a point behind. We may, therefore, have also with concave reflectors what might be called the *virtual* echo, in which the reflected sound appears to come from a point through which no sound actually passes, just as the reflected image appeared to do so. We have spoken of the distance between the point of emission—the point of reflexion—and that of hearing, as being those upon which depends the confusion of echo with, or its distinct separation from, the original sound. Where the image of the sound is actually made at the point at which we hear it, the sum of the distances of the points of emission and of hearing from that of reflexion, respectively, must be a moderate fraction of 1,090 feet, for any sound to give a distinct echo. Where, again, the image is *virtual*, the difference of the two distances of the points of emission, actual and virtual, from the ear must be such a fraction. If the dis-

tance of the auditor from the actual point of emission were greater than from the virtual point, we should hear the echo sooner than the original sound, but a recurrence to the figure for plane mirrors (CATOPTRICS) will readily show that this is impossible. In fact, the distance from the point of virtual emission to the ear is equal to the sum of the distances of the point of reflexion from that of actual emission, and from the ear. These two last lines, along with the distance of the ear from the point of actual emission, form a triangle, and their sum is, therefore (I. 20), greater than that line. Hence the echo can never come before the sound to be echoed.—It is usual to describe an echo by the number of syllables it repeats. This depends on these distances, in the same way. We can repeat so many syllables in a certain time. Hence, if an echo takes up our repetition of 15 syllables, after we have just finished it, there is a sort of measure furnished of the distances referred to. Some of them are quite measurable—and we can, therefore, sometimes calculate the position, in a simple echo, of the reflecting body. Sometimes, however, we have a more complicated case to deal with. The reflected wave may be again reflected, and that again and again and again before reaching us. If the position of these reflecting bodies is quite unknown, it will not be easy to find it out. When the centre of emission of the sound moves during the time that it is sounding—if it move with a greater velocity than that of sound, the position relative to us may manifestly be so much altered that we shall hear the latest emitted sound first, by reflection, and the first latest. Thus a flash of lightning moving towards us produces a sound which is echoed by the clouds; and, as its velocity is so much greater than that of sound, we shall hear the reflexion of the last emitted sound, first—and, as we distinguish distances to a slight extent by the ear, calculating from the intervals of sight and sound, we shall seem to hear the thunder roll backward when the lightning has been moving forward. The clouds are capable of reflecting sound as well as more solid bodies, although the reflexion is not at all so distinct. Some bodies of much more substantial texture absorb all vibrations more completely. Thus carpetings and cloths are nearly effectual cures, if applied properly, for the most violent echoes. The sail of a ship at sea, when distended by the wind, is like a stretched string, very sensitive to impressed force, and evidences this very readily by sonorous vibrations. The particles of the atmosphere play a very important part in the reflexion of sounds from their own mass—not merely transmitting them passively; just as the same particles reflect light and give rise to the phenomena of dawn and twilight. They do not reflect at all so powerfully, however, as clouds do. Thus a cannon fired on a clear day and on a cloudy day, will give a perceptibly different report with the same charge. A circumstance frequently noticed in the case of echoes, is that, after ceasing, they appear to begin again. Sometimes this arises from the fact, that the reflected sound has passed through one portion and struck on another surface perpendicular to its direction, or on a series of surfaces from which it has been again reflected back. As in no case is there not some loss of sound in producing these internal vibrations; and as, generally speaking, smaller and smaller fragments of the original sound wave are reflected every time, the second echo will be fainter than the first, and, if reflected again, fainter still. Sometimes it arises from another cause—namely, that there are, in fact, two reflecting surfaces, at different places, from which the original sound is reflected. The one, perhaps, 500 feet distant, gives an echo in about half a second; while another, perhaps, 2,000 feet distant, gives another echo in about two seconds. The effects of echoes on buildings in rendering them unfit for particular purposes, will be described in the article HEARING.

Eclipses. It happens occasionally that the sun's disc loses its usual circular form. It becomes indented on one side, the indentation increasing gradually in extent; then gradually diminishing, until finally it disappear. Sometimes this indentation augments until the whol[e] disc is covered, and the sun remains for som[e] minutes out of sight. At the end of that tim[e] he reappears, passing successively in the in[-] verse order through the same phases that ha[d] preceded his disappearance.—The moon, als[o] from time to time, undergoes similar modifica[-] tions in the form of her disc, which must no[t] although there is a certain resemblance, b[e] confounded with her phases. They last nev[er] longer than some fraction of a day; they a[re] much more irregular in their reappearance and these are separated by much larger inte[r-] vals. — These remarkable phenomena, whic[h] were objects of terror for a long period, ex[-] citing, however, only our curiosity now, a[re] called ECLIPSES. Those of the sun happen alwa[ys] at the time of the new moon, and those of t[he] moon always at full moon. This indicates t[he] explanation very readily. At new moon, t[he] moon, which is passing *between* the sun and t[he] earth, may hide a larger or smaller portion [of] the sun from us; and there is an eclipse of t[he] sun. At full moon, the earth is between t[he] sun and the moon, and may therefore keep t[he] sun's rays from falling on her surface, so that [she] becomes a dark body—and there is an eclipse [of] the moon.—If the moon kept, in its moti[on] round the earth, constantly in the plane of t[he] ecliptic, there would manifestly be an eclipse [of] the sun at every new moon, and one of the mo[on] at every full moon. They do not happen [so] often, because the moon moves in a plane [in-] clined to the ecliptic; so that being sometimes [on] one, and sometimes on the other side of it, [and] at various distances from it, it passes at

oment of syzygy far enough from the ecliptic prevent there being an eclipse. There can only an eclipse at those times of new or full moon, hen the moon is in the point of her orbit here it cuts the ecliptic, or near that. From is circumstance the name *Ecliptic* itself comes. 'e shall enter into some explanations of the cumstances which accompany eclipses of the ın and Moon, and the methods employed r predicting their return. We shall begin ith—

(1.) *Eclipses of the Moon.*—Let us try, if we n discover whether it is possible for the earth, ming between the sun and moon, to prevent e sun's rays from falling on her. The sun nits rays of light in all directions. Those rected towards the earth are stopped by it, d beyond it, therefore, there is a portion of ace in shadow. Let us conceive a cone, A O A′,

Fig. 1.

mpletely enveloping the sun S, and the earth —touching their surfaces all round. It is easy see that no solar ray, supposing it constantly keep its rectilinear direction, will penetrate to that part of the cone between its apex and e earth; whilst it will be seen that there may ways be rays, if not from the whole, at all ents, from a part of the same body, reaching y other point in space. The part B O B′, then, nstitutes the shadow of the earth, turned away om the sun.—That there may be an eclipse of e moon, that body must enter the cone of the adow. If we draw through T the line T C, rallel to O A, we shall have two similar tri-gles O B T, T C S, which will give the propor-n $\frac{OT}{TB} = \frac{TS}{SC}$.—Taking T B, the radius of the rth, as unity, S C, (= S A—T B) will be 111; d the distance (mean) T S, of the sun from the rth is about 24,000. Hence O T will be 216, at is, the distance between the centre of the rth and the apex of the cone will be 216 mi-diameters of the earth. The moon's aver-e distance is only 60 such semi-diameters. follows that the moon can enter the sun's adow, and not only so, but, where it enters, e diameter of the circular section of the cone ll be very much larger than that of its disc, that it may be completely within the cone. r, any section nearer T than half the distance T (108), will be greater than half the size of e earth's disc—that corresponding to the lunar stance from O, giving a space of $\frac{156}{216}$ of this c, which is much larger than that of the oon.—When the moon only enters the cone in rt, the eclipse is said to be *partial;* when it es so completely, it is *total.*—If we conceive the moon to progress pretty uniformly, and in a direction almost perpendicular to the axis of the cone of shadow, we may get an idea of the leading circumstances of an eclipse. Where it is partial, the shadow gradually increases over the moon's surface till the moon's centre is in that part of her orbit nearest the axis of the cone, and then gradually diminishes and disappears. Fig. 2 will give an idea of the indentation apparent on the moon's surface when the earth's shadow passes in this way over any part of it. The indented rim *a b c*, is a portion of the outline of a transverse section of the cone where the moon is; and the rounded form of the rim, which is always distinctly noticeable in an eclipse, clearly demonstrates the round shape of the earth, (see EARTH, Figure of,) which gives character to the cone.—In a total eclipse, the cutting of portions of the luminous disc proceeds till all is so taken away. After some time the moon reappears on the other side of the umbra, and goes through the same phases, identically as before, but in inverse order.—The indentation, when the body is but partially eclipsed, is far from being so distinctly marked as the figure would lead us to suppose. The shadow has a *penumbra*, as always happens when we speak of a shadow cast by an object exposed to the sun's rays.—Let us imagine—that we may have an idea of the size of this penumbra—another cone A O A′, having its apex O,

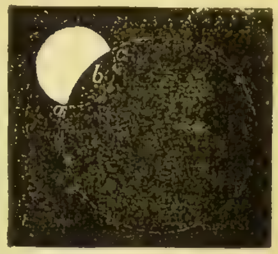


Fig. 2.

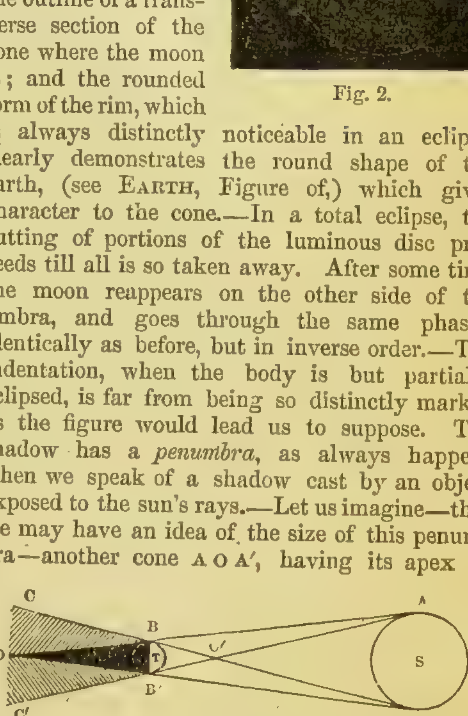


Fig. 3.

between the sun and earth, enveloping the sun S, and the earth T, in its two opposite halves A O′ A′, B O′ B′, which touch the surfaces of these bodies all round.—It is clear that every point within the space C B B′ C′, and without the shadow B O B′, must receive rays of light from a portion of the sun's disc only—from such a point, only a portion of the sun could be seen, the rest being concealed by the earth's interposition. It will be clear, too, that the portion of sun seen, or that sends rays to the point, will be greater, the nearer the point is to the outer edge of this space. So that, whilst the moon is moving so as to enter the cone of the earth's shadow, a portion of its surface begins to lose its brightness as it enters C B B′ C′ — the light diminishing as it goes farther from the outer surface of this space, till it disappears in the

umbra itself; the different portions of the disc occupying, at different moments, positions within this space corresponding to the penumbra, there must be an insensible diminution of light, from the points illuminated by the whole surface of the sun to those to which no ray reaches. But it is easily seen, that the diameter of the moon's disc is not so large that we should clearly distinguish the penumbra in its whole extent. The angular breadth of the penumbra is exactly the angle C B O, and that is equal to A B A′, which is just the apparent diameter of the sun, seen from the earth; and as the apparent diameter of the moon is almost the same as that of the sun, it follows that she may fall almost the whole breadth of the penumbra.—The passage from the pure umbra to the penumbra is quite insensible; the softening down of the shading is so gradual, that it is impossible to tell the exact moment when any remarkable point on the moon's surface leaves the penumbra to pass into the umbra, or the reverse.—There are other circumstances, due to the presence of the earth's atmosphere, which we shall now consider.—We have conceived hitherto the rays of the sun passing near the earth to preserve the rectilinear direction with which they are emitted from the sun. But we know that rays traversing the atmosphere do not maintain their original direction; they change it whenever they pass from one layer to another of different density, when, after entering the atmosphere on one side they leave it on the other, they must have experienced some similar deflexion. Imagine a special ray S A,

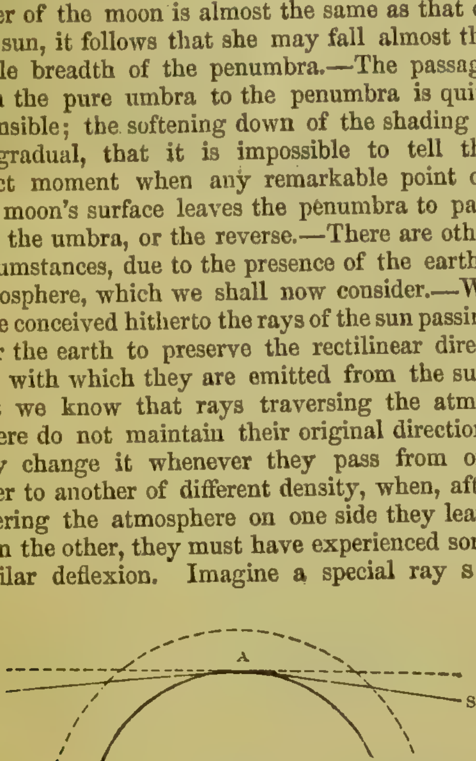


Fig. 4.

passing through the atmosphere near the surface of the sun. The direction of this ray at the point A, where it has become, so to speak, a tangent to the surface, is not the same as it had before passing into the atmosphere; the deviation that it has experienced up to the point A, is more than 33°, in average circumstances. From A, till it leaves the atmosphere, it experiences another deviation equal to that, and in the same direction; so that the line which it ultimately follows makes an angle of more than one degree with the primitive direction. This total deviation of a light ray, in passing through the atmosphere, is the smaller, the farther the ray is from the surface at its lowest point, and may take any magnitude inferior to that just given. We see, then, that the cone of the umbra will not be deprived of sun rays, through its whole extent. They are deflected, so as to approach nearer to the axis. If we consider those rays which, like A B, A′ B′, pass through the lowest strata of the atmosphere, and continue their

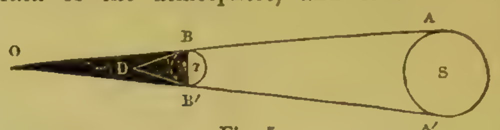


Fig. 5.

course, we shall see that they converge at a point D, much nearer the earth than O is. The cone B D B′, which is formed by these rays, divides B O B′ into two parts—in the inner of which there is absolutely no light—while the outer, B O B′ D, receives the light refracted through the atmosphere of the earth. If we find the distance of D from the earth's centre, we obtain for it 42 radii of the earth. We see, then, that the moon (at 60 radii distance) can never enter the space B D B′, which is completely dark—the moon, at the time of total eclipse, is within the partially illuminated space B O B′ D. That is the reason why she never loses her light entirely, even in such an eclipse.—It will be noticed that this faint light, which the moon retains in total eclipses, is of a very distinct reddish colour. Some bright points, which Herschel had remarked on certain parts of the surface of the body, during eclipses, had induced him even to fancy that there were volcanoes existing in a state of activity; but the whole effect is due to the influence on the transmitted light of the beds of air through which it has passed. Air stops a certain portion of light passing through it, and reflects it in every direction, occasioning a diffused light; but this action is not the same on the different rays of which white light is made up. Those at the violet end of the spectrum are stopped in much greater numbers than those at the red end; which occasions the blue of the sky, the rays of the first kind predominating in diffused light: it also produces the ruddy colour of the illumined clouds at sunset, because the light which reaches them having traversed a great thickness of the atmosphere, contains a larger proportion of the rays of the second kind than white light does. We see, then, that the light which reaches the moon's surface, during her total eclipses, must have a reddish tint, since it does not reach her till after passing through a very great thickness of atmosphere. This red light, when strongly reflected by some peaks of the mountains of the moon, probably occasions the bright points which Herschel took for volcanoes in activity.

(2.) *Prediction of Lunar Eclipses.*—As these eclipses are due solely to the positions of the sun and moon in regard to one another, the knowledge of the laws of the motions of these two bodies must enable us to predict, not merely the periods at which these phenomena must take place, but the various circumstances characterizing them. We shall show how this aim may be attained. The ancients did not know

the laws of the solar and lunar motions with anything like accuracy; but, by assistance of the period of 18 years and 11 days, which we have mentioned (see CYCLES), they managed to foretell lunar eclipses with tolerable correctness. We know that, if the moon remained in the plane of the ecliptic, there would be an eclipse at every full moon. The reason that lunar eclipses are much less frequent, is simply this, that the moon, if on one side of the ecliptic or the other, when in opposition to the sun, may pass above or beneath the cone of the earth's shadow, without entering it at all: there is an eclipse only when, at the time of opposition, the moon is near enough the ecliptic, or, what is the same thing, near enough one of the nodes of her orbit. If, at two different periods, the moon, being then in opposition to the sun, be also in the same position as regards her nodes, there cannot be an eclipse at one such period unless there be one also at the other. Now, if starting from any observed eclipse, we count on, for 223 *lunations*, we find ourselves almost at a full moon, where the body occupies the same place with respect to her nodes as at the beginning of that time: since, during it, there have been 19 synodical revolutions of the nodes, we may then, after these 223 lunations, expect a recurrence of eclipses similar to that which went before. We see, in this way, how it was sufficient to have noticed the dates and principal phases of the lunar eclipses for the space of 223 successive lunations, to enable us to predict the return of those eclipses for an indefinite space of time.—If 223 lunations *exactly* closed the 19 synodical revolutions of the nodes, there would be no necessity for the use of any other methods for the calculation of lunar eclipses. But the equality of the two periods which is assumed,—is merely *approximate*. So that, if we may certainly foretell, by the help of this period, that an eclipse will happen at such and such an epoch, we cannot very distinctly indicate its time of duration or its character; as it differs in reality a little from the one before, with which, were the period exact, it would have been identical. It might even happen that a very partial eclipse should not reproduce itself at all at the end of 18 years and 11 days; or that a partial eclipse might come, in 18 years and 11 days, when nothing of the sort had been observed before. The use of this period, therefore—the single means employed by the ancients for the calculation of eclipses—is not satisfactory, now that astronomical theories admit of an exactitude so incomparably greater. The laws of the movements of the stars, as science has exhibited them up to this date, have been exhibited by astronomers in tables, by means of which it is possible to indicate beforehand the position which a star must occupy in the sky at any given time. It is on the data, supplied by the solar and lunar tables, that we found, in making predictions of the lunar eclipses. But these data are to be obtained elsewhere than in such tables themselves. The *Nautical Almanac* is published by order of the Admiralty, several years in advance; and contains all the indications relative to the positions of the sun and moon in the sky, for every day; and it is by means of these that we obtain whatever is needful for the determination of the different circumstances of eclipses.—To understand the process of calculation for an eclipse of the moon, we must conceive that the radius of the celestial sphere has been selected so that its surface passes through the centre of the moon; this sphere, the centre of which must be supposed placed at the centre of the earth, will cut the moon, with a circular section, and the cone of the umbra of the earth with another; and, by studying the relative positions of these two circles, we come to understand the whole circumstances of lunar eclipses. The centre of the circle of the umbra is always diametrically opposite to the centre of the sun; and so, always on the ecliptic, changing its position, with the same velocity as the centre of the sun himself does, but in the opposite direction. The circular section of the moon, again, also moves, so that its centre remains always on the moveable orbit already mentioned. As long as these two circles remain entirely beyond one another, there is no eclipse; when *they cut* there is an eclipse; total, if the circle of the moon be entirely within that of the umbra.—In order to compare the successive relative positions of these circles, we must know their magnitudes.—We already know that the moon's diameter (average) is 31′ 25″·7, its value for any day in the year, which varies always between 29′ 22″ and 33′ 28″, is given by the *Almanac*, at mid-day and midnight, and we may, by means of these, correct it for any given hour. As for the circle of the umbra, it is easy to see how we may calculate its magnitude. Let M N be the surface

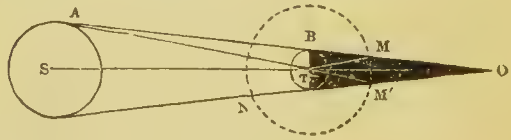


Fig. 6.

of the celestial sphere which we suppose passing through the moon's centre; this surface cuts the cone of the earth's umbra, in the circle M M′, and the angle M T M′ is the apparent diameter, which we desire to determine. Its half, M T O, is equal to B M T, which is the moon's parallax (since M T is the moon's distance from the earth), less by the angle M O T; but the angle M O T, is itself equal to A T S (semi-diameter of the sun), less by B A T (the sun's parallax); in order, then, to have the apparent semi-diameter of the circle of the umbra M M′, we must add the sun's parallax and the moon's parallax together, and subtract the sun's

apparent semi-diameter. This circle of the diameter is found thus to vary in diameter between 1° 15′ 32″ and 1° 31′ 36″; its value, at any time, may be found by means of the *Nautical Almanac*, which furnishes these supplementary data. We add, however, that in consequence of the presence of the atmosphere, the earth's umbra appears to have a diameter a little larger than that thus obtained. Mayer has found that, in order to make the predictions of eclipses agree with our observations, we must suppose the diameter of the umbra increased by a sixtieth of its value, and astronomers usually conform to this rule.

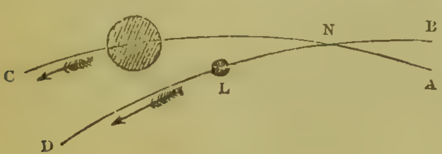


Fig. 7.

Let AB be the great circle of the ecliptic, and CD be the orbit of the moon, N will be one of the nodes of this orbit. The umbra O, moves along the first circle with the same speed as the sun moves, and the moon L, along the second with a velocity 13 times as great. In order that—in this common movement—the moon L may meet the umbra O, it is necessary that, at the moment of the moon's opposition, the centre of the umbra be sufficiently near the node N. Remembering that the apparent diameter of the moon and the umbra vary from time to time, and that the distance of the centre of the umbra from the node N is exactly equal to the distance of the sun's centre from the moon's other node, we find that, 1st, if, at the epoch of a full moon, the distance of the sun's centre from the nearest node be greater than 12° 3′, there cannot be an eclipse; 2d, if, at such a time, the distance of the sun's centre from one of the nodes be smaller than 9° 31′, there will certainly be an eclipse; 3d, lastly, if the distance of the sun from one of the nodes be between these two values, the eclipse is doubtful, and a detailed calculation of the circumstances will be required to determine whether it takes place or not.—Let us see now, how the calculation of the different circumstances of an eclipse, and of the precise periods at which its phases will appear, is actually made. The best thing we can do, in that way, is to give an example of this sort of calculation.—Take the eclipse of 13th and 14th November, 1845, at Paris. According to the *French Nautical Almanac* (the *Connaissance des Temps*), on the 13th November, at noon (Paris time), the sun's longitude exceeds the moon's by 186° 20′ 7″·1; on the 14th, also, at mean noon, the sun's longitude is greater than the moon's only by 174° 45′ 8″·6. In the interval there must have been a time at which the difference of the two was exactly 180°; and it is readily found that this moment, at which the moon is in opposition, is on the 14th November, at 1h. 4m. 20s·9 in the morning. The *Almanac* tells us that at that time the sun's longitude exceeds that of one of the moon's nodes by about 5½ degrees. We are certain, then, that the moon will penetrate into the umbra of the earth, that is, that there will be an eclipse. We find also in the *Almanac* that, at the moment of opposition,

The Moon's parallax is about....	55°	39″·6
The Sun's parallax is about		8″·7
The apparent semi-diameter of the Moon's about	16°	10″·1
The apparent semi-diameter of the Sun	16°	12″·8

We infer from this that the semi-diameter of the umbra is about 39° 36′, or 2376″, so that, increasing it by one-sixtieth of its value, for the reason above indicated, it becomes equal to 2415″·6.—We find again, by the *Almanac*, that, 1st, on the 14th November, at 0h. 30m. in the morning, the excess of the sun's longitude over the moon is about 180° 16′ 33″·7, and the moon's latitude is about 0° 25′ 57″·6 A; 2d, the same day, at 1h. 30m. in the morning, the excess of the sun's longitude over the moon's is about 179° 47′ 37″·7, and the moon's latitude about 0° 28′ 51″·5 A.—By help of all these data we may study the whole circumstances of the eclipse in the following way. Consider the portion of the celestial sphere on which the moon and the earth's shadow are found, during the whole duration of the eclipse, as a plane—a hypothesis which may be made without appreciable error. Suppose, besides, that the earth's shadow is immovable, and that the moon does not move, except in virtue of the movement which it has relatively to this shadow. We may represent the shadow of the earth by the circle ABCD (in the next figure), by choosing the radius OA of this

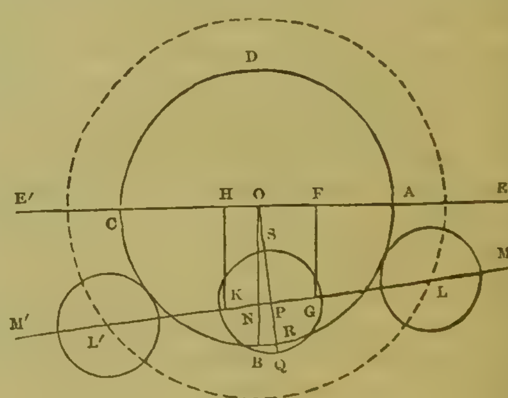


Fig. 8.

circle, so that it may correspond to the value of the semi-diameter of the shadow (2415″·6) after the scale which we have adopted in the figure. The straight line EE′, passing through the centre O, of this circle will represent a portion of the ecliptic. At 0h. 30m. in the morning, the sun's longitude exceeds that of the moon by 180° 16′ 33″·7; the longitude of the centre O, of the

shadow, exceeds that of the moon's by 16′ 33″·7, or 993″·71. If we suppose longitudes to be reckoned from right to left in the figure, and take O F equal to 993″·7, according to the scale—the point F will be the foot of the centre of the moon's latitude for the moment. Raise at F, a perpendicular on the ecliptic E E′, then take on this perpendicular a length F G equal to 25′ 57″·6, or 1557″·6, which is the corresponding latitude of the moon, and we have at G the position of the moon's centre at 0h. 30m. in the morning.—Take similarly O H equal to 12′ 22″·3, or 742″·3, which is the excess of the moon's longitude over that of O, the centre of the shadow, at 1h. 30m. in the morning; then, on the perpendicular to the ecliptic drawn through the point H, take a length H K equal to the corresponding latitude of the moon, whose value is about 28′ 51″·5, or 1721″·5, the point K will be the position of the moon's centre at 1h. 30m. in the morning. We may, without sensible error, consider the movement of the moon, in relation to the shadow, as rectilinear and uniform, during the whole duration of the eclipse. So that, if we draw a straight line M M′ through the points G, K, this line will be the path over which the centre of the moon moves, in relation to the circle of the umbra A B C D. The point N where the line M M′ is met by the perpendicular to the ecliptic drawn through the point O, is nothing else but the position which the moon takes at the moment of opposition, that is, on the 14th Nov., at 1h. 4m. 20s·9 in the morning.—Describe a circumference from O as centre, and with a radius equal to the sum of the radii of the moon and of the umbra, that is equal to 3325″·7; this circumference will cut M M′, the relative orbit of the moon's centre, in two points L, L′. It is very evident, according to the way in which these two points have been obtained, that if a circle be described from either of them as centre with the radius of the moon, which is 910″·1, as radius, these two circles will touch the centre of the circle of the shadow A B C D, and will represent, consequently, the two positions of the moon relative to the beginning or ending of the eclipse. If, further we drop from O, a perpendicular on M M′, the foot, P of this perpendicular will be the position of the moon's centre at the middle of the eclipse. The moon takes an hour in passing from G to K. According to the proportion between the two lines N P and G K, the length of which may be measured on the figure, we find that the moon should take 5m. 40s·8 to pass over the distance N P; it is then 5m. 40s·8 before the opposition, that is at 0h. 58m. 40s·1 in the morning, when the middle of the eclipse takes place. Similarly we find that the moon should take 1h. 39m. 19s·4 to pass over either of the two distances L P or P L′; it will, therefore, be at 11h. 19m. 20s·7 on the evening of the 13th November, that the eclipse will begin; and on the 14th, at 2h. 37m. 59s·5 in the morning, that it will conclude.—Describing a circle from the point P, as centre, with the moon's radius, as radius, we immediately see whether the eclipse is total or partial. Here we see it to be partial, because at the moment when the moon's centre is nearest the centre of the shadow, a part of its disc remains outside of the circle of the shadow. If we draw the diameter, Q S, towards the point O, and take the proportion between the portion R S of this diameter, which is in the umbra, and the diameter itself, this proportion is what is called *the magnitude of the eclipse.* In the particular case above, the magnitude is 0·92. It is usually expressed in *digits.* We conceive the diameter Q S to be divided into 12 equal parts or digits, and state how many of these R S contains. The fraction 0·92, being almost equal to $\frac{2}{2}\frac{2}{4}$, we say that the magnitude of the eclipse of the 13th and 14th November, 1845, is 11 digits. If the diameter Q S, were entirely within the circle of the shadow, in which case the eclipse would be total, we should determine the beginning and end of the total eclipse by seeking the positions of the moon, at which its disc, is a tangent interiorly, to the circle of the shadow. This will be as easy as the search after L L′, where the disc of the moon and the umbral circle touch externally.—Throughout the foregoing, we have been supposing that it was graphically, by measuring certain lengths, on the figure, that the determination of the different circumstances of the eclipse is effected. It will be readily understood that corresponding processes of calculation may take the place of those rather inexact methods, and that thus a much higher accuracy may be attained. This is what is actually done.—To complete our sketch of all that relates to the particular eclipse selected, it only remains to indicate at what places of the earth this eclipse would be visible. Let us seek, in the first place, for all the places where the eclipse may be seen, at the moment of its maximum intensity. We have found that the middle of the eclipse happens on the 14th November, at 0h. 58m. 40s. of mean Paris time. Taking the equation of time, which, at this date *(Almanac)*, is 15m. 27s., we see that it is at 1h. 14m. 7s. of true time that this occurs. Consider the point of the earth for which the moon is at that moment at the zenith, it will be readily recognized that it is midnight there, and that, consequently, its longitude west of the meridian of Paris is 18° 31′ 45″. As for the latitude at this point, it is equal to the declination of the moon's centre, at the same instant—a declination which, according to the *Almanac*, is 17° 42′ 17″ B. Hence, we have merely to conceive the earth to be divided into two hemispheres, by a plane drawn perpendicular to the radius which ends at the point whose longitude is 18° 31′ 45″ O, and whose latitude is 17° 42′ 17″ B; the middle of the eclipse will be visible for all the points of the earth situate in one of these hemispheres, and invisible for all of them which are situate in the other.

What we have just done as to the middle of the eclipse, might be repeated for its beginning and its end; and we should thus find all the places whence it would be seen, either wholly or partially. It is easy to infer that the places from which an eclipse may be seen for the whole or a part of its duration, extend over more than half of the globe.— In order that we may see a lunar eclipse, the moon must be above the horizon, as well as the earth's shadow, or at least a part of it, and this can only take place when the sun is below the horizon; it is only during the night, therefore, that eclipses of the moon can be seen. There are certain circumstances, however, in which we may see an eclipse of the moon for a few seconds before sunset or after sunrise. If, for instance, we stand at such a point as A at the moment

Fig. 9.

when an eclipse begins, the whole sun will be below the horizon, and the part of the moon which is found in the cone of the shadow will be equally so; but atmospherical refraction, elevating the bodies above the horizon, will permit us to see the sun and the eclipsed part of the moon at once.

(3.) *Eclipses of the Sun.*—We have said that eclipses of the sun are due to the interposition of the moon between the sun and the earth. It is evident that, when this circumstance occurs, the moon must cut away from sight a greater or less portion of the sun. Let us first attempt to see whether the moon can at any time completely cover the sun?—Following a course completely similar to what we have pursued in the case of lunar eclipses, we may find the length of the cone of the shadow which the moon projects from the side opposed to the sun. Compare this length O L, then, calculated when the moon, L, is exactly between the sun S, and the earth T, with

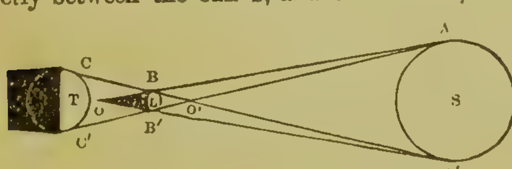


Fig. 10.

the distance L T, between the centre of the earth and that of the moon. The radius of the earth being taken as unity, the smallest value of the distance L T is equal to 55·947; besides, the greatest value of the distance O L, of the summit of the cone of the umbra from the centre of the moon is 59·73; therefore, in the circumstances to which those values of L T and O L correspond, the umbra of the moon extends to the earth and beyond it. For every point that is completely included within the portion A B of the earth's surface, the moon entirely covers the sun; there is, then, a *total eclipse.* But, again,

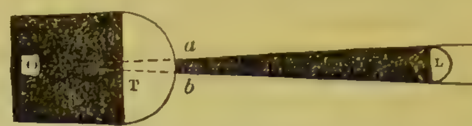


Fig. 11.

the smallest value of the length O L, of the cone of the moon's umbra is found to be 57·76; and the greatest distance of the moon's centre from that of the earth is 63·820; when we have these circumstances, the cone of the moon's umbra does not extend to the earth. In this

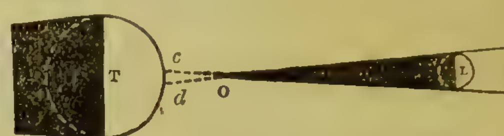


Fig. 12.

case, there is no total eclipse for any point of the earth's surface; from all points of the hemisphere from which the sun can be seen, a portion, if not the whole of his disc will be seen. One thing is worthy of note, viz., if the cone of the moon's umbra be prolonged out beyond its apex, O (last fig.), the second cone will intercept within it a small portion, c *d*, of the earth's surface, over all of which there will be an *annular eclipse;* from every point of it the moon will appear projected as a black disc on the middle of the sun's disc, and the portion of it which stands out, will form a luminous ring all round this black circle. Thus, when the moon places itself between the earth and the sun, there will be a total or annular eclipse, for certain points on the earth's surface, according as their distances from the earth vary.—Other considerations would lead us to the same conclusions. If, at the moment when the moon is passing before the sun, her apparent diameter is greater than his, she will be able to cover him altogether, and there will be a total eclipse: but, it is easy to see that this may take place, since the greatest value of the moon's apparent diameter from the surface of the earth is 34′·6″, and the smallest value of the sun's apparent diameter only 31′·31″. If, again, the sun's apparent diameter be the greater, the moon cannot cover his whole disc, so that there will be an annular eclipse; which again may occur, seeing that the least apparent diameter of the moon is 29′·22″, and the greatest of the sun is 32′·35″·6. In the latter case, at the point where the centres of the two bodies seem just to coin-

Fig 13.

cide, the sun's disc will appear as in the figure. At the same time when the eclipse is total or annular at some points of the earth, it will be partial at a great many other points. Conceive, round the sun and the moon, a cone like that which we have used to find the penumbra in lunar eclipses. It will be easily seen that for every point of the earth within this cone C O′ C′ (fig. 10), not included within the cone of the umbra B O B′ or its prolongation, there should be a partial eclipse of the sun; from such a point, we should see the moon project itself on a portion of the sun's disc, producing a circular indentation (fig. 14), and the part covered by the moon should be greater the farther the point of observation is from the surface of the cone C O′ C′ and the nearer to B O B′ (fig. 10). The transversal dimensions of C O′ C′, in the neighbourhood of the earth are not so great that the globe of the earth can be entirely contained within it. To understand this, simply notice, that because of the littleness of the moon in proportion to the sun, which makes the distances O L, O′ L appear very small in proportion to the distance L S, and sensibly equal, one to the other, the angles B O B′, B O′ B′, are almost of the same size. Notice besides, that the mean length O L of the cone of the moon's shadow being almost equal to L T, the moon's distance from the earth, the angle B O B′ is not very much different from the apparent diameter of the moon, so that we may consider B O′ B′ as equal to it. Now, since O′ T is sensibly double of O′ L, the transversal dimensions of the cone C O′ C′, near the earth T, should be double what they are near the moon L; the earth's diameter would have then to be only double of that of the moon, in order that the globe should be contained within the cone C O′ C′, touching it round its whole outline Now we know that it is nearly four times as large as it, so that this cone can include only a portion of the visible hemisphere of the earth. Hence, when there is an eclipse of the sun at some places, there are others where there is none, but where the whole disc can be seen with no appearance of eclipse.—The moon moves through the sky, about 30 times as rapidly as the sun. Hence, she alternately approaches and recedes from him, and at certain times passes before his disc so as to produce eclipses. Reflecting on this, we readily find the various phenomena which should distinguish an eclipse, to an observer on the earth. They are perfectly analogous to those in the case of lunar eclipses. The eclipse begins when the moon's disc just touches that of the sun's. Then the moon slowly encroaches. If her centre does not pass so near the sun's centre, that the distance of the two points becomes less than the difference of the apparent radii, the eclipse will be partial. When that distance is the least possible, the eclipse is deepest. Then the moon moving on, leaves more and more of the sun's surface uncovered; at length the two discs are just separating and the eclipse ends. If the distance of the two centres diminishes below the limit indicated, the eclipse will be total or annular; total, if the moon's apparent diameter, seen from the place where it is observed, be greater than the sun's; annular, if the opposite. In either case the moon goes on to cover a constantly increasing portion of the sun's surface. The total or annular eclipse begins at the moment when the distance of the centres of the two discs becomes equal to the difference of their apparent radii, which makes the circumferences of the discs touch internally. After some time, as the distance of the centres becomes again equal to and greater than the difference of radii, the eclipse becomes partial only. Calculation shows that 4h. 29m. 44s. is the longest possible duration of an eclipse at a place on the equator 3h. 26m. 32s., under the parallel at Paris. We understand, besides, that these phenomena may be shorter than that, by any amount. If, in place of examining the different phases of a solar eclipse, to an observer at one special place, we try to understand the phenomena which the eclipse will present everywhere, we shall discover them quite as readily. We must conceive the moon, in her motion round the earth, to carry with her the umbral and penumbral cones B O B′, C O′ C′ (fig. 10), which we have previously mentioned. When, in consequence of this movement, the penumbral cone touches the earth (fig. 15), the eclipse begins at the point of contact. Almost immediately, the penumbral cone proceeds—covering more and more of the globe. Soon, the umbral cone touches the surface, and at that point it is that we begin to observe, either a total or annular eclipse, since it is only this cone or its prolongation which can reach the earth. These two cones, proceeding together, cover successively different portions of the earth, passing from region to region. At the end of any time, the cone of the umbra—then that of the penumbra, again touch the surface, and, the instants when they do so close the total or annular and the partial eclipses respectively. Sometimes, in our atmosphere there are small isolated clouds which project a shadow over the sun, in the middle of places, all the rest of which is illuminated. These being generally in motion, we can see

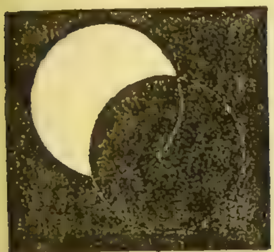


Fig. 14.

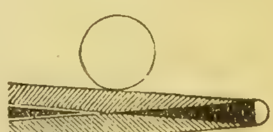


Fig. 15.

the shadow move, often very rapidly over the earth. It is just in the same way, that the umbra of the moon, in total solar eclipses, changes its position on the earth, moving from one edge to the other of the illuminated hemisphere. Astronomers usually determine the general circumstances of a solar eclipse over the whole of the world, beforehand, and, to show these results at once, they make a chart on which to mark its course. The next figure (fig. 16) shows the arrangement of such charts;—it represents the annular eclipse on the 1st April, 1764. The line A B C passes through all points where the eclipse has begun at the moment of sunrise, and

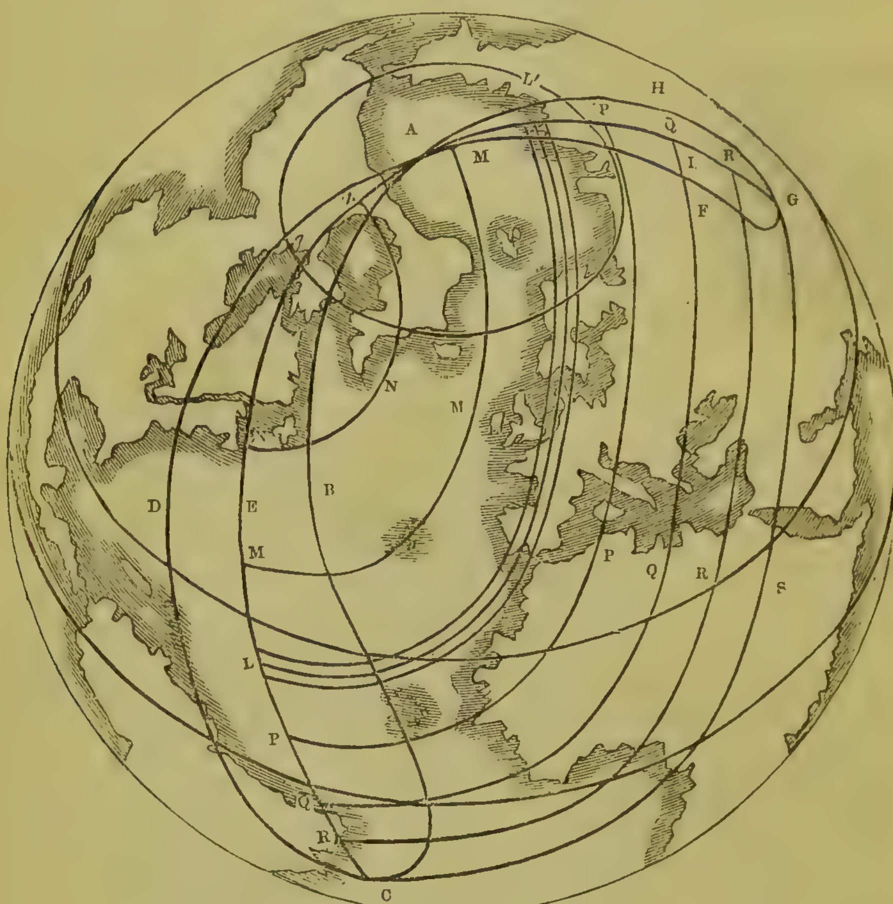


Fig. 16.

the line A D C, where it ends at that time. For all points, on A E C, the sun rose at the middle of the eclipse. Similarly, the lines A F G, A H G, A I G, contain those points respectively where sunset happened at the end, the beginning, or the middle of the eclipse. The narrow strip L L′, is the route which the prolongation of the moon's umbra pursued. We see that it passed to the north of the Cape Verd Islands, over the Canaries, and to the south of Madeira; that it scarce touched on the coast of Morocco, and then passed through Portugal, Spain, France, the Nether lands, Denmark, Sweden, Lapland, and Nova Zembla. The eclipse has been annular at Lisbon, Madrid, and Paris. On either side of the strip L L′, there has only been a partial eclipse, feebler and feebler, the farther the points of observation are from L L′. Over all points of the line M M, the eclipse was only 9 digits—over the line N N, 7 digits. Along P P again, 9; along Q Q, 6; along R R, 3; and along C S G, the edges of the discs merely touch, but there is no eclipse. Beyond C S G there has been no eclipse, although the sun has been above the horizon. The period of 18 years and 11 days, at the end of which the moon passes through the same positions in regard to the sun and to her nodes, is as useful in solar as in lunar eclipses. All that have been observed in one such period, are reproduced in the succeeding ones. There are some few changes, however, because the 223 lunations are not exactly equal to 19 synodical revolutions of the nodes. Observation shows, that, on an average, in the space of 18 years 11 days, there are 70 eclipses, 29 lunar and 41 solar. There are never more than 7, or fewer than 2, in one year; if only two, they will be both solar. It is easy to see why solar eclipses are the more frequent. In fact, if we consider the cone A O A′ enveloping the sun and earth, we know that the

moon must enter this cone at C, that there may be a lunar eclipse; but we see that it must penetrate this cone at D that there may be a solar

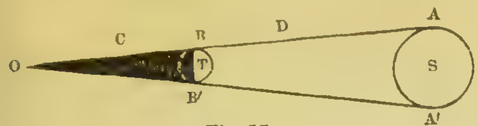


Fig. 17.

eclipse for some places of the earth; now, the transversal dimensions of the cone being greater at D than at C, it results, of necessity, that the moon should reach its surface oftener at D than at C, and that therefore solar eclipses should be the more frequent. But we must not believe that in any one special place more solar than lunar eclipses can be seen. A lunar eclipse is visible over more than a hemisphere, a solar one, only in part of a hemisphere, sometimes a very small part. This is the reason why there are more lunar than solar eclipses visible at one place, in spite of the greater frequency of the occurrence of the latter over the whole earth. We may understand this also, by noticing that the apparent diameter of the earth's umbra, taken at the distance of the moon, is much greater than that of the sun, and that, consequently, it must oftener happen that the moon, observed from any special place on the surface, should touch the earth's shadow, than the sun's disc. As for total solar eclipses, they are extremely infrequent in any one place, as we may readily see by considering how small is the shadow which the moon projects on the earth. The part of the earth covered successively by this shadow is but a very slight fraction of the whole space from which a solar eclipse may be observed. At Paris, for instance, there was only one total eclipse of the sun in the 18th century, that of 1724; and there will not be another till towards the end of the 19th. At London, for 575 years not one was witnessed, from 1140 to 1715; and, from 1715, no such eclipse has been seen again there. Total eclipses of the sun are very rare, very remarkable, and therefore very startling phenomena. The sudden disappearance of the great luminary, the source of our light and heat, naturally fills those who do not know why it must be, with alarm and dismay. When we know that, and that in a few moments the sun will reappear, our terror is lessened, but yet, involuntarily, when the darkness begins, we are seized with a vague fear. Whilst the sun is entirely hid, there appears all round him a certain darkness which seems very intense, because it comes almost instantaneously; but which is yet something very different from the darkness of night. The cone of the moon's shadow, though it does extend to a certain distance all round the place where we stand, cannot enclose the whole atmosphere above the horizon; it leaves round it a considerable mass of air, which receives the rays of the sun directly, and reflects them back again, into the regions of the earth where the total eclipse is being observed; it follows that, instead of a complete darkness, there is a sort of twilight. The brightest stars and the chief planets become visible. The temperature of the air sinks rapidly some degrees. Animals show their terror, and many of them do as they are wont at the approach of night. As long as the total eclipse lasts, we see round the sun and moon a luminous crown, as in the figure. The moon is projected, as a black circle in the midst of this crown. It has been asked whether this

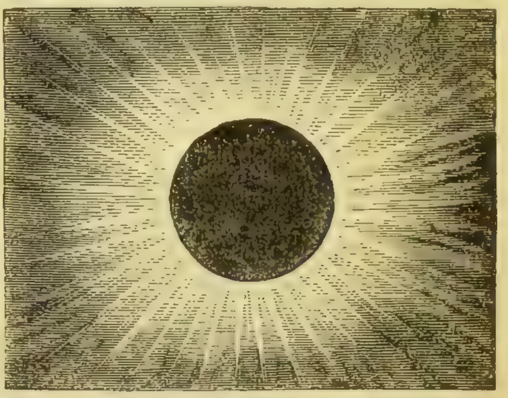


Fig. 18.

aureole be due to an atmosphere of the sun, which his brilliancy would not let us see, or whether it be not due to a very rare lunar atmosphere? To resolve the question, we have sought to ascertain whether the luminous crown follows the moon as it moves over the sun, during the whole time of eclipse, or if it remains behind, preserving its position relative to the sun. Hitherto, observation has given no certain answer. During the total eclipse of 8th July, 1842, visible in the South of France, when astronomers were about to note this, an unforeseen phenomenon arrested their attention. Protuberances of a violet, rose colour appeared round the moon, as seen in the figure. What is the cause of these, for they have been observed in various places by various observers? We cannot tell. Several ideas have been started, but we cannot accept any of them positively. If, for instance, they were due to solar mountains, as is most improbable, the height of these mountains would be prodigious, as the figure will enable us to judge. Partial eclipses of the sun, like the phases which go before and follow the total or annular eclipses, do not produce effects by any means so marked. When the eclipse is a large one, the light is very sensibly diminished, though as long as any portion of the disc is uncovered, it remains very considerable. It is impossible, in observing partial solar eclipses, to look at the sun directly; we must do so through coloured glass or glass smoked by being held over a flame. If we turn to his disc, during such an eclipse,

a plate or card in which a hole is bored, and set a screen behind it, or set it before a wall, we obtain, on the principle of the camera, a representation of the form of the sun, with its indentation by the moon, on the screen or wall. This is a very simple way of observing the eclipse in its progress. The foliage of trees often allows some rays of the sun to pass which may illu-

Fig. 19

minate some parts of the ground in the middle of the shadow of the foliage. The interstices then do just what the card or plate with the hole in it does; it follows, that those parts of the ground, which are illuminated through them, take a form depending on that of the sun's disc. Usually, as the sun's disc is circular, and the

Fig. 20.

rays fall obliquely on the ground, the form of these illumined parts is elliptical. During solar eclipses, the more or less distinct indentation of the disc, is exhibited in these bright spaces, and they take the form of ellipses, indented all on the same side, and by the same amount. This is very distinct, and it is very difficult to help it, being struck, even if it has been anticipated.

(4.) *Prediction of Solar Eclipses.*—The period of 18 years 11 days, which served the old astronomers for lunar, serves also for solar eclipses. But, though it can indicate whether or not there will probably be an eclipse, it does not at all indicate whether that eclipse will or will not be visible, and, if so, to what extent. This difference flows from the different nature of the two phenomena. A lunar eclipse is due to the actual loss by the moon of her light, and is visible wherever she is, and everywhere in the same degree. In the solar eclipse, the sun loses none of his; the moon prevents observers from seeing part or the whole of his disc, and that portion differs with their position. As the period mentioned is not rigorously accurate, and as the rotation of the earth brings different points successively within the moon's shadow, we may see how the eclipses which happen at one place in one such period may not occur at the same place, in the next. Thus, the ancients were obliged, with their imperfect methods, to confine their predictions to lunar eclipses. They continued unable to discover any law among the complex phenomena of the recurrences of solar eclipses. But we can now predict the solar as well as the lunar. In this case, however, the calculations are much more complex. If we seek to predict the character of the eclipse over the whole hemisphere, the complication thus introduced is enormous; even for one place it is very considerable. In fact the parallaxes of the sun and moon are of the highest importance here, since, if we were at the earth's centre, we should generally see the two stars occupying different portions in the sky; and since, from this point, the eclipse might appear a very different one from what it does at our actual position. Now, the parallax in altitude, of the moon—which enables us to pass from the one of these positions to the other—varies considerably at the different hours of the day, and so throughout the duration of any one eclipse. The parallax in altitude, at the beginning of an eclipse, is not, in short, the same as at the end of it. This occasions much more numerous calculations, although not in any way more difficult, than in the other case. We shall give no example, because of its tediousness. It is enough to say, that it is by comparison of the relative positions of sun and moon in the sky, at successive very short intervals (*e.g.* 10 minutes), that we determine the various dates of the beginning, middle, and end of the eclipse, &c. It is to be noted, that though we have to consider the parallaxes, we have not to take account of refraction. In fact, the effect of it will be nearly the same upon both the sun and moon at the time of the eclipse. Hence, we may calculate the times when the sun and moon would appear together, were there no atmosphere, seeing that refraction only lifts them both up for some little distance in the sky—not changing their relative positions.

Ecliptic. The circle in the visible heavens described by the sun in his annual course. Every day he seems to describe a complete circular figure in the sky, returning next morning to the place from which he set out. But, if his exact position be noted, it will be found that he does not so return, but that his motion is in a line something in the form of the spiral in a spring of several coils, bringing him *nearly* back to the same place. In fact, he has moved some distance. If he remained quite stationary relative to the axis of the earth for a whole day, the diurnal revolution would give him a complete apparent circular path. It does not so, because his position has changed during the 24 hours of our rotation. If the points at which he is successively in the sky be noted, a complete circular figure called the ecliptic, representing the sun's apparent or the earth's real path of annual revolution, will be drawn. This is called the ecliptic. Celestial *longitude* and *latitude* (*q.v.*) are unfortunately measured from the ecliptic and its pole, as terrestrial longitude and latitude from the equator and its pole. This want of uniformity tends to confusion. The better known circle, the equator, is usually taken as a sort of circle of reference, and the position of others is given by means of it. The ecliptic is defined in this way, as a circle cutting the circle of the equator, on an average at an angle of about 23° 27′ 54″. This value is, however, not permanent. The attempt to measure it was first made by Eratosthenes, about 270 B.C. (at least except among the Eastern nations; Tcheou Kong, the regent of China, measured it about 1100, and his result is only about 2½ minutes greater than it should have been); and he made it 23° 51′ 13″. It was found by various astronomers, after him, to be 23° 35′; 23° 29′ 47″; 23° 29′, &c. The constant decrease was noticed. It was doubted for long whether the obliquity did not remain constant, only that astronomical errors of observation had crept in: not until the theory of gravitation was established, was the question finally settled. Euler shows that it is decreasing, and must be so for a very long period. The formula was subsequently found to be $\theta = 23° \cdot 27' 54'' \cdot 8 - 0'' \cdot 488566\, t - 0'' \cdot 000005\, t^2$, where θ is the obliquity and t the number of years counted from 1800. This formula gives a sufficiently accurate result for about 1200 years behind or before; but, after that, other terms of the formula, introducing t^3, require to be taken into account.—The physical cause of this change in the obliquity of the ecliptic is the action of the other planets, especially Jupiter, Mars, and Venus, upon the mass of the Earth. The variation, however, is, like most variations of nature on so large a scale, one that oscillates within narrow limits. If the equator and ecliptic came together, we should have no seasons whatever, and the whole economy of our world would be disturbed.

Eidograph. See PANTOGRAPH.

Elasticity is the property which bodies possess of occupying, and tending to occupy, portions of space of determinate volume, or determinate volume and figure, at given pressures and temperatures, and which, in a homogeneous body, manifests itself equally in every part of appreciable magnitude.

2. An *Elastic Pressure* is a force exerted between two bodies at their surface of contact, or between two parts into which a body either is divided or is capable of being divided at the surface of actual or ideal separation between those parts. The magnitude of an elastic pressure is stated in *units of weight* per *unit of area* of the surface at which it acts.

Values of various units of Elastic Pressure in pounds avoirdupois per square foot.

One pound per square inch	144
One inch in height of a mercurial column, at the temperature of melting ice	70·73
One atmosphere of 29·922 inches of mercury,	2116·4

3. The elasticity of a *perfect fluid* is such that its parts resist change of volume only, and not change of figure; whence it follows, that the pressure exerted by a perfectly fluid mass is wholly perpendicular to its surface at every point: principles which form the basis of hydrostatics and hydrodynamics.—Fluids are either gaseous or liquid. A *Gaseous Fluid* (popularly called an "Elastic Fluid") is one whose parts (so far as is known by experiment) exert a pressure against each other and against the vessel containing them, how great soever the volume to which they are expanded. See HEAT—MECHANICAL ACTION OF, sec. 9, 10, 11, 35; also BAROMETER, VAPOUR and SOUND.

4. The elasticity of a *Perfect Liquid* resists change of volume only, and differs from that of a gaseous fluid chiefly in this: that the greatest variations of the pressure which it is possible to apply to a liquid mass produce very small variations of its volume.

5. The *Compression* undergone by a liquid mass in consequence of the application of a given pressure over its surface, is measured by the ratio of the diminution of volume produced by the given pressure to the entire volume of the mass: a ratio which is always a very small fraction.—The *Compressibility* of a given liquid is the compression produced by a unit of elastic pressure; in other words, the ratio of a compression to the pressure producing it.—The *Modulus or Coefficient of Elasticity* of a liquid is the ratio of a pressure applied to and exerted by the liquid, to the accompanying compression, and is therefore the reciprocal of the compressibility.—The compressibility of a liquid is ascertained by means of an instrument called a *Piezometer* (*q.v.*)—The following empirical formula for the compressibility of pure water at any temperature between 32° and 128° Fahrenheit has been deduced by Mr. Rankine from the experiments of M. Grassi

(*Comptes Rendus*, *XIX.*; *Philos. Mag.* June 1851).—*Compressibility per Atmosphere*,

$$= \frac{1}{40\ (\text{T} + 461°)\ .\ \text{D}}.$$

T, temperature in degrees of Fahrenheit. D, density of water at that temperature under one atmosphere, the maximum density of water under the pressure of one atmosphere being taken as unity. At the temperature of maximum density, 39° Fahr., the compressibility of water per atmosphere is 0·00005, and its modulus of elasticity, 20,000 atmospheres.

Compressibilities of some Liquids, per Atmosphere, from M. Grassi's experiments.

Saturated aqueous solution of nitrate of potash	0·0000306565
Saturated aqueous solution of carbonate of potash	0·0000303294
Artificial sea water	0·0000445029
Saturated aqueous solution of chloride of calcium	0·0000209830
Æther	0·00011137 to 0·00013073
Alcohol	0·00008245 to 0·00008587

The compressibility of æther and alcohol increases with the pressure.

6. A *Solid* body, besides resisting change of volume like a liquid, possesses also *Rigidity*, or the property of resisting change of figure.—As in the case of liquids, the utmost alteration of volume of which a solid body is capable by any pressure which can be applied to it, is always a very small fraction of its entire volume.—The elastic pressures at the surface of a solid body or particle are not necessarily normal, but may have any direction, from normal to tangential.

7. In popular language the words *Strain* and *Stress* are applied indifferently to denote either the system of pressures at the surface of a solid body whereby its volume and figure are altered, or the alteration of volume and figure of the body and its parts thereby produced. For the sake of clearness in scientific language, certain authors have recently endeavoured to appropriate the word *Strain* to the alterations, of what nature soever, in the volume and figure of a solid body and of its parts, produced by forces applied to it, and the word *Stress* as synonymous with Elastic Pressure, or combination of elastic pressures. This nomenclature will be used in the present article.—*Fracture* of a solid occurs when a strain is carried so far as to cause actual division of the solid into parts. The strains and fractures to which a solid, considered as a whole, is subject, may be classified according to the following table. To each kind of strain there corresponds a kind of stress; being the external force which produces that strain and the equal and opposite force wherewith the solid resists that strain.

	Strain.	Fracture.
Longitudinal..	Extension	..Tearing.
	Compression	..Crushing and Cleaving.
Transverse....	Distortion	..Shearing.
	Torsion	..Wrenching.
	Bending	..Breaking across.

8. A body is said to be *Perfectly Elastic* which, if strained at a constant temperature by the application of a stress, recovers its original volume, or volume and figure, when such stress is withdrawn, and gives out, during such recovery, a quantity of mechanical work exactly equal to that originally exerted in producing the strain. Deviations from this property constitute *Imperfect Elasticity*. Gases, and liquids perfectly free from viscosity, are perfectly elastic.—The elasticity of every solid is sensibly perfect when the strain does not exceed a certain limit. This has been proved to be the case even for solids so plastic as moistened clay. For every solid there are limits, which if a strain exceed, *set*, or permanent alteration of volume or figure, is produced; and such *Limits* of *Elasticity* are less, and often considerably less, than the strains required to produce fracture. It has been proved by Mr. Hodgkinson that these limits depend on the duration of the strain, being less for a long-continued strain than for a brief strain. The *elasticity of volume* in solids is in general much more nearly perfect than the *elasticity of figure*. It is true that the density of many metals is permanently increased by hammering, rolling, and wiredrawing, and that of some other materials by intense pressure (Fairbairn; Report of the British Association, 1854;) but the stresses which operate during these processes are very great.—The degree to which the elasticity of a solid substance is imperfect is sometimes measured by means of experiments on the disappearance of vis-viva during the collision of two balls of the substance experimented upon. The mechanical work expended in permanently altering the volume or figure of an imperfectly elastic body produces heat according to the same law with work expended in friction. See HEAT, MECHANICAL ACTION OF. A body which is capable of undergoing great alterations of figure, and whose elasticity of figure is very imperfect, is a *plastic solid*. The gradations are insensible between plastic solids and *viscous liquids*, in which there is a resistance to change of figure, but no tendency to recover any particular figure.—*Rise of Temperature*, so far as we yet know, increases elasticity of volume in all substances, and at the same time diminishes the amount and the perfection of elasticity of figure, so as to make solids more plastic and liquids less viscous.—See HEAT.

9. The *Ultimate Strength* of a solid is the stress required to produce fracture in some specified way. The *Elastic Strength* is the stress required to produce the greatest strain of a specific kind, consistent with perfect elasticity. Strength, whether ultimate or elastic, is the product of two quantities, which may be called *Toughness* and *Stiffness*. *Toughness*, ultimate or elastic, is here used to denote the greatest strain which the body will bear without fracture, or without imperfection of elasticity, as the case may be:—*Stiffness*, which

might also be called *Hardness*, is used to denote the ratio borne to that strain by the stress required to produce it,—being, in fact, a *modulus of elasticity* of some specified kind. *Malleable* and *Ductile* solids have ultimate toughness greatly exceeding their elastic toughness. *Brittle* solids have their ultimate and elastic toughness equal or nearly equal.—*Resilience* or *Spring* is the quantity of *mechanical work* required to produce the limiting strain of perfect elasticity, and is equal to the product of that strain, by the *mean stress* in its own direction which takes place during the production of that strain:—such stress being either exactly or nearly equal to one-half of the stress corresponding to the limiting strain. Hence the resilience of a solid is exactly or nearly one-half of the product of its elastic toughness by its elastic strength; in other words, one-half of the product of the square of its elastic toughness by its stiffness.—Each solid has as many different kinds of Toughness, Strength, and Resilience as there are different ways of straining it.

10. The theory of the elasticity of solids has been reduced to a body of *mathematical principles* applicable to those cases in which the strains of the particles of the body are so small, that quantities in the stresses depending on the squares, products, and higher powers of the strains may be neglected without appreciable error, and that, consequently, *Hooke's Law*,—"ut Tensio sic Vis"—is sensibly true for all relations between strains and stresses. This condition is fulfilled in nearly all cases in which the strains are within the limits of perfect elasticity—the exceptions being a few substances, very pliable and at the same time very tough, such as caoutchouc. The mathematical theory, as thus limited, consists of three parts, viz., the Resolution and Composition of Strains, the Resolution and Composition of Stresses, and the relations between Strains and Stresses.

11. Let a solid of any figure be conceived to be ideally divided into a number of indefinitely small cubes by three series of planes parallel respectively to three co-ordinate planes. Each such elementary cube is distinguished by means of the distances, x, y, z, of its centre from the three co-ordinate planes.—If the solid be strained in any manner, each of the elementary cubical particles will have its dimensions and figure changed, and will become a parallelopiped which may be right or oblique—its size being conceived to be so small, that the curvature of its faces is inappreciable.—The *Simple* or *Elementary Strains* of which a particle, cubical in its free state, is susceptible, are six in number, viz.:—three *longitudinal* or *direct strains*, being the three proportional variations of its linear dimensions, which are elongations when positive, and compressions when negative; and three *transverse strains*, being the three *distortions*, or variations of the angles between its faces from right angles, which are considered as positive or negative according to some arbitrary but fixed rule, and are expressed by the proportions of the arcs subtending them to radius. When the values of these six strains for every particle are expressed by functions of the co-ordinates x, y, z, the state of strain of the solid is completely expressed mathematically.—The six elementary strains, in the cases to which the theory is limited, are very small fractions.—The method of reducing the state of strain of the solid at a given point, as expressed by a system of six elementary strains relatively to one system of rectangular axes, to an equivalent system of six elementary strains relatively to a new system of rectangular axes, is founded on the following theorem. Let α, β, γ, be the longitudinal strains of the dimensions of a given particle along x, y, z, λ, μ, ν, the distortions of its angles in the planes y z, z x, x y. Conceive the surface of the second order whose equation is

$$\alpha x^2 + \beta y^2 + \gamma z^2 + \lambda y z + \mu z x + \nu x y = 1.$$

Transform this equation so as to refer the same surface to the new axes of co-ordinates; the six coefficients of the transformed equation will be the elementary strains referred to the new axes. Other ways of resolving strains have been pointed out by Professor W. Thomson, *Cambridge and Dublin Mathematical Journal*, May, 1855.—The sum of the direct strains $\alpha + \beta + \gamma$, represents the cubic dilatation of a particle when positive, and the cubic compression when negative. The state of strain of a transparent body may be ascertained experimentally by its action on Polarized Light. On this subject experiments have been made by Fresnel, Sir D. Brewster, M. Wertheim, and Mr. Clerk Maxwell.

12. Let ξ, η, ζ, be the projections, parallel to x, y, z, respectively, of the *displacement* of a particle in a strained solid from its position when the solid is free, expressed as functions of x, y, z. Then

$$\alpha = \frac{d\,\xi}{d\,x}; \quad \beta = \frac{d\,\eta}{d\,y}; \quad \gamma = \frac{d\,\zeta}{d\,z};$$

$$\lambda = \frac{d\,\zeta}{d\,y} + \frac{d\,\eta}{d\,z}; \quad \mu = \frac{d\,\xi}{d\,z} + \frac{d\,\zeta}{d\,x};$$

$$\nu = \frac{d\,\eta}{d\,x} + \frac{d\,\xi}{d\,y}.$$

13. The elastic pressures exerted on and by an originally cubical particle, which constitute the state of stress of the solid at the point where that particle is situated, may be resolved into six *Elementary Stresses*, viz.:—three *Normal Stresses*, perpendicular respectively to the three pairs of faces, and tending directly to alter the three linear dimensions of the particle—and three pairs or *Tangential Stresses*, that is, pressures acting *along* the double-pairs of faces to which they are applied, and tending directly to alter the angles made by such double-pairs of faces. To reduce

the state of stress at a given point expressed by a system of six elementary stresses referred to one system of rectangular co-ordinates to an equivalent system of elementary stresses referred to a new system of rectangular co-ordinates, let P_1, P_2, P_3, be the three normal stresses, and Q_1, Q_2, Q_3, the three tangential stresses; conceive the surface whose equation is,

$$P_1x^2 + P_2y^2 + P_3z^2 + 2Q_1yz + 2Q_2zx + 2Q_3xy = 1;$$

transform this equation so as to refer the same surface to the new set of axes; the six coefficients of the transformed equation will be the six elementary stresses referred to the new axes. For the complete investigation of this subject, see M. Lamé's *Leçons sur la Théorie Mathématique de l' Elasticité des Corps Solides*, Paris, 1852.

14. The *Potential Energy of Elasticity* of an originally cubic particle in a given state of strain is the *work* which it is *capable of performing* in returning from that state of strain to the free state; and is the product of the volume of the particle by the following function:—

$$U = \frac{1}{2}(\alpha P_1 + \beta P_2 + \gamma P_3 + \lambda Q_2 + \mu Q_2 + \nu Q_3).$$

This function was first employed by Mr. Green, *Cambridge Transactions*, vol. vii.

15. According to Hooke's Law each of the six elementary stresses P_1, &c., may, without sensible error, be treated as a linear function of the six elementary strains, α, &c., each multiplied by a particular *Coefficient* or *Modulus of Elasticity.* By expressing all the stresses in terms of the strains, the Potential Energy U is transformed into a homogeneous quadratic function of the six elementary strains, which must have twenty-one terms, and consequently *twenty-one Coefficients*, multiplying respectively the six half-squares, and the fifteen binary products, of the six elementary strains. The coefficient of $\frac{1}{2}\alpha^2$ in U is that of α in P_1; the coefficient of $\alpha\beta$ in U is that of α in P_2 and also that of β in P_1; and so on.

16. According to Hooke's Law also, each of the six elementary strains may be treated, without sensible error, as a linear function of the six elementary stresses, so as to transform U to a homogeneous quadratic function of the elementary stresses P_1, &c., having twenty-one terms, and twenty-one coefficients expressing different kinds of *Pliability*. The word "Pliability" is here used in an extended sense, to include liability to alteration of figure of every kind, whether by elongation, linear compression, or distortion.

17. Coefficients, whether of Elasticity or of Pliability, may be thus classified:—*Direct*, or *Longitudinal*, when they express relations between longitudinal strains, and normal stresses in the same direction.—*Lateral*, when they express relations between longitudinal strains, and normal stresses in directions at right angles to the strains.—*Transverse*, when they express relations between distortions, and tangential stresses in the same direction.—*Oblique*, when they express any other relations between strains and stresses.

18. An *Axis of Elasticity* is any direction in a solid body, with respect to which some kind of symmetry exists in the relations between strains and stresses. Various kinds of axes have been pointed out by Mr. Haughton, *Trans. Roy. Irish Acad. XXIII.*, and Mr. Rankine, see *Philosophical Transactions*, 1855. In particular, an *Axis of Direct Elasticity* is a direction in a solid body such, that a longitudinal strain in that direction produces a normal stress, and no tangential stress on a plane normal to that direction. Every such axis is a direction of maximum or minimum direct elasticity relatively to the directions adjacent.—By the aid of the Calculus of Forms, and of an improvement in the Geometry of Oblique Co-ordinates, it has been shown by Mr. Rankine that every homogeneous solid must have *at least three* axes of Direct Elasticity, which may be rectangular or oblique with respect to each other,—that the number of such axes increases as the symmetry of the action of elastic forces becomes greater,—and that their various possible arrangements correspond exactly with those of the normals to the faces and edges of the various *Primitive Crystalline Forms.*

19. In an *Isotropic* or *Amorphous Solid* the action of Elastic Forces is alike in all directions. Every direction is an Axis of Elasticity. The coefficients of Oblique Elasticity and Oblique Pliability are all null. The number of different coefficients of Elasticity, and of different coefficients of Pliability, is three. The following notation and equations show their relations to each other:—

ELASTICITIES.

Direct, ... $A = \dfrac{\mathfrak{a} - \mathfrak{b}}{\mathfrak{a}^2 - \mathfrak{a}\mathfrak{b} - 2\mathfrak{b}^2}$;

Lateral, ... $B = \dfrac{\mathfrak{b}}{\mathfrak{a}^2 - \mathfrak{a}\mathfrak{b} - 2\mathfrak{b}^2}$;

Transverse, ... $C = \dfrac{A - B}{2}$;

Elasticity of volume, $\dfrac{1}{\mathfrak{h}} = \dfrac{A + 2B}{3}$.

PLIABILITIES.

Direct, ... $\mathfrak{a} = \dfrac{A + B}{A^2 + AB - 2B^2}$,

(otherwise, the extensibility).

Lateral, ... $\mathfrak{b} = \dfrac{B}{A^2 + AB - 2B^2}$;

Transverse, ... $\mathfrak{c} = \dfrac{1}{C} = 2(\mathfrak{a} + \mathfrak{b})$;

Cubic compressibility, $\mathfrak{h} = 3\mathfrak{a} - 6\mathfrak{b}$.

The quantity to which the term "*Modulus of*

Elasticity" was first applied by Dr. Young, is the reciprocal of the Extensibility, or Longitudinal Pliability; that is to say,

$$\frac{1}{\mathfrak{a}} = \text{A} - \frac{2\,\text{B}^2}{\text{A}+\text{B}}.$$

This quantity expresses the ratio of the normal stress on the transverse section of a bar of an isotropic solid to the longitudinal strain, *only when the bar is perfectly free to vary in its transverse dimensions*, but not under other circumstances. In the commonly received theory of the strength of materials, it has been erroneously taken for granted that Young's Modulus expresses that ratio under all circumstances. The magnitude of some of the errors to which this assumption leads is shown in Mr. W. H. Barlow's experiments on the flexure of Cast Iron Beams, (see *Philosophical Transactions*, 1855). The values of Young's Modulus have been determined experimentally for almost every solid substance of importance, and tables of them have been published in treatises on mechanics. (See, in particular, *Leçons sur la Résistance des Matériaux, par le Général Morin*, Paris, 1853.) Those of the Transverse Elasticity C have been deduced from experiments on Torsion, of which many have been made, especially by Bevan and Savart. The only complete sets of Coefficients of Elasticity and Pliability which have yet been computed are those for Brass and Crystal, deduced from the experiments of M. Wertheim, (*Annales de Chimie*, 3d series, vol. 23), and are as follows: the unit of pressure being *one pound on the square inch*:—

	Brass.	Crystal.
A	22,224,000	8,522,600.
B	11,570,000	4,201,100.
C	5,327,000	2,159,100.
$\frac{1}{\mathfrak{d}}$	15,121,000	5,643,800.
$\frac{1}{\mathfrak{a}}$	14,300,000	5,746,000
$\mathfrak{a}$	0·0000000699	0·0000001740.
$\mathfrak{b}$	0·0000000239	0·0000000575.
$\mathfrak{c}$	0·0000001877	0·0000004631.
$\mathfrak{d}$	0·0000000661	0·0000001772.

20. The general problem of the *Internal Equilibrium of an Elastic Solid* is this:—given the free form of a solid, the values of its coefficients of elasticity, the attractions acting on its particles, and the stresses applied to its surface: to find its change of form, and the strains of all its particles. This problem is to be solved, in general, by the aid of an ideal division of the solid (as already described) into molecules rectangular in their free state, and referred to rectangular co-ordinates. For isotropic solids, some particular cases are most readily solved by means of spherical, cylindrical, or otherwise curved co-ordinates. (See Lamé, *Leçons*, &c.; Maxwell, *Edinburgh Transactions*, *XX.*; Rankine, *Manual of Applied Mechanics*.

21. Many attempts have been made to simplify the theory of elasticity by the aid of *Hypotheses* as to occult molecular structures in bodies. That most cultivated has been the hypothesis of Boscovich, that bodies are systems of physical points or centres of force, occupying space solely in virtue of attractions and repulsions between them. A necessary consequence of this hypothesis is, that for all isotropic bodies, A=3 C. This consequence is contradicted by experiment: therefore the hypothesis is untenable. Another hypothesis is that of Æpinus, Franklin, Mossotti, and others, which, to atomic nuclei, like the physical points of Boscovich, superadds elastic atmospheres enveloping them. As to the Hypothesis of Molecular Vortices, see HEAT, MECHANICAL ACTION OF, secs. 34, 35. This hypothesis has been serviceable in the theory of the relations between elasticity and heat;—but when the theory of elasticity is considered in itself, it is unquestionably best to avoid hypotheses altogether.

22. The subject of Elasticity has been investigated mathematically and experimentally by a multitude of authors too great to be completely enumerated here; but amongst them may be specified the names of Galileo, Leibnitz, Huyghens, Hooke, Boyle, Newton, the Bernouillis, Euler, Boscovich, Coulomb, Dupin, Marriotte, Robison, Young, Rennie, Bevan, Tredgold, Brewster, Fresnel, Gauss, Savart, Navier, Poisson, Oersted, Colladon, Sturm, Mossotti, Cauchy, Lamé, Clapeyron, Grassi, Regnault, Wertheim, St. Venant, Poncelet, Morin, Green, Stokes, M'Cullagh, Haughton, Kelland, Hodgkinson, Fairbairn, P. Barlow, W. H. Barlow, Forbes, W. Thomson, J. Thomson, Gordon, Jellett, Maxwell, Rankine. As to the vibrations of Elastic Solids, see SOUND.

Electrical Egg: long known as an interesting philosophical toy, and a favourite in the popular lecture-room. As in other by no means unfrequent cases, however, it has suddenly assumed the position of a philosophical instrument of high import, revealing phenomena worthy to arrest the attention of such men as Grove, Gassiot, Rwhmkorff, Quet, Plücker, and Dr. Robinson of Armagh. We shall rapidly indicate the nature of the appearances that are sustaining inquiries so sedulous and grave, and present afterwards a *resumé* of the present state of the problems thus originated.

I. The INSTRUMENT itself is exceedingly simple and well known. It consists of an oval glass vessel, like that in fig. 1, so constructed, that the air or gas within it can be attenuated by means of connection with an air-pump; and through which approximate vacuum an electric discharge may be passed by aid of the rods indicated in the same figure. At the ends of these rods, balls are represented, as the positive and negative electrodes; but these may be replaced by platinum points, or by discs,

according to the object of the experimenter. The primary requisition made of the instrument, being merely that it permit an electric spark or current to pass through an attenuated gas, it is clear that the form of it is by no means confined to the one represented; and this form has been varied accordingly. It occurred to Mr. Gassiot, that by employing the Torricellian vacuum, and then hermetically sealing the tube, the cumbrous appendage of the air-pump might be dispensed with. He carried out his idea most successfully, and with a rich harvest of results. Their shape is shown in figs. 2 and 3, where the bent platinum wires constitute point-electrodes in the one case, and external strips of foil in the other, for the induced discharge. But the most important addition to our instrumental resources in this case, unquestionably came from M. Geissler of Bonn, who not only succeeded in sealing tubes filled with varieties of gases of all degrees of density, but conceived the idea of diversifying the shape of the tubes, so that the effects of another element might be tested and determined. He has made them accordingly as globes, as pear-shapes—one of which is drawn in fig. 10 (see below). One of his favourite forms is rather complex;—the centre is an ellipsoid, not unlike the original egg; the two ends containing the platinum electrodes are globes, and these are connected with the central ellipsoid by tubes of different diameters. In devising these forms, most of which present peculiar phenomena,—Geissler has had the great advantage of the aid and counsel of Plücker. Mr. Ladd, of Chancery Lane, London, has recently imported a number of Geissler tubes, so that they may easily reach the hands of British inquirers.

Fig. 1.

II. Turn now to the PHENOMENA.—The *general* phenomena are these. The electrical egg, as known originally, was supposed to manifest no appearance, save the brilliant one represented in fig. 1. After the air had been sufficiently exhausted, and a succession of electric sparks was being sent through the approximate vacuum, the negative pole (the lower one in the figure)

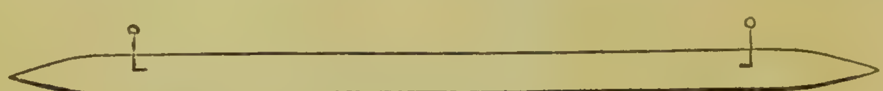


Fig. 2.

Fig. 3.

became surrounded by a blue aureola, while from the positive or upper pole, streams or billows of superb fiery red passed downwards to the neighbourhood of the negative pole, from which, however, they were always separated by a dark space, varying in breadth with change of circumstances. This fine experiment could be prolonged for hours—as long, indeed, as the vacuum was kept up, and the succession of sparks continued. It was commonly employed to indicate the probable nature of the northern auroras.—In 1852, however, the phenomenon assumed quite another aspect. An observation on the part of Mr. Grove—which must have been as accidental, and may yet prove as fertile as the famous one by Malus—called his attention to the interior structure of these developments of light. He found that, in certain circumstances of exceedingly frequent occurrence, the light is *stratified.* The blue envelope of the negative pole is not single; but consists of at least three envelopes, the central one being the darkest. It is, however, in the streamers from the positive pole that this stratification is most distinctly seen. Its aspects vary with circumstances; but fig. 4 may be accepted as a sufficiently characteristic eye sketch of its more distinctive features. On filling the tubes with the different gases, the phenomena become perplexingly diversified; nor perhaps is the time yet come for a sufficiently simple classification of their appearances. There are, however, two elements essential to the production of the phenomena, that must exercise a principal sway, viz., the power of the electric current or of the electromotor, and the quality of the attenuated gas. Other circumstances interfere; for instance, the quality of the gas may be changed by the persistent action of the current, *e. g.*, Oxygen may pass into Ozone, and compound gases are generally decomposed. Of these circumstances, however, we shall not attempt to take account; and in our short exposition we shall largely follow the track of Dr. Robinson, who has examined the subject with his usual

Fig. 4.

sagacity and faithfulness. (See *Proceedings of the Royal Irish Academy* for January and December, 1846, and *Philosophical Magazine* for April, 1859).

The following diagram, fig. 5, will impress far better than any description, the phenomenon as

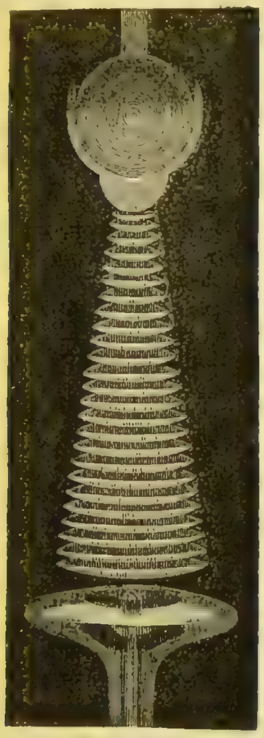


Fig. 5.

obtained by Dr. Robinson, when the medium in the receiver was attenuated pure Hydrogen. The large upper ball is the positive electrode; the negative electrode a point, below which a disc had been placed. The disc and stem of the negative electrode had its usual three envelopes; but the issue from the ball seems to have been of very great splendour. As the woodcut shows, it was trumpet-shaped, and consisted of a succession of menisci, separated from each other by about a quarter of an inch. This mass of menisci was also *in rotation* around the longitudinal axis. Now these phenomena are common to all such experiments. In all cases we have the menisci separated by these dark bands; but when the power is high, their curvature is reversed towards the end of the positive stream. In all cases we have their rotation, and the quiescence of the light around the negative electrode, which is likewise invariably divided into two envelopes, sundered by a dusky interval.

The foregoing phenomena, therefore, must be owing to the only agency invariable in their production, viz., a discharge through an imperfect but mobile conductor.—The manner of discharge, which seems to facilitate the evolution of these appearances, is stated by Dr. Robinson to be the following:—1. The electrodes should be guarded points, that is to say, pointed platinum wires, surrounded by small glass tubes up to the very point. Geissler, we believe, always employs such.—2. The positive electrode should be the upper one. In the reverse arrangement, the ascending currents of heated air seem to cause confusion.—3. A very weak electromotive power will give no strata or menisci; while, on the other hand, a very strong force conceals them. A spark of a *tenth*, or even a *fifth* of an inch, fails to evolve them even in a Torricellian vacuum: very powerful discharges, again, cause the bright streaks to throw out cloudy appendages into the dark intervals; and sparks of five inches altogether overcome that resistance of the imperfect conduction to which the phenomena are probably owing,—entirely filling these intervals with light. It is found also that a more perfect attenuation of the medium compensates to a certain degree for the greater feebleness in the discharge.

If from these general phenomena and their conditions, we turn to the variations due to the use of different gases as media, condensed description becomes exceedingly difficult. There are, however, two phenomena even here, of a comparatively general nature, that ought to be especially noted. (1.) In many cases the rarified gases became *phosphorescent* under the influence of the discharge; that is to say, they continue to give out light after the cessation of that discharge. The circumstances under which such appearances take place are now being studied by Edmund Becquerel. This physicist thinks that he can distinguish two sorts of *persistence*.—*First*, in the case of rarefied hydrogen, sulphuretted hydrogen, protoxide of nitrogen, and chlorine, faint gleams of persistence are discerned; but the action appears *limited to the internal surface of the glass tube.* Becquerel shows clearly enough that this cannot be due to *phosphorescence* of the *glass* itself; "therefore," he concludes, "the effect presented by tubes containing these gases appears to be the result of the *electrization* of the glass, or of the adherent gaseous stratum." It is not likely to be due to the electrization of the glass, because it does not appear in all cases when an inductive current is transmitted through a glass tube.—*Secondly*, with oxygen, with sulphurous acid gas, with air slightly impregnated with phosphorus, &c., the effect is different. When an inductive discharge passing through rarefied oxygen, for instance, is suddenly stopped, the whole tube appears to be illuminated with a yellow tint that persists for several seconds after the interruption, and decreases with more or less rapidity until it disappears. The illuminating action, as seen in such cases, evidently takes place between the actual molecules of the gas, and does not pass along the walls of the tube, for on making use of *spheres* of large capacity, instead of tubes,

the *entire mass* of the gas becomes opaline. Becquerel remarks that the phenomenon probably depends on a peculiar action produced by the electric *currents;* for solar light, or even the electric *light* itself, does not give rise to any such phosphorescence. And he asks,—"Is it the result of vibrations impressed upon the molecules of the gases, or of a peculiar state of molecular tension persisting for a few moments, or of some other physical or chemical cause?"—(2.) With some media, the *fluorescent* rays of Mr. Stokes are very largely developed; employing others, not a trace of these rays can be discerned. For instance, when sufficiently rarefied atmospheric air is the medium, the fluorescent rays appear in abundance; and this also occurs if we substitute either of its main constituents, oxygen or nitrogen. On the other hand, hydrogen, sulphur, and carbon, are pre-eminently *anti-fluorescent;* hydrogen above them all. This singular quality accompanies the bodies named through their components, although sometimes the one and sometimes the other antagonist element prevails. Ammonia, for instance, is not sensibly fluorescent—the energy of the hydrogen prevailing over that of the nitrogen. But cyanogen is very fluorescent; the nitrogen in this instance overmastering the carbon. The remote cause of this well marked distinction is as yet unknown. Nevertheless, the enumeration just made cannot fail to suggest the inquiry,—whether fluorescence or anti-fluorescence depends in any degree *on the electro-negative or electro-positive character of the media.*

The strata, as we have said, greatly vary with the media, in the *colour* of their light. With atmospheric air the mass of the menisci are violet, and seem enveloped in a faint yellowish light. With hydrogen the light is a pale greenish-blue. With oxygen and nitrogen the colour is nearly the same as with common air. The oxygen, however, seems to pass into ozone. Nitrogen yields a positive light, more pink than that of air; while the envelopes of the negative electrode are indigo. On continuing the exhaustion in the latter case, the stream assumes the hue of a tawny brown. With carbonic oxide the stream is a bright green, yellowish at the positive end, bluish at the other. The stream is also *green* with carburet of sulphur, coal-gas, vapour of chloroform, vapour of alcohol, vapour of camphor, &c.; while livid blue and lilacs are evolved when the media are rarefied sulphurous acid gas, cyanogen, naphtha, ammonia, &c., &c. It remains for inquiry to determine how far these singular varieties may be found connected *with the density of the medium and its electric conducting power.*

III. Whatever the origin of the singular striæ we have been describing, they may certainly be considered as lines or discs of electric force. And accordingly it was no rash conjecture that the neighbourhood of a magnet would make itself be felt by changes on their form and distribution. Gassiot has made several allusions to this new part of the subject in the Bakerian Lecture of last year, establishing that stratifications arising from the direct discharge taken from two point electrodes, tend to rotate around the poles of a magnet, according to the well known law of magnetic rotations. When a reciprocating discharge is effected (obtained by coating two portions of a Torricellian tube externally with stripes of tin-foil), this discharge was found by Gassiot to be divided by the magnet—the two divisions having a tendency to rotate in different directions. —The most extensive experiments on the subject, however, are due to Plücker of Bonn. His memoirs—originally printed in Poggendorff's *Annalen*, No. ciii. and civ., and reproduced in an English dress in the second volume of the *Philosophical Magazine* of 1858—are so replete with new, curious, and diversified facts, that a satisfactory analysis of them within our brief space is impossible. Putting aside the changes of colour and every feature apparently depending on the varying nature of the medium, we shall treat only of the *constant* portion of the phenomena described. Plücker's researches refer mainly to phenomena connected with the strata and the negative electrode; but he has recorded valuable information also regarding the influence of magnetic action on the general current, or on the strata or menisci streaming from its opposite. Taking, for instance, a Geissler's tube of the form already described—viz.: a central ellipsoid, joined to two terminal spheres, within which the platinum points were placed—he states that on feeling the magnet approach, the light entering the ellipsoid becomes concentrated into a luminous arch, as in fig. 6, traversing the upper

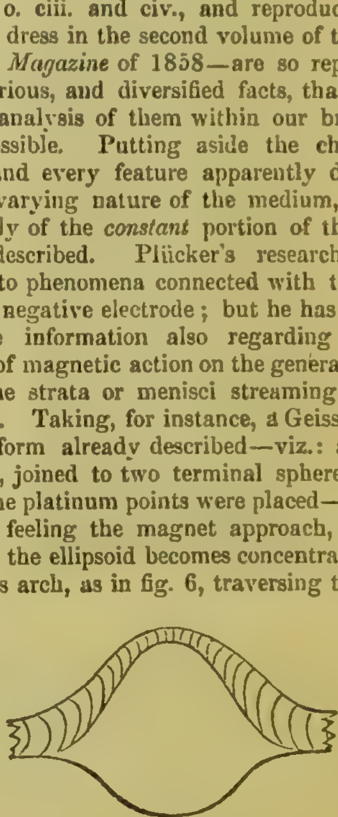


Fig. 6.

part of the bulb in the equatorial plane of the magnet. On entering the ellipsoid, the stratification of the light became finer; at the upper part of the arch where the concentration of light was greatest, the dark intervals became more and more numerous and distinct—in fact, it presented the aspect of a *repelled arch of stratified light.*—Let us turn, however, to the researches of our physicist as to the strata around the negative electrode. It will be recollected that in the ordinary discharge, and in the ordinary phenomena of stratification, the negative electrode is surrounded in its immediate neighbourhood by an envelope of variously coloured and finely stratified light—separated by a dark interval from the stratified mass, streaming from the opposite pole. These fine layers are spherical when the positive

electrode itself is spherical: and they have a cylindrical form when the electrode projects into the tube as a wire. Now this stratification is quite broken up by the neighbourhood of a magnet, and thrown into the form of magnetic curves. Plücker, in all these inquiries, made much use of the irregular Geissler's tube, described above, and the following are a few of his results. Previous to the approach of the magnet, in the globe containing the negative electrode, a diffuse violet light existed, surrounded by a pale green light, clinging to the internal surface of the glass. On placing the globe between the armatures of a powerful magnet, as below (fig. 7)

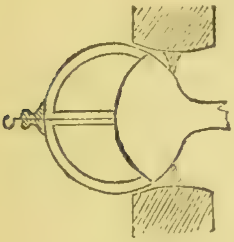


Fig 7.

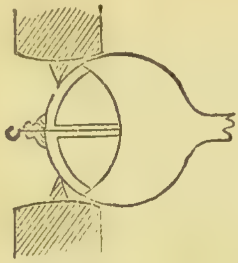


Fig. 8.

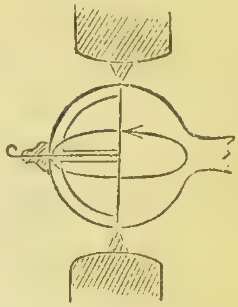


Fig. 9.

the diffuse violet light, collected into a horizontal, semilune, bright, and uniformly luminous disc, bounded towards the tube by a well defined concave arch. On its opposite side this disc was enveloped by a narrow strip of beautiful *bright green light*, filling the curvatures of the glass. On changing the position of the tube, in reference to the magnet, the changes passed on this disc which are represented in figs. 8, and 9. In fig. 9 the disc rotated in the direction of the arrow. Plücker concludes in general terms, that the planes or curved surfaces in which the diffused light spreading around the negative pole becomes concentrated, are formed of *lines of light which, proceeding from the separate points of the* POSITIVE *electrode, coincide with magnetic curves.* He employed next a long tube, tapering conically towards one end, laying it, as represented below, in an *axial* position. In fig. 10, one set of results are depicted. The stratification having refer-

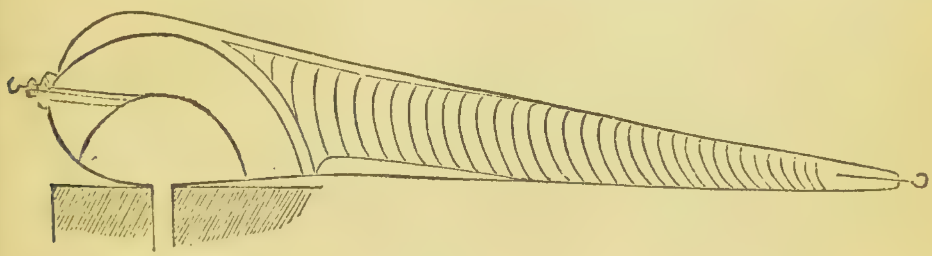


Fig. 10.

ence to the positive electrode was modified only in this: it did not extend to the sides of the tube through its lower part. But the light belonging to the negative electrode was thrown into two concentric arcs, whose centres coincide with the middle point between the higher sur-

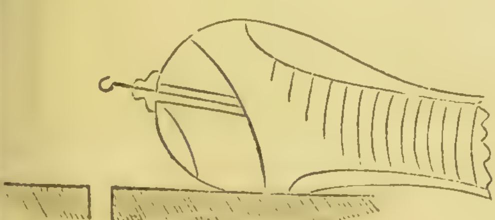


Fig. 11.

faces of the two armatures. Move now the tube to the position shown in fig. 11, and the phenomena are wholly changed. These two arcs have again assumed the form of magnetic curves. By the change thus effected, a remarkable commixture of the light occurs, and new colours appear when the ring opens and its light approaches that of the other part of the tube. The former of these was violet, the latter reddish; a beautiful blue light being formed on their approximation.—On inverting the poles, the light moves backwards and forwards in the neighbourhood of the negative electrode, and collects at the place where the surface formed by magnetic curves touches the glass.

Plücker has been able farther to discern the precise agreements of these curious phenomena with the well established dynamical relations existing between electric currents and the magnetic force. The gas through which the current passes must be viewed physically as simply an *absolutely flexible* conductor: and Plücker first lays down the following new electro-magnetic

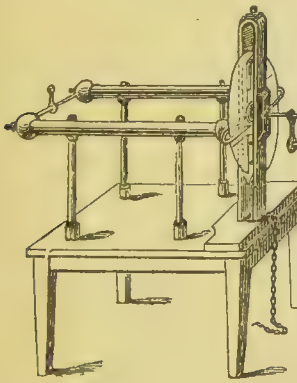
principle:—"*If an absolutely flexible conductor, traversed by a current, is subjected to the action of any system whatsoever of magnetic forces, it is necessary, and at the same time sufficient for equilibrium, that the current assume the form of a negative curve.*" Now there are three sets of circumstances under which this principle may act with different results:—(1.) Suppose the electric discharge to take place between two fixed points, as in the case of the common voltaic arch. Under influence of a magnet, this arch will take the form of a magnetic curve, *if its two extremities lie on the same magnetic curve.* If this is not the case, curious transformations occur not hitherto investigated.—(2.) One of the extremities of the discharge may be fixed, while the other is merely under the condition of being somewhere on a given surface. This happens with the luminous discharge around the negative electrode in Geissler's tubes—the discharge then terminating at one side in that electrode, and at the other in the internal surface of the glass. From this condition all the phenomena discovered by Plücker, connected with that electrode, are readily deducible.—(3.) Or lastly, both extremities of the discharge may be found, not in points, but on two surfaces, or two portions of given surfaces. This happens in experiments with the ellipsoidal swelling or centre of the tube of Geissler. This ellipsoid being placed on the armatures of a powerful electro-magnet, so that its axis be perpendicular to the line of the poles, there is necessarily formed in its interior, at a distance from the two electrodes, a luminous vault, presenting the form of a magnetic surface, terminating in all its parts in the internal side of the glass. Plücker seems to have found the complete explanation of this portion of the curious inquiry. This memoir is in *Poggendorff*, vol. civ.

IV. The important question remains, however, what is the *cause* of these *stratifications?* We have been able to describe the main phenomena; but nothing in the previous part of this paper tends to explain why they exist at all. Several speculations, indeed, have been hazarded, but none of them grasp the subject;—nor, perhaps, are the phenomena themselves fully or *adequately* known. At a comparatively early period, for instance, after Grove's discovery, the idea of *interference* was not unnaturally suggested,—an idea peremptorily negatived by Gassiot, who artificially produced "*interferences*" of two currents, without evolving any analogous striæ. M. Morren of Marseilles is probably not far from the truth, when he writes—"J'attribue la stratification a une variation dans l'intensité de la tension de l'electrité qui circule, mais surtout a l'insuffissance des corps conductives gazeux, a travers desquels le courant passe;" but he is quite in error when he imagines the phenomena represented by the forms of brushes, &c., received on a piece of glass, of lateral disruptions from a strong current, forced along a wire lying on the face of the glass. Rigorously examined, there is no resemblance whatever between these explosion-pictures and the electric stratification. Mr. Grove, to whose lightest opinions every consideration is due, has recently been inclined to attribute the phenomenon to the interaction of two discharges from the induction coil—one of the currents being the *true induction current*, and the other an *extra current.* In Dr. Robinson's more recent paper, however, this solution appears satisfactorily disposed of (*Philosophical Magazine*, April, 1859), so that, in so far as this goes, we are as much in the dark as ever. A recent note, by MM. Quet and Seguin, seems to have advanced one step. They have modified the original conception of M. Morren, and given experimental proof of its reality, as thus modified. An attenuated gas being simply a flexible or very mobile conductor, they have rightly asked, whether similar stratifications might not be produced experimentally, by directing the current through imperfect and mobile conductors, of a sort that would leave traces of the passage of the current in a visible form? They took accordingly a plate of glass, narrow and of some length, and having strewed it over with charcoal dust, they brought an induced current to act at its extremities. After the discharge had continued for some time, the dust arranged itself in *transversal lines*, separated by a small interval,—presenting an appearance entirely analogous to the stratifications of the Electrical Egg. Precisely the same appearances were evolved in the midst of a dark or smoky flame, such as that from the burning oil of turpentine, when the discharge passed through it. Their conclusion is, that these stratifications may be explained by the propagation of electricity through the conducting dust, causing a repulsion between the particles of the dust. "The foregoing experiments," they say, "prove that gases *electrized* are subject to electric attractions and repulsions, that imperfectly conducting and mobile media are disposed by these electric influences into beds of different densities, and that this disposition, giving rise to differences in tension, originates the luminous strata." The electric influences passing through an attenuated gas thus originate condensed and rarer beds: "that the dilated beds conducting electricity in the same way as is done by the metallic particles in the common experiment of the illuminated sheet,—the two fluids acquire on both sides of the more imperfectly conducting beds such a tension as enables electricity to traverse them under the form of a discharge, and therefore to illuminate them."—It is easy to see, that supposing MM. Quet and Seguin to be correct, they have done little more than produce analogous appearances. One thing alone is plain:—setting aside all specialties and variations, we have before us the fact, that the current passes through these im-

perfect and mobile conductors in such a way that successive zones become charged up to the point of disruption: so that we are forced on the difficult inquiry,—in what manner does the "mode of motion" which we term *Electricity* propagate itself through such conductors? perhaps also on the still more general one,—in what manner does this mode pass into that other mode of motion, which we term *Light*? Inquiries like these are rapidly coming within our reach; and while indulging in all Dr. Robinson's hopes, we agree with him, that the first essential step must be a thorough "mathematical investigation of a current's motion through an imperfect conductor."

Electrical Images. See IMAGES.

Electrical Machine. The electrical machine consists of a circular glass plate from $\frac{1}{12}$ to $\frac{1}{8}$ of an inch in thickness, and the diameter of which may vary at pleasure. The plate is traversed at its centre by a metallic axis fixed solidly to the glass. This axis rests on two wooden supports fixed vertically; and is so placed that the glass plate is situated between the two supports, and at an equal distance from either. A handle fixed on the extremity of the axis serves to give a rotatory motion to the plate. The greater part of the handle is made of glass or some very bad conductor of the electric influence.



Two pairs of horse-hair cushions are made to clasp, the one the upper, and the other the lower part of the plate; cushions which by their elasticity exert a considerable pressure on the plate. To facilitate the liberation of electricity, the surface of the leather of each cushion is coated with an amalgam of zinc, or of mosaic gold (deutosulphuret of tin). Finally, the vitreous electricity developed on turning round the glass plate is collected by cylindrical conductors generally of brass, that, with their ends near the plate, are placed horizontally and supported on glass pillars attached to the table.—In this machine, the cushions communicate with the ground by means of the wooden supports and the table: sometimes a metallic chain is added to facilitate this communication: thus the *negative* electricity which they acquire—as the positive is given off to the conductor—passes away as it is liberated: should we wish to collect it, the cushions themselves must be insulated by glass supports, and the construction of the machine slightly modified. Both electricities, or any one of them, may thus be manifested and collected by means of this machine. Formerly, in the construction of electrical machines, glass muffs or cylinders were employed, instead of circular plates; but although very powerful machines of the cylindrical sort have been constructed, the disc or plate is now almost always used.—The power of the plate machine—other things being equal—depends on the size of the plate: the largest ever made is the gigantic one at present in the Panopticon in Leicester Square, London, constructed after the designs of Mr. Marmaduke Clarke, of a plate whose diameter is about *eleven feet!* It can produce effects rivalling those of thunder.—The electricity given by the machine—as contradistinguished from that of the battery—is always in a state of high tension.

Electrical Machine—Armstrong's. *Hydro-Electric Machine.*—A most powerful method of obtaining electricity in a state of high tension; discovered accidentally by a workman who discerned electric sparks emitted by a locomotive engine; reduced to practice by Mr. Armstrong; and subsequently thoroughly investigated by Faraday. The electric excitation taking effect when steam at a high temperature issues from a tube attached to the steam boiler, it was at first imagined that the singular result is in somehow connected with the process of evaporation or vaporization; but Mr. Faraday, by a series of perfectly satisfactory experiments, has shown that this change of state, has nothing to do with the matter. It is purely a case of electricity created by *friction:* and the friction is the violent rubbing of particles of water (carried rapidly forward by the force of the steam) against the orifice through which they issue. *Dry steam* will not produce any effect; nor do effects follow when water is mixed with any saline or other material that converts it into a good conductor. The particles of other liquids carried forward with the same force, produce in every case the effects which we should expect from them employed as *rubbers.* Dry air produces no effect; but *powders* of different substances, carried by a stream of dry air, also evolve the electricity natural to them under friction.—This simple explanation grasps the entire case; and suggests many forms of apparatus. Mr. Armstrong has made a very effective one—of an insulated steam boiler, with openings conveniently placed, through which moist steam, or steam carrying particles of pure water, may rush, with whatever violence is desired. The mouthpieces of the issue pipes are usually of wood.

Electrical Machine—Rhumkorff's. See INDUCTIVE ELECTRICAL MACHINE.

Electricity. In some of the ancient Greek writings it is mentioned as a then well known

fact, that when a piece of amber is subjected to vigorous friction, it acquires the temporary power of attracting light bodies from a distance. A similar property appears to have been observed by the ancients in one or more bodies besides amber; but the fact of such a peculiar development of force by friction is stated in the ancient writings in special connection with the latter substance, probably because the property was first observed in that substance and most generally known in connection with it. The Greek name of amber is Electron, and hence the term Electricity, a term that has been universally adopted as the name of that peculiar agency first observed in rubbed amber, and as the name also of the science of that agency. The great use of the name of a science is to indicate clearly the subject matter of the science; and in this respect the term electricity is perhaps as good as any other that could be devised even in the present state of knowledge. But the name has a great additional interest to the student of nature from its historical connections: it forms, in fact, a permanent memorial of the smallness of those beginnings from which this, the most extensive of the physical sciences, has sprung. We cannot attempt to give in this article even the briefest sketch of the progress of electric discovery. We may only observe that Electricity is a science of purely modern growth. Down indeed till the 17th century, the science made no real advance beyond the fundamental fact already stated, if we except a slight extension of the list of substances excitable in the same way as amber. But since the appearance of Gilbert's work on magnetism in the year 1600 till the present day, the phenomena of electricity have been studied very closely and laboriously by many eminent philosophers. In consequence of their unwearied exertions, directed as they have been by the sound inductive spirit of the modern philosophy, discovery has followed discovery in rapid succession; new and wide fields of inquiry have been successively opened up, while the older fields were yet evidently unexplored; and powerful means of further discovery have been permanently secured from time to time by the invention of new scientific instruments and the perfecting of the old. So that in the present day, the science of electricity contains a very large and varied body of most interesting physical knowledge, and is replete also with clear illustrations of the principles, methods, and resources of the inductive philosophy. It will not be out of place to make now a brief statement or two in recommendation of the study of this branch of science. And first, if we look into the records of the sciences, we observe a very peculiar feature in the case of electricity. On several occasions in the course of discovery, this science has spoken for itself to the world as no other has. By the marvellous nature of its results it has forced the attention even of the most ignorant, and roused the admiration of all. Witness the simultaneous discoveries of the Leyden Phial and the Electric shock. Witness also the astonishing achievement of Franklin, in drawing down a charge of lightning from a thunder-cloud, taking it home with him, and experimenting upon it at his leisure. After such things, Electricity must have taken its place among the most popular and the best known of the sciences. Further, if we look to the practical applications of knowledge, electricity holds in this respect a very high place in fact, and a still higher in legitimate expectation. The Lightning-Conductor, so valuable as a means of preserving life and property; the Electric Telegraph, so widely employed already for the purpose of express; and the Electro-Plating processes, so useful in the arts, and so conducive to the comforts and elegancies of life: these are instances of what electricity has done, and earnests of what it may have yet to do. So that in the practical view electricity takes its place among the most valuable of the sciences. But in conclusion, we cannot see the full importance of this branch of physical science, till we take a liberal and comprehensive view of it, as one section of the great study of nature. And in this respect, the singular importance of electricity appears chiefly in the closeness and variety of those relations that have been discovered between the electric agency and the other forces of nature. It is now clearly established that Electricity is intimately connected with Magnetism, Light, Heat, the chemical forces, and the molecular forces; and that, in fact, we can detect the presence of electricity either as cause or effect in almost every physical action which is accessible to us. By the discovery of such relations the varied phenomena of nature are being drawn together towards a great central theory; and if the hopes of philosophers are to be realized in regard to one science of the natural forces that will embrace all the branches of mechanical and chemical knowledge under a few common principles, there can be no doubt that electricity will be the principal guide to such a science, and will hold the principal place in it. These remarks are sufficient for our present purpose; and we may now attempt a brief statement of the simpler phenomena and laws of electricity.

In treatises upon this subject, the development of an attractive power in amber and other bodies by friction is usually the fact first mentioned; and this not merely because it was the fact first discovered in electricity, but because it is in itself one of the simplest facts, and the most easily brought out in experiment. We can develop the attractive power very easily by means of a rod or tube of common glass. Let the rod be well dried, which may be insured by heating it; and let it then be vigorously rubbed with a dry silk handkerchief. This is all that is necessary for exciting

or electrifying the rod of glass. When the experiment succeeds best, a slight crackling may be heard during the progress of the friction. But even when this effect is not produced, the electric state of the glass may be made manifest by its mechanical actions; for if the excited part of the rod be brought near to any very light bodies, such as bits of paper or barbs of feathers, the bodies will fly through the air from a distance, and attach themselves to the surface of the rod. The attractive power of the glass disappears very soon, but it may be restored by a repetition of the friction. To manifest this and other mechanical actions of electrified bodies in a convenient way, we make use of a simple apparatus, called the Electric Pendulum, which is shown in the adjoined figure. In this form of electroscope, the light body employed is generally a pith ball, one-fifth or one-fourth of an inch in diameter; and it is attached by a thread or fine wire to a fixed support, which is bent into such a form as not to interfere with considerable movements of the ball. We shall suppose that the thread is a film of raw silk, and that the support is of glass. With this simple apparatus we can easily manifest some of the elementary laws of the electric forces. A glass rod is excited by friction, and brought near the ball of the pendulum in the same horizontal plane or nearly so: the force is immediately made evident by the attraction of the ball, and the consequent displacement of the thread from the vertical position. If the excited rod be withdrawn to a greater distance from the pendulum, the thread returns towards its vertical position; showing that the electric attraction diminishes as the distance between the bodies is augmented. If a dry pane of glass be interposed between the excited rod and the ball of the pendulum, there is no change observed in the mechanical action of the rod upon the ball; showing that the electric attraction can be transmitted through solid bodies as well as through air. Further, if the excited rod be brought so near as to attract the pith ball up to contact with itself, we observe that, after contact for an instant, the ball falls away from the rod into its original position, and even beyond it; and if the rod be now made to approach the ball, the thread again moves from the vertical position, but in such a direction as to indicate that the electric force after contact is repulsive. It will be easy to observe in this case that the pith ball itself is now charged or permanently excited; for if the rod be altogether withdrawn, the ball will now act mechanically upon light bodies brought near to it, in precisely the same way as it was itself acted on by the excited rod. We see, therefore, that when the electric affection has been developed in glass by friction, it can be transferred from the glass to another body by mere contact; and it is this fact, as we shall soon find, which explains the change of the electric force from attraction before contact to repulsion after contact. A few additional statements may be here made in regard to the transference of electricity from an excited or charged body to another body. And first it may be observed, that contact between the bodies is not necessary. An excited glass rod will part with more or less of its charge to another body, such as a piece of metal, when it is brought very near it, without actually touching it. In this case the electricity gives indications of its passage by a slight spark between the bodies, accompanied by a snapping sound, phenomena which are identical in their nature with the thunder and lightning of the atmosphere. As electricity may be transferred from an excited glass rod to another body, so it may be transferred from the latter to a third body, either by contact or spark. We are therefore able to conduct a charge from one body to another *through* an intermediate body. Thus, if a long metallic wire be suspended by threads of silk, and if one end of the wire be electrified, a pith ball suspended in contact with the other end of the wire will be immediately repelled by it; showing that the electricity has been conducted along the wire, and that part of it has been transferred to the pith ball. Here, however, we meet with a remarkable class of distinctions among bodies. If a rod of glass, or a rope of silk, had been used instead of the wire in the last experiment, no charge would have been transmitted to the pith ball. Glass and silk are therefore called nonconductors, while the metals and other bodies that act similarly are called conductors. For further information upon this point see the article upon CONDUCTION. The facts now mentioned, with others of the same class, admit of important experimental applications in the *Insulation* of conductors. Generally a conductor is insulated when it is placed in such relations to surrounding matter that a charge bestowed upon the conductor shall be retained by it. Arrangements to this effect are of essential importance in a large class of electrical experiments; and they are very simple, both in principle and practice. We insulate a body by enveloping it upon all sides with nonconducting matter. The atmospheric air is a good nonconductor, and we therefore insulate a conductor when we suspend it in air by a nonconducting thread, or support it in air by a nonconducting rod. But the insulation in such cases will be rendered very imperfect by the presence of moisture to any great extent in the air; for water is a very good conductor, both as a liquid and as a vapour. For details in regard to insulation, and the dissipation of charges from electrified conductors see the article upon INSULATION. We have hitherto considered only one case

Fig. 1.

of electrical excitation, that of glass when rubbed with silk. We may now take up another case equally simple, that of a stick of sealing wax when rubbed with woollen cloth. The two cases taken together will give a simple proof of the fundamental fact, that there are two distinct species of electricity. It has been already stated, that when the insulated ball of an electric pendulum has been attracted up to contact with an excited rod of glass, it receives a charge of electricity and is repelled by the rod, and conducts itself thereafter in the same way as the rod itself does, attracting generally those bodies that the rod would attract, and repelling those that the rod would repel. Excited sealing wax gives precisely the same results. But a new effect is observed when we charge the ball of the pendulum by contact with glass, and bring an excited stick of wax near to it: the ball in fact is attracted by the wax, instead of being repelled as it would be by excited glass. A similarly attractive action is exerted by excited glass upon a ball that has been previously charged by contact with excited wax. The electricities developed in the glass and in the wax must be distinct from each other, since they thus exert contrary actions in similar circumstances, each attracting a body that would be repelled by the other. The one electricity is called the Vitreous or Positive, and the other the Resinous or Negative. In another instructive form of the above experiment, we charge one pair of insulated pith balls from excited glass, and another pair from excited wax, and observe their mutual actions when brought near each other. We find thus, that similarly charged bodies are mutually repelled, while those dissimilarly charged are mutually attracted.—From the law of the mechanical actions now stated, we derive a simple method of determining the species of electricity with which a body is charged in any actual case. Let the insulated ball of an electric pendulum be charged with a *known* electricity, either Vitreous or Resinous; and let the body which we wish to examine be brought up from a distance towards the ball. Then if the body repel the ball, it has a charge similar to that of the ball; and if it attract the ball, it is either dissimilarly charged or not properly charged at all. The former action, that is, the repulsive, may be depended upon in all cases as indicating the existence of a charge upon the body similar to that upon the ball. We say, therefore, if a body repel the ball when vitreously electrified, that the body is also vitreously electrified. Such a statement cannot be understood in its full and proper import without the generalization of our notions in relation to the *two electricities*. We have defined the Vitreous and Resinous electricities, as those which are derived from the excitation of glass and sealing wax by friction with silk and wool respectively; and we suppose that the test-ball of the pendulum is electrified from either of these sources. If we now obtain, by any means, a conductor which repels the ball, we say that this conductor and the ball are similarly electrified, that is, both vitreously, or both resinously. And by this we mean, not merely that the ball is mechanically affected by the new charge in the same way as it would have been affected by a *Vitreous* or *Resinous* charge, according to the original and restricted sense of these terms; but also, that the new charge, when once identified so far, say with what we have called the Vitreous Electricity, by the simple determination of its mechanical action upon the ball of the pendulum, may be now identified with that electricity in all respects. In other terms, every actual charge is identifiable in all its properties, either with what we have called the Vitreous or with what we have called the Resinous electricity. And this, of course, is the only adequate reason for the unqualified extension of the terms Vitreous and Resinous to any other charges besides those which have been obtained by the excitation of glass and sealing wax. The two electricities, as already stated, are called also Positive and Negative, the Negative being the Resinous.—In these introductory remarks, there is only another point that we shall refer to, that is, the effect of the mutual contact of two oppositely electrified conductors. The best way of exhibiting the effect is to allow the two conductors, say two pith balls, to attract one another up to mutual contact. At the instant of contact there is an evident modification of the electric forces. The two bodies fall away from each other; and thereafter they either repel one another, or exert no mutual action at all. The opposite electricities appear to have united and destroyed each other wholly or partially at the time of contact. A similar annihilation of the electric forces may be effected by bringing the two bodies into contact with a third conductor. The conductor opens a path to the two charges, and fulfils the function of a mutual contact of the bodies. These facts, along with those already stated under the head of Conduction, will give an elementary conception of *Electricity in Motion.* Contrasted with other facts, they suggest a simple, though not a perfect division of electric science into two branches, Electro-Statics and Electro-Dynamics. In the former of these branches we study the various phenomena that are presented by *stationary charges* of electricity; and in connection with these phenomena we study also the laws of the development and disappearance of charges, or in other terms, the laws of *excitation* and *discharge.* In Electro-Dynamics, which is now much the more extensive branch of the science, we consider the phenomena of electricity in motion, or the laws of discharge in general, and more especially the laws of continuous discharge or of electric currents; and here also we meet a special and important class of facts in regard to the excitation of electricity. In the present article we

shall take up the principal points of Electrostatics as briefly as possible, referring the reader to various subordinate articles for additional information.

1. Of *Excitation* we have had already two instances, in the friction of glass with silk, and in that of resin with wool or fur. We have detected and compared the charges which are developed in those cases upon the glass and the resin, and we have taken them as the standard representatives of *the two electricities;* but we have not yet examined the electric state of the rubber in either case. The examination may be simply effected by wrapping a silk handkerchief round a non-conducting rod, and then rubbing it with a tube of glass. By this action the glass will be positively electrified, as we know; and if the silk be examined by the action of the electric pendulum, or other similar means, it is found to be also electrified, but negatively. A similar examination of the second case would show, that when resin is negatively electrified by friction, its rubber is also electrified, but positively. These facts are suggestive of a law of excitation, which is found to be universal, that when one of the electricities is evolved by any means, the other is evolved by the same means along with it. Extending now our inquiry upon excitation by friction to other substances besides glass and resin, and their rubbers, we can hardly find any couple of bodies that are incapable of being electrified by a properly managed friction of the one against the other. In the infancy of electric science, many bodies, including all the metals, were understood to be unexcitable by friction, and they were therefore called Anelectrics or Nonelectrics, in distinction from the excitable class, which were called Electrics or Idioelectrics. This was prior to the discovery of the electric conductive powers of bodies—a discovery that completely explained the appearance of unexcitability presented by the metals and some other substances. If a rod of iron, for example, be held in the hand, and rubbed with woollen cloth or any other substance, it will remain perfectly unexcited. But, remembering the high conductive powers of the metals, we explain the absence of charge after friction, in this case, by supposing that the electricity, as soon as it is developed, is conducted away from the metal, through the hand and body of the operator, into the ground. To test the truth of this explanation, we have only to insulate the metal in relation to the hand that sustains it. We may attach it firmly, for instance, to a glass handle, and hold it by the handle. In these circumstances, the metal conducts itself under friction as an Idioelectric, receiving a sensible charge, and retaining it for some time, just as a rod of glass would. By the adoption of precautions similar to the above, in other cases that require it, we find that all bodies, except the gases, appear to be capable of electrization by friction, though in very different degrees. There is one class of cases, however, that presents great difficulty—that is, when the two bodies employed are both good conductors. It is only in special forms of experiment that we can obtain any sensible charge in such cases; and this might have been expected. Still, though we cannot obtain a sensible charge, we have satisfactory evidence otherwise of the development of the electricities by friction. In extending the assertion of excitability, as we have done, to all bodies except the gases, we have included, of course, the liquids. These are excitable, both by mutual friction and by friction against solids. Of the latter case, we have a good instance in Armstrong's Hydro-Electric Machine, in which the *rubber* is water in the form of small drops. The drops are suspended in a rapid current of steam, and are driven along with it through an irregular channel of wood, against the sides and projections of which they are powerfully dashed and rubbed by the force of the steam. By the friction thus accomplished, both the wood and the steam are electrified, and the electricities are easily collected. The Hydro-electric machine appears to be the most powerful instrument of excitation that we yet possess. The simple apparatus represented in the adjacent figure may be usefully employed in illustration of the laws of excitation. The discs are formed of glass, wood, resin, or any other substances, the same or different, and they are both furnished with insulating handles. We can change the nature of either of the rubbed surfaces at pleasure, by covering the face of one of the discs firmly with different kinds of cloth or paper, or by coating it with varnish or other similar substances. In every variety of experiment thus obtained, we find that when, by the mutual friction of the discs, one of them is positively electrified, the other is at the same time negatively electrified. We find also that the two opposite charges, which are simultaneously developed, are in every case equal to each other. This equality we infer from the perfect equality of the mechanical actions of the discs upon the insulated ball of a pendulum, and still more surely from the neutralization of the electric actions of the discs upon all external bodies when the excited faces are placed in mutual contact. We find also that the quantities of the opposite electricities which are developed in any case of excitation, leave no free electricity when they unite in discharge; and hence we derive the important law, which is found to be universal, that the quantities of opposite electricities which are neutralized in discharge are equal to each other.—When two bodies, then, are subjected to mutual friction, they are *oppositely* and *equally* electrified. But, can we certainly foresee which body of a given couple shall be positively electri-

Fig. 2.

fied, and which negatively? This question leads us to a branch of the subject that has been very thoroughly studied. And first, it appears that the determination of a particular electricity—say the positive—to one element rather than the other of a rubbed couple is dependent, to a great extent, upon the *nature of the bodies.* We have had proofs of this fact already, in glass and silk as one couple, and in resin and woollen cloth as another. In these and some other instances it is barely if at all possible, to change the final disposition of the electricities upon the two bodies by any variation of the circumstances of the experiment. If we now form a number of couples, and observe the positive and negative body in each, it will appear that the relation between the elements of a couple, as positive and negative, is still preserved when the bodies are coupled separately with other bodies. It is evident, therefore, that bodies may be tabulated in the order of their positive, or negative tendencies, when subjected to friction; and such tables are accordingly given in the elementary works on Electricity. If any two bodies contained in such a table be rubbed against each other, the one which is farthest on, say in the table, is always negatively electrified. The following table is one that is generally adopted:—Fur of a cat, polished glass, woollen cloth, feathers, wood, paper, silk, gum-lac, rough glass. It is not to be supposed, however, that the facts will constantly come out in accordance with the arrangement of bodies now indicated. Very slight changes in the circumstances of the experiment are often sufficient for the inversion of the electricities on the two bodies after friction. And we have now to notice, therefore, some of the circumstances, besides the nature of the rubbed bodies, which affect the determination of a particular electricity to a particular element of the couple. The *state of the surfaces* of the couple is one of these circumstances. Thus, polished glass is a highly positive body, while rough glass is highly negative. Two ribbons, again, which consist of the same stuff, but which are of different colours—the one white and the other black—are strongly electrified by mutual friction—the black being always negative. These are instances of the influence which the state of the rubbed surface exerts upon the phenomena of excitation, and they are cases of what appears to be a universal law—that, by roughening or blackening the surface of a body, we render the body more highly negative—that is, more susceptible of being negatively electrified by friction against other bodies. The *proportion of the parts rubbed* of the two surfaces is another important element. Two ribbons, for example, are electrified by mutual friction, though they are similar to each other in every respect, when the one is rubbed longitudinally across the other. The latter is always negatively electrified; and, in general, when the frictional action is concentrated more upon one of the surfaces than upon the other, the tendency of the former surface, in virtue of the unequal distribution of the friction, is always negative. Another important circumstance is the relation of the two bodies rubbed in regard to *temperature.* When we subject two perfectly similar bodies to mutual friction symmetrically there is no excitation produced; but the result is different if one of the bodies has been heated before the friction; the bodies, in fact, are now electrified after friction, the heated body being negative. We find, also, as we should expect, that if we take any couple of electrics, and heat the positive body before friction, the amount of electricity now excited by a given amount of friction is less than formerly; the positive body, in fact, has become more negative or less positive by the increase of its temperature, and the couple has been to that extent weakened. And in many cases, where the positive body can stand a sufficient increase of temperature, we are able actually, by changing the temperature, to reverse the relation of the two bodies as positive and negative, and to obtain considerable charges in the other direction. While we restrict ourselves to charges of sensitive intensity, Friction is, practically, the most important cause of excitation; but there are two other causes—pressure and heat—that may not be altogether overlooked. Two bodies may be sensibly electrified by a mere pressure of the one against the other. This is true of the most of bodies, but the phenomena are presented very remarkably by some crystalline and laminated structures. Calcareous spar, arragonite, mica, topaz, and some other bodies, are very sensibly electrified by pressure for an instant between the fingers, and the electricity thus developed is retained by the bodies in sensible intensity for hours, and even sometimes for several days. In the case of excitation by pressure, as in that due to friction, the bodies of any couple have definite and pretty constant relations to each other, as the one positive and the other negative, and these relations are affected in the same way in both cases by an unequal distribution of heat between the bodies, and by a difference of polish in the surfaces. Considering the important part that heat plays in excitation, we might expect that the agency of heat alone would be sufficient, in favourable circumstances, for the development of electricity, and this is actually found to be the case. The electrifying action of heat has been specially studied, as exhibited by the tourmaline—a crystal that we obtain chiefly from Ceylon. When the temperature of a tourmaline is either increasing or diminishing within two definite limits, the crystal is found to be electrified positively at one extremity, and negatively at the other. When the heating or the cooling is effected rapidly, the intensity of the electricities evolved is very considerable, as appears by the attraction of light bodies from a distance to the surface of the crystal. If a tourmaline has been retained for some time at any constant tem-

perature, it gives no electric signs; but when we add or subtract heat, the electric poles begin immediately to appear, and remain active till we reach either of the limiting temperatures. It is a remarkable fact that the disposition of poles for increasing temperatures is contrary to that for diminishing temperatures; so that, if the tourmaline is regularly heated to a certain point, and then regularly cooled, the electricities first disappear at the extreme temperature, or shortly after it, and finally reappear at the ends of the crystal, having changed places. There are many crystals which are now known to possess electric properties analogous to those of the tourmaline; but we will not enter further into the subject at present.—In electrical experiment it is very frequently necessary to obtain powerful charges, and the best means to this effect have been conjoined in that important instrument, the Electric Machine. The most common form of the instrument is that represented in the article ELECTRICAL MACHINE. It consists of several parts, which we may describe in connection with their separate functions of excitation, collection, and insulation. The means of excitation are—first, a glass plate which is turned round a fixed horizontal axis by means of a handle attached to it; and secondly, a pair of rubbers which are kept in contact with the two surfaces of the plate above its centre, and a pair similarly disposed below the centre. The rubbers consist of leathern surfaces coated with an amalgam, and both pairs are attached to the vertical support of the axle of the plate, and, by means of interposed cushions, are pressed constantly against the moving surfaces of the glass. When the plate is well dried, and the amalgam of the rubbers fresh, the turning of the plate will powerfully excite both the glass and the cushions, the former positively. The next part of the apparatus is that by which we collect and accumulate the electricity thus excited. Two conducting rods, in the form of hollow metallic cylinders, are fixed in positions parallel to the axle, and with their ends near the extremities of the horizontal diameter of the plate. From each of these ends proceed two metallic arms towards the centre of the plate, on opposite sides of the plate, and close to its surface. The surfaces of the arms which face the plate are furnished with points, which favour the desired effect, as we shall afterwards see, although they are not necessary to it. It is evident that when we work the machine by turning the plate, the part of the glass surface which has been excited by friction against either of the rubbers comes immediately under the influence of the metallic arms, and parts with its electricity to the conducting cylinders. In the machine represented in the figure, the electricity excited upon the rubber is conducted away into the ground, as its accumulation on the rubbers would put a stop to the process of excitation; but in other forms of the machine the two electricities are both collected. Further, the two cylinders are connected by a rod of metal at their ends, remote from the plate, and the one piece of conducting matter thus formed is called the Prime Conductor of the machine. The third part of the apparatus is for insulation, or for effecting the retention of the charge upon the Prime Conductor. This function is fulfilled by the insulating power of the glass rods that support the Prime Conductor. The apparatus now described is a very important instrument of science. It enables us to exhibit the properties of stationary charges in a much more convenient and clearer manner than we could do by the elementary methods of excitation, with glass rods or sticks of wax; but it is especially valuable in experiments upon the communication of electricity, and upon the various effects of discharge.

2. We may now attend to the phenomena of *Induction*. In the adjacent figure, B represents an *insulated* metallic cylinder, which has several pairs of pith balls, attached by conducting threads to different points of its surface. The

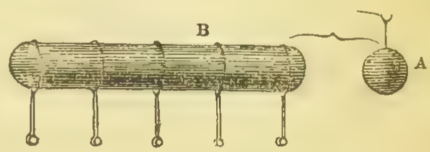


Fig. 3.

cylinder being uncharged, let an electrified ball, A, be brought up towards one of its extremities. We know that if the distance between B and A be sufficiently small, the electricity of A will be partly transferred to B, by discharge through the air. But, before the distance has been thus far diminished, there is a sensible development of electricity in B. The pith balls of several pairs in the above arrangement are seen to diverge from one another, and the divergence increases as A approaches—showing that the pith balls and the cylinder to which they are attached are electrified, without discharge, by the mere neighbourhood of A. In this simple experiment it is observed, further, that the electricity is developed in greatest intensity at the extremities of B, and that there is no sensible development about the middle of the cylinder. If the electrified ball, A, be now withdrawn, the pith balls collapse, and the cylinder loses all traces of charge. Here then we have a method of developing electricity which is in remarkable contrast with the two methods already noticed—excitation and communication; for the charges developed by the latter methods in insulated conductors are, in every case, persistent or self-sustaining, when the originating agency has been suspended. This remarkable property of the charges *induced* in insulated conductors is explained, as we might have expected, by the fact that the inductive action of A develops in B, not merely one electricity, but *equal* quantities of *the two* electricities. This fact is easily proved by observation. Let

an insulated pith ball be charged by contact with A, and let it be brought near the balls of B when they are diverging under the inductive action of A. It will be seen that the pith ball attracts those balls of B which are at the extremity next to A, and repels those which are farthest from A. The cylinder is therefore oppositely electrified at its two extremities by the influence of A, and the charge similar to that upon A is at the extremity remote from A. The two electricities appear also, as already stated, to diminish in intensity as we proceed from the extremities of B towards the middle, where there are no electric signs at all. These facts are equally well proved without the attachment of pith balls or other electroscopes to the cylinder. By means of the Proof Plane—a very small metallic disc attached to a long insulating handle—we can draw small charges from different parts of the surface of B by contact, and then we can test the species and intensity of the charge upon the Proof Plane by means of an electroscope. It can be proved further, by direct observation, that the opposite charges developed in B are equal to each other; but this appears to be sufficiently proved by the fact, that when A is withdrawn, the two charges on B are mutually destroyed. Suppose now, that while B is under the inductive influence of A, we uninsulate B, or connect it with the ground, the charge upon B that is remote from A, or similar to that upon A, immediately disappears; and if, after this, we first insulate B, and then withdraw the body A, we find that B is charged, and with an electricity opposite to that upon A. This result may be explained by a reference to our original arrangement of pith balls in connection with B; for when B is uninsulated, it is connected with the conducting mass of the earth, just as one of the pith balls is connected in the original arrangement with the mass B. And, more particularly, the relation between B, when uninsulated, and the immensely greater conducting body of the earth, is precisely similar to the relation between B when insulated, and the smaller conducting mass of a pith ball attached to the end of B that is *next* to A; so that, as the pith ball will be charged with one electricity in the latter case, the cylinder B will be similarly charged in the former, and will retain its charge when it is detached from the earth by insulation.—Hitherto the charge upon A has been considered as merely acting inductively upon B; but this is not an adequate view of the electric relations of the two bodies. The inducing charge is itself affected inductively by the electricity which it develops and sustains in B. It is affected, not in the way of loss or gain of electricity upon the whole, but in the way of distribution. If we examine different points of the surface of A by means of a proof plane and an electrometer, we find that the intensity of the electricity on the parts of the surface which are next to B is much greater than on the parts remote; so that, as the charge of A attracts the opposite electricity in B towards A, the latter attracts the opposite charge of A towards B. If we now consider the case of two insulated conductors, which are both electrified, and brought near to each other, we can express their mutual inductive actions in these terms—that the opposite electricities are mutually attracted, and the similar electricities mutually repelled. Referring to our original arrangement of the ball A and the cylinder B, suppose the two bodies to be insulated and similarly electrified. Then, if A be brought up towards one extremity of B, the pith balls at that extremity will collapse, and those at the other extremity will have their divergence increased—showing that the electricity of A repels the similar electricity of the other body. If B had been oppositely electrified, its charge would have been attracted or determined with greater intensity towards the parts of the surface adjacent to A. And in both cases, and indeed in all cases, the action of the charge of B upon A is precisely similar to that of the charge of A upon B. It may be now easily understood that an insulated conductor which is electrified, say positively, may have a distribution of negative electricity induced over a certain portion of its surface, and this whether the inducing charge be positive or negative. Suppose that the inducing charge is positive, and much greater than the charge upon the body examined, then the first effect of induction will be a diminution of the intensity upon the parts of the body adjacent to the inducing charge, with a corresponding increase of intensity on the parts remote. As the bodies approximate this effect increases, until the positive electricity is entirely repelled from one part of the surface; and upon this part the negative electricity will be elicited if the distance between the bodies be still diminished.—The precise consideration of the laws of induction cannot be here attempted; but it may be stated that, in every attainable system of inductive actions, the two electricities appear to conduct themselves as two material fluids, which are self-repulsive and mutually attractive with forces, varying inversely as the square of the distance, and directly as the product of the fluid masses. And we include in this statement the case of uncharged conductors by assuming, as the phenomena compel us to assume, that every insulated conductor possesses an unlimited fund of the two electricities; and that these electricities, though mutually neutralized and inactive, are yet subject to the attractive and repulsive actions of induction, just as free charges are. Suppose now that we have a system of conductors placed near one another, and that one or more are electrified. The whole system will immediately fall into a particular electric state. Each conductor will have a stationary distribution of one or both electricities upon its surface—a distribution sustained by the inductive actions

of all the charges, original and induced, throughout the system. And it is evident, from the above statement of the law of induction, that the question of the distribution of electricity in such a system is just a question of equilibrium of fluids, which are subjected to definite forces, attractive and repulsive. This forms one of the principal subjects of investigation in the Mathematical Theory of Electricity. The adjacent diagram represents a simple experimental arrangement which we owe to Faraday, and which is of great value for the simplicity and clearness with which it brings out some important principles of inductive action. The vessel A is an insulated pewter ice-pail, about a foot in height. It is connected by a wire with a delicate electrometer, E. C is a metallic ball, insulated by a silk thread three or four feet in length. If the ball, C, be charged and let down into A, as in the figure, the electroscope, E, will at once diverge with a charge similar to that upon C. This effect the reader will understand from what precedes. If C be positively charged, the negative electricity will be attracted to the interior surface of A, and the positive will be repelled to the exterior surface, and still more strongly to the remote body, E. As the ball enters A, the divergence of the electroscope will increase till it is several inches below the edge of the vessel, and it will then remain constant for all lower positions of C. From this we infer, that in those lower positions the entire inductive action of the ball's charge is exerted upon the inner surface of A, while, in the higher positions, part of that action is spent upon internal conductors. Of course, we here speak of the inductive action as it is *sensibly manifested* by its effect upon the electroscope. It may now be understood that a charged conductor, placed in the middle of a large room, and apart from other conductors, will yet perform its inductive function as completely as if it were in the circumstances of the ball, C; for the place of A will be supplied by the walls of the room, which will therefore be electrified with a charge opposite to that of the conductor, and equal to it, as we shall see immediately. When the ball C is suspended in the vessel at a sufficient distance to prevent discharge, the vessel A, and its attached electroscope, have no excess of either electricity. This may be proved by withdrawing C from the vessel, when E will collapse perfectly if the apparatus is in good order. When C is suspended in the vessel, the divergence of E is therefore a measure of the charge induced upon the inner surface of A; and if C touch the interior surface of A, and communicate its charge to the vessel and the electroscope, the divergence of E will be a measure of the original charge of the ball.

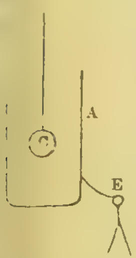


Fig. 4.

But the divergence of E is absolutely unchanged by internal discharge between C and A; and hence Faraday infers, from direct experiment, that "the electricity *induced* by C and the electricity *in* C are accurately equal in amount and power." For developments upon this subject, into which we cannot enter, the reader is referred to *Faraday's Experimental Researches*, vol. ii., page 280. The most important additions that have been made to Electro-Statics in recent times are contained in the discoveries of Faraday, and in his peculiar views, with regard to the nature of inductive action. For information upon these points, consult the article on INDUCTION; see also ELECTROSCOPE and ELECTROPHORUS, for simple applications of the principles above stated.

3. We may now consider the laws of the *Electric Forces.* We have seen that similarly charged bodies repel one another, while those oppositely charged attract; and also, that excited electrics attract uncharged bodies which are conductors, a case that is evidently reducible by the principles of induction to that of charged bodies. It should be observed in passing, that these attractions and repulsions are quite distinct, in fact, from what we have called the attractions and repulsions of the electricities in our statement of the laws of induction. The experimental investigation of the laws of the electric forces was a work of great difficulty; and Coulomb had the high merit of commencing and completing it. The chief instrumental means that he employed in this work was the Balance of Torsion, a fine apparatus of his own invention, whose general form and principle we need not here describe, as a separate article has been devoted to it. When the Torsion Balance is formed especially for electric experiment, the lever or needle suspended by the torsion thread is a slight rod of nonconducting matter, bearing at one end a small and light-conducting sphere. The other electrified body, or the carrier, is in the simplest case a similar conducting sphere; and it is attached to a long insulating stem by which we introduce it through an opening in the glass case into the zero-position of the first sphere. To determine, by means of this apparatus, the relation between the electric force and the mutual distance of the bodies, the carrying ball is charged and introduced into its proper position, when it shares its charge with the other ball and repels it to a certain distance. At this distance the force of torsion is evidently equal to the electric repulsion. By causing the force of torsion to take various definite values, as we can do at pleasure, we are able to keep the moveable ball in equilibrium at various measurable distances from the fixed ball; and this while the two charges are sensibly constant. And then by comparing the different forces of torsion with the corresponding distances of the balls, we find that the mutual repulsion of the balls is inversely proportional to the square

of their distance. The attractive force of the two balls when oppositely charged is found to obey the same law. This case is managed by first charging the moveable ball and removing it by the torsion force to some distance from its zero-position, and then charging the carrier with the opposite electricity and putting it into position in the apparatus, and observing the simultaneous values of the torsion force and the distance as in the former case. For determining, further, the relation between the electric force and the quantities of electricity upon the balls, we must be able first of all to obtain isolated charges which are quantitatively related to one another in a perfectly definite way; for otherwise we can institute no comparison between different forces and the corresponding charges. To obtain charges thus related to one another, Coulomb adopted a very simple method. He took two perfectly similar conducting balls, of which one was charged and the other not: he placed these in contact with one another; and he took it as an axiom that after separation the balls were equally charged, and each therefore charged with half the quantity of electricity originally possessed by the one. In this way several submultiples of a given charge could be obtained and experimentally compared in the Torsion Balance. Suppose that the two balls in the balance were charged and in position, and that the torsion force and the distance of the balls were noted; that then the carrying ball were withdrawn and put in contact with a perfectly similar and equal uncharged ball, and finally restored to its position in the apparatus. The force of torsion that would be now required to preserve the original distance of the balls would be less than formerly, the repulsion of the balls having diminished. The charge upon the carrier might be again halved by a similar process: the charge upon the moveable ball might be also subdivided at pleasure; and the subdivisions of both charges might be conducted in any order, separately or together. In this way, and by a great number and variety of experiments, Coulomb proved, that the attractive and repulsive forces of the balls vary as the product of their charges. The laws above stated may be simply expressed in one mathematical formula. Let P and Q be the charges upon two small spheres, D the distance between the centres of the spheres, and F the attractive or repulsive force: then $F = \frac{P\,Q}{D^2}$. We can even include in this formula the directions of the forces as attractive or repulsive, if we agree to affect all vitreous charges with the sign $+$, and all resinous charges with the sign $-$; for on this understanding, all positive values of F derived from the formula will indicate repulsions, and all negative values attractions Such are the simple laws which regulate the attractions and repulsions of small charged balls, or of *charged points*, if we may so speak. Resting as these laws do upon rigorous experimental grounds, they have been universally adopted as first principles by the cultivators of the mathematical theory. They have been questioned, however, in recent times, especially by Sir W. Snow Harris, who has attempted an experimental revision of this and other elementary points in the theory of electricity. The results which he obtained were certainly such as to throw some doubt at first upon the recognized laws of the electric forces; but it is now understood by those familiar with the subject, that the results of Coulomb's labours have been absolutely untouched, if not rather confirmed. The reader who desires full information upon these points may consult a paper by Professor W. Thomson, in the *Philosophical Magazine* for July, 1854. It will be evident that, within certain limits, the method of experimental inquiry described above might be still followed, though the electrified bodies were of various forms and magnitudes. But there is another method, which we owe still to Coulomb, and which may be followed in cases that would be unmanageable by the method of torsion. If one of the electrified bodies, for example, were very much greater than the other, the smaller body might be attached to the end of a light nonconducting lever and made to oscillate round a fixed point in front of the large body. The electric attraction in this case would supply the place of gravitation in the common pendulum; and the values of the attraction for different distances of the bodies would be proportional, according to the law of the pendulum, to the squares of the number of oscillations performed in a given time at those distances. By varying the distance, and observing the number of oscillations performed in a given time, we could therefore approximate to the law of the force in any given case. It should be observed that this method of oscillations was also employed by Coulomb for the direct confirmation of those fundamental results already described as obtained by the method of torsion. As the first result then of these experimental researches of Coulomb, we have obtained a general mathematical formula of great simplicity, which expresses the entire law of the electric force in the fundamental case of two charged points. And in more complex cases many accurate numerical results have been obtained, which are of use not for suggesting a general mathematical expression of the law of force in the case of given conductors, but for testing and confirming the results of the simple mathematical theory founded upon the equation $F = \frac{P\,Q}{D^2}$, and upon the laws of induction and distribution. For some development upon these points see a subsequent article concerning IMAGES, ELECTRICAL. We should not leave this subject without referring to the dissipation of electricity from the charged bodies in the course of experiment. In the researches

'oulomb and of those who have followed in his teps, this was found to be a most important eleıent. It is in fact very evident, that in virtue f the dissipation the charges experimented on ·ill be continually changing in quantity, and ıis independently of the will of the operator. Vithout entering into details, it may be stated ıat Coulomb certainly and completely overcame ıis difficulty in his researches, partly by preenting the dissipation as far as possible, and ıereafter by taking fully into account the effects ' the dissipation that remained.

4. We may now consider briefly the *Distribuon* of electricity upon conductors. It is a funamental proposition in this branch of the subject, ıat a charge bestowed upon an insulated conıctor is distributed wholly over its external urface, and not in any degree throughout its ıass or over interior surfaces. We exclude, of ourse, the case of an interior surface which ıvelops a charged and insulated body, a case ell exemplified in Faraday's arrangement of the e pail A and the enclosed carrier C. In proof ' the proposition that we have now stated some riking experiments might be adduced. The pparatus employed in one of these is represented the adjacent cut. It consists of an insulated conducting sphere, and two hemispherical conducting shells which are attached to insulating handles. The shells fit accurately to the surface of the sphere, so as to envelop the

Fig. 5.

here, in contact with it and with one another. When we charge the sphere and then cover it with ıe hemispherical shells, we find that the surface ' the latter, that is, the external surface of the hole conducting mass, is now electrified. And ill more, when the shells are withdrawn by their sulating handles, we find that they are sepaıtely electrified, and that the sphere is perfectly scharged, presenting no trace of its original ectricity. This is a clear proof that the charge as distributed wholly over the external surface. ı further illustration take a hollow metallic linder, such as the ice pail A in the diagram on induction. Let such a conductor be charged, ıd let the electric state of its various parts be sted by the proof plane. It will be found that ıe proof plane receives a sensible charge from ıy point of the external surface, but none whatrer from a point of the interior surface; a clear oof that the electricity is distributed in accordıce with the proposition. Like results may be otained with perforated shells of conducting atter, whatever be the forms of the shells, proded they have closed or nearly closed surfaces. he figure represents one of the curious arrangeents which Faraday has made in illustration of ıis subject. A conical bag of muslin has its outh bounded by a metallic ring which is supported by an insulating stem. A silk thread attached to the apex of the cone passes through the ring. When we charge the ring and the bag and employ the proof plane, we find no trace of electricity upon the interior surface of the muslin, while we receive considerable charges from its outer surface. By drawing the thread we turn the cone inside out, and we find that the entire charge has changed surfaces, the inner surface being still destitute of electricity. As a last illustration: if we dig a small and sharp cavity in a spherical or other conductor and then charge the conductor, we find that the proof plane takes no charge whatever from the bottom or the sides of the cavity. From these and other instances we conclude, that when a charge is bestowed upon any connected body of conducting matter, it is distributed wholly over the external surface, and not in any degree through the interior of the mass, or even upon *surfaces* that are properly internal or enveloped. In all further questions therefore about the distribution of charges, we can have respect only to the electric intensities at the various points of conducting surfaces. The general question of distribution has been so far stated and answered in our remarks upon induction. The general statement there made may be now repeated and explained, and so far proved: that in all inductive and distributive actions the two electricities conduct themselves as if they were self-repulsive and mutually attractive with forces varying inversely as the square of the distance and directly as the product of the quantities of electricity. It must be noticed that in this statement we extend the assertion of a self-repulsive force both to the mutual actions of similar charges and to the action of every single charge upon itself. When conductors of certain forms are insulated and electrified, the simplest examination is sufficient to show that the charges are not uniformly distributed. If the conductor, for example, be a long cylinder with hemispherical ends, the attraction of light bodies will be evidently twice as powerful at the ends as at the middle of the cylinder; a result that indicates a greater accumulation of electricity at the ends than at the middle. But a much more accurate method of investigation than this is required for the true determination of the distribution in any case; and Coulomb supplied such a method by means of the Torsion Balance and the Proof Plane. When the Proof Plane is put in contact with any part of an electrified conducting surface, it becomes electrically an element of the surface; and when it is removed it retains a charge equal to the quantity of electricity resident upon the element that was covered by the proof plane. By successively discharging the proof plane, and applying it to different points of the given sur-

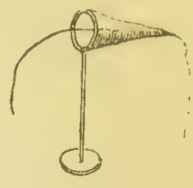


Fig. 6.

face, we obtain a series of small charges which have this definite relation to one another, that they are the quantities of electricity resident upon *equal* superficial elements in the given distribution. Now the natural measure of the electric intensity at any point of a charged surface is just the quantity of electricity resident upon a given and very small superficial element situated at the point in question; so that by means of the proof plane we can exhibit the various electric intensities of any particular distribution in the form of isolated charges that can be quantitatively compared with one another apart from all disturbing actions by means of the Torsion Balance. This is the simple theory of the employment of the Proof Plane in the experimental study of the laws of distribution. The theory of the employment of the Torsion Balance in this investigation is equally simple. The proof plane, when charged by contact with the surface, is introduced to the position of the fixed ball of the balance, where it attracts the moveable ball and gives it, as we otherwise know, a constant proportion of its own charge, and then repels it to a certain distance. By giving a proper value to the force of torsion we can keep the balls at any assigned distance from each other. When the torsion force which corresponds to the assigned distance is noted, the proof plane is withdrawn and applied to another point of the surface, and the moveable ball is perfectly discharged. The experiment is repeated in the same form for the new point of application of the proof plane, and the force of torsion for the same distance as before is determined. The process may be repeated for any other points of the charged surface. Now since the distances between the two small charged bodies in the balance are the same in the several experiments, the torsion forces which have been determined are simply proportional to the products of the charges upon the two bodies. But these products are proportional to the squares of the original charges upon the proof plane, since the latter body gives a constant proportion of its charge to the ball. And the charges drawn from the surface by the proof plane are therefore proportional to the square roots of the observed forces of torsion. By the simple calculations thus indicated, we can determine accurately the original charges of the proof plane and the electric intensities which are measured by them. It should be noticed, however, that the results thus obtained require correction, because of the gradual dissipation of the given charge. It is evident that two successive contacts of the proof plane cannot give the *simultaneous* intensities at the points examined, for the charge of the conductor diminishes in some degree during the interval between the contacts. The serious difficulty thus arising was met successfully by Coulomb in his researches upon this subject. He determined the laws of the dissipation, and corrected his results accordingly. The mode of correction that he generally adopted in his experiments is worthy of special notice for its rigour and simplicity. The electric intensity at one point of the surface was determined by a single observation with the proof plane and the balance. After the shortest possible interval of time, the intensity at a second point of the surface was similarly determined; and after an equal interval the intensity at the first point was again observed. The second result and the arithmetical mean of the first and last results were then taken as the accurately simultaneous intensities at the points examined. When greater accuracy was desired, the mean of a greater number of results was taken; for in this way the unavoidable errors of separate experiments had their influence upon the final result eliminated or greatly diminished. By the method of investigation that we have now described, every actual case of distribution may be subjected to rigorous examination. A few of the simpler facts observed in regard to distribution may be now stated. Upon an insulated conducting sphere the distribution of a charge is uniform, subject always, however, to unlimited variations in virtue of inductive actions from without. Upon a prolate spheroid the intensity of the electricity at the poles exceeds that at the equator. The converse holds in the case of an oblate spheroid. In general, the distribution of a charge upon a conducting surface approaches to or departs from uniformity according as the form of the charged surface approaches to or departs from perfect roundness, and the electricity is always distributed with greatest intensity at the extremities of the greatest dimension. One case of distribution may be here quoted from Delarive's work on Electricity. An insulated cylinder, 2 inches in diameter and 33 in length, terminated by hemispherical surfaces, was charged and examined by Coulomb's method. The values of the intensity at the middle of the cylinder, at ten inches from the extremity, and at the extremity, were found to be as 100, 125, and 230. The electric intensity at any point of a charged surface depends, as we have seen, upon the general form of the surface and upon the position of the point in the surface. It depends also, and essentially, upon the value of the curvature in the immediate neighbourhood of the point in question. The greater the curvature at any point the greater is the intensity if other things are equal. And so powerful is this influence of curvature, that if a conducting surface be formed with very sharp and prominent edges, angles, or points, it will be impossible to retain any charge upon the body; for the intensities at the salient parts of the surface rise to the limiting value of discharge through the air, and this while there is hardly a sensible charge upon the body as a whole. The two influences that we have mentioned, that of the distant parts of the surface, and that of the curvature at the

oint in question, may be so combined that the ne shall annul the other. An angle, or point, or example, however sharp and prominent, will ave no discharging influence if it be so enveloped y other distant parts of the body as to be conderably internal in relation to the entire conucting surface. These and other facts lead us) the conclusion, that every insulated charge ossesses a property which is equivalent to a rce of self-repulsion or expansion. For a view the evidence by which our former statement f the quantitative law of this force is supported, e may refer the reader to the article entitled ORCES, ELECTRIC. Some important results are tained when two insulated conductors are aced in contact with each other, so as to prent one conducting surface. When a surface of is kind is charged, it is observed that the law distribution depends only on the *forms* and *agnitudes* of the two bodies, provided they are th good conductors. Whether the two bodies nsist of the same or of different kinds of conicting matter, and whether both bodies be solid the one be solid and the other hollow even to e utmost thinness actually attainable, the disibution is sensibly the same for the same forms id magnitudes of the two bodies. And hence e draw the important inference, that there is) electrical relation among the different kinds matter analogous to that of specific capacity r heat. One of the most interesting cases of stribution is that upon two spheres which are contact with each other. This case was expementally studied by Coulomb with great accucy. It was afterwards taken up by Poisson his developments of the mathematical theory; id the several quantitative results that he tained coincided so closely with those of Coumb as to confirm very powerfully both the first inciples of the theory and the difficult matheatical processes founded upon them.

5. Before proceeding to the subject of *discharge*, we may attend briefly to the laws of *sguised electricity*, and to the principles of the ethods usually adopted for the accumulation of arges. We have seen that when two insulated id electrified conductors are put in contact with ch other, they share the given charge between em in a proportion depending upon the forms id magnitudes of the surfaces. In these cirimstances, the portion of the total charge that received by one of the conducting surfaces ineases as the magnitude of that surface increases latively to that of the other surface, though t exactly in the same ratio. In much the me way, if an electric machine, or electrophos, or other active source of electricity, be put connection with an insulated conductor, it ill give a charge to it very nearly proportional the magnitude of the conducting surface. ence the simplest way of obtaining increasing arges from a given source, say from a given achine kept at a given rate of work, is to increase, or to virtually increase, the surface of the Prime Conductor. This was done by Volta, in his invention of the Secondary Conductors, which are nothing more than a system of well insulated conducting cylinders, which have their extremities rounded to prevent the rapid dissipation of the charge. When these cylinders are placed in connection with the prime conductor and with one another, they present a large conducting surface to the source, and receive a charge proportionally great. This method of accumulation is subject to grave inconveniences in practice, as it often requires an unmanageable extent of conducting surface, and is always attended with a rapid loss of the charge by dissipation in the air. The other method, now to be described, is free to a great extent from such inconveniences, besides possessing the advantage of an actual accumulation independent of an increase of surface. We know that a charge of any desired intensity may be developed and sustained in an *uninsulated* conductor. All that is necessary to this effect is, that another conductor be insulated and charged and brought into the neighbourhood of the former, but not into conducting connection with it. The opposite electricity of the uninsulated conductor is at once attracted, and the other electricity is repelled into the ground. The distribution of the inducing charge is also affected, a greater portion of the charge being now determined to that part of the surface that is adjacent to the uninsulated body. Suppose now that the charged conductor is in permanent connection with the prime conductor of the machine, and electrified to the utmost intensity attainable. When the uninsulated body is brought up towards its surface, the distribution upon the prime conductor and the connected body will be immediately changed according to the above law; and if the machine be still wrought the diminished intensities of the distribution will be restored to their former values, as we know by direct observation. Here then we have an instance of accumulation of electricity upon the prime conductor and connected bodies by simple inductive action. The amount of accumulation depends upon the distance between the two conductors, augmenting rapidly as this distance diminishes, and subject indeed to no limit of its increase except such as arises from the nature of the interposed nonconductor or dielectric, which admits of discharge through its mass when the intensities of the charges have reached certain values. To make the limiting intensity as great as possible, we employ glass or other solid nonconductors as the medium between the two conductors. The adjacent cut represents the glass plate condenser, one of the simplest arrangements that can be adduced in illustration of this subject. Two squares of metallic foil are pasted on the opposite sides of a square of glass, so as to leave a considerable edge, v, of the glass uncovered all round. To the upper parts of the two sheets of

foil two pith ball pendulums are attached, as represented in the figure. We charge the apparatus by uninsulating one of the metallic sheets, B, and connecting the other, A, with the prime conductor of the machine. As the machine is wrought, the pendulum *a* rises, while *b* remains at rest in contact with B; and the condenser is fully charged when *a* has reached a stationary position. If now B be first insulated, and the connection between A and the prime conductor be then broken, the apparatus will remain charged, and the positions of the two pendulums will be unchanged. We infer from the divergence of *a* that the metallic sheet with which it is connected is electrified, but this electricity is not to be confounded with the charges that have been *accumulated* in the apparatus. The divergence of *a* is due in fact to a free charge disposed upon the external surface of A, a charge that A receives independently of the inductive action of B. Besides this free charge there are two opposite and powerful charges disposed upon the internal surfaces of A and B. This is an inevitable inference from the simplest principles of induction; but it may be otherwise proved in the present case by direct observation. When the apparatus has been charged and insulated, so that *a* is repelled and *b* not; let the knuckle be presented to the sheet A—a spark passes; the pendulum *a* falls at once to permanent contact with A, and *b* at the same time ascends. By drawing a charge from A, we have thus liberated a part of the charge of B from its internal surface, where the entire charge was originally confined by the inductive action of the more powerful charge upon A. The pendulum *a* remains in contact with the metallic sheet, because the entire charge of A is confined to the internal surface by the action of the now more powerful charge of B. Charges which are thus confined, so as to produce no inductive effects at external and accessible points are said to be *disguised* or *dissimulated*. By presenting the knuckle now to B, we partially discharge it; the pendulum *b* falls and *a* rises as before. By again discharging A, the pendulum *b* rises a second time; so that we have thus liberated a second part of the entire charge originally accumulated on the inner surface of B. In this way we may draw dozens of successive charges from the two plates before the condenser is discharged; a clear proof of the accumulating power of the arrangement, if we observe how much electricity is withdrawn, for example, by the first partial discharge of A. The arrangement possesses the further advantage of retaining the accumulated charges for a great length of time. The facts now stated find practical application in two of the most useful electrical instruments, the Condenser and the Leyden Jar. Both instruments are founded upon the principles of disguised electricity; but they are of different forms, which are suited separately to the two ends usually aimed at in the accumulation of charges. The use of the Condenser is, to condense weak charges, or to increase the intensity of electricity derived from a given source by drawing upon the quantity, so as to make the charge measurable, or at least sensible. For a view of the form and theory of this instrument see the article on CONDENSER. The use of the Leyden Jar, on the other hand, is to accumulate great quantities of electricity for the purpose of powerful discharge. For a description of the instrument, we may refer to the article on BATTERY, ELECTRIC; but it may be here stated that the Leyden Jar is virtually the same as the *glass plate condenser* already described; only the glass dielectric is, in the one case, a square plate, and in the other, the wall of a wide mouthed phial. The mouth of the phial is closed by a nonconducting plug, and this is pierced by a conducting rod, which is terminated without by a knob and within by a chain or wire that touches the internal metallic coating of the Jar. The instrument is charged by connecting the knob with the Prime Conductor, and the outer coating with the ground, or *vice versa*. The greatest part of the electricities thus accumulated may be discharged instantaneously through any sufficiently conducting path established between the knob and the outer coating. And we can increase in any degree the power of discharge obtainable in this way from a given machine, by connecting a number of Jars together, giving them virtually one outer coating and one inner coating, an arrangement which is called an Electric or Leyden Battery.

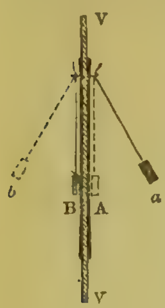


Fig. 7.

6. We may now consider very briefly some of the simpler phenomena of *Discharge*. These phenomena are presented on a grand and terrific scale in the effects of lightning, such as the fusion of metals, the splintering and rending of trees, the shattering of the hardest rocks, and the destruction of animal and vegetable life; effects that can be exhibited on a very formidable, though a much smaller scale, by the Leyden Battery. Generally, when a discharge passes through matter of an inferior conducting power, it produces a very sensible mechanical disturbance among its particles. For the exhibition of these effects we make use of the Leyden Jar or Battery and the Universal Discharger or other similar instrument. The universal discharger is a simple arrangement of insulated metallic rods or wires, by which we are enabled to establish an *interrupted* conducting path between the coatings of the battery. In the interruption of the circuit we place the body that we wish to examine; and it is supported in that position by a nonconducting table placed under it. When the interruption of the circuit is short enough, and the charge

of the battery strong enough, the discharge traverses the body, and produces the desired effect as soon as the end of the circuit is brought up to the knob of the battery. In this way pieces of wood and stone and other nonconductors are violently fractured and projected out of the circuit. To this class of actions we must refer the occasional fracture of the walls of Leyden Jars by discharge between the coatings through the glass when the jars are too strongly charged. The disruptive action of discharge can be confined in certain cases to one part of the nonconducting mass that is placed in the circuit. The famous experiment of the pierced card is an instance. The interruption of the circuit in this experiment is terminated by two metallic points; and the body traversed by the discharge is a card of stiff paper or pasteboard, which is placed obliquely in the interruption of the circuit, so that its opposite surfaces are very near the discharging points. When the discharge passes, the card is pierced, but not rent or otherwise injured. The small perforation is generally close to the negative point, and presents a burr upon each surface of the paper as if it had been formed by two threads drawn violently through the card in opposite directions. By discharge a small hole may be pierced even through a sheet of glass without the glass being cracked or otherwise injured. All that is necessary for this effect is, that the points of the discharger be placed opposite to each other, and in contact with the surfaces of the glass, and that one of the points be enveloped with a drop of oil. Such facts clearly prove that when discharge traverses a nonconducting mass it produces great mechanical commotions among its particles. In the case of liquids and gases we cannot expect such permanent results as we have found in the case of solids; but here also we have clear proofs of powerful mechanical action. Thus, if the two ends of the interruption of the circuit be made to dip near each other under the surface of water, the discharge projects the water out of its path with great force; and in another arrangement, where the water is contained in a well closed phial, the commotion produced by a powerful discharge bursts the phial, and disperses the glass and water in all directions. In air and the gases the commotions produced are as evident as in solids and liquids, producing sounds and expansions of various intensities according to the strength of the discharge. The suddenness and intensity of the expansion are well illustrated in the *electric mortar*, a small and strong phial of glass which is perforated by two wires, and has its mouth closed by a ball of cork. When a discharge passes along the wires and through the air or gas enclosed in the phial, the sudden expansion projects the cork with considerable force. Kinnersley's thermometer, represented in the adjacent cut, consists of a large upright tube of glass, closed at both ends, and opening at the bottom into a small tube which is bent upwards and open at the top. The ends of the large tube are traversed by conducting rods which are terminated within by knobs. The tubes contain water, as represented in the figure, to a level considerably above the mouth of the small tube: so that the air surrounding the knobs is confined to an air-tight chamber. Accordingly, when a discharge traverses the rods and the interjacent air, the water is launched upwards in the smaller tube with a sudden and powerful force, and immediately falls again, but not accurately to its original level. The apparatus gives in this way a very elegant proof of the sudden and transient expansion produced by discharge through air; but it proves also the existence of a more permanent though smaller expansion, by the permanent rise of the level in the smaller tube. The latter expansion is due to heat which is developed in the air by discharge, and which is lost not suddenly but by degrees.

Fig. 8.

The calorific, illuminating, and inflaming actions of discharge are exhibited in a great variety of beautiful experiments that we have not space here to notice. The appearance of the electric spark and its inflaming power are known to all. Of the heating power of discharge we have had an instance in the experiment with Kinnersley's Thermometer. More marked effects of this kind are produced by the passage of considerable charges through attenuated masses of metal, such as thin leaves and fine wires. It requires no considerable charge to raise a fine iron wire to a red heat. With stronger discharges the metal is instantaneously fused into drops, or even dispersed in vapour. In this way a wire of metal which is immersed in a vessel of cold water can be melted into drops in an instant. The last effect of discharge that we shall notice is the *Physiological*, or the electric shock, which rises in intensity with the strength of the discharge, from a slight nervous commotion to the instant destruction of life. When we touch the outer coating of a charged phial with one hand, and bring the other up to the knob, the discharge of the electricities is effected through the body; and a peculiar sensation is felt, which, to be understood must be experienced. It might perhaps be described as a sudden and violent swelling of the muscles of the arms, in its gentler forms. With weak charges the shock is felt in the hands only; with stronger charges it is felt in the arms. When it is felt painfully in the chest, a stronger charge would be dangerous. The circuit may be formed as well by a number of persons with hands joined as by one; and in this case the two free hands at the ends of the circuit are brought up respectively to the outer coating and the knob. The shock is felt at the same instant by all the members of the circuit. There is a famous experiment of the French philosophers upon record, in which a whole regi-

ment of grenadiers was overthrown at one blow by discharge from a Leyden Jar. The shock rises in intensity, as we have said, with the strength of discharge. It requires no very powerful charges to kill mice, birds, rabbits, and other small animals; and in many experiments, discharges are employed that would be sufficient for the destruction of any animal. The state of the animal frame after death by electric discharge or by lightning has been made the subject of diligent investigation; but no determination has been arrived at in regard to the organ or set of organs especially injured. But there can be little doubt that death by electric discharge is due chiefly if not wholly to an excessive shock in the nervous system. For additional information upon the laws of discharge see the article on that subject.—Upon the theories of electricity we cannot here dwell at any length. We shall merely state their fundamental assumptions. According to *Franklin's Theory*, the phenomena of electricity are due to the special properties and actions of a subtle kind of matter called the Electric Fluid. This fluid is self-repulsive, and attracts the common kinds of matter, and is reactively attracted. When a body is in a natural or electrically unexcited state, the attractive force of the matter is balanced by the self-repulsive force of the electric fluid, and the body is said to be *saturated* with electricity. When a body possesses more or less of the electric fluid than corresponds to the state of saturation, it is excited or charged, *positively* or by excess in the state of vitreous charge, and *negatively* in the state of resinous charge. These assumptions give apparently simple explanations of some of the elementary facts, especially of the electric forces, the simpler facts of induction, and the laws of charge. According to the theory of Dufay and Symner, there are two electric fluids, the Vitreous and the Resinous. Each fluid is self-repulsive, and each attracts the other. The theory of a single fluid and that of two fluids appears to be of much the same value, as the facts explained by the one are generally capable of being equally well explained by the other. The former theory, however, is liable to the special objection that it necessarily infers a mutual force of repulsion among the common kinds of matter. For a brief statement of Faraday's theory see INDUCTION.

Electricity: *Applications of.* Electricity is no longer an abstract or unapplied science: it has become an available source of power to the *Mechanician*, the *Clock-Maker*, the *Engineer* civil and military, the *Physicist*, the *Chemist*, the *Observer*, the *Bronze Gilder*, &c., and, in short, to all persons interested in the practical work of advancing civilization through the instrumentality of science. It is but a very slight account that we can give in this dictionary of its various applications: what room we can spare is taken advantage of under articles ELECTRIC LIGHT, ELECTRO-MAGNETIC MACHINE, ELECTROTYPE, OBSERVATION, REGISTRATION, TELEGRAPH, and THUNDER-ROD. We expressly warn our readers that only a brief notice of the efficiency of this very valuable agent can be promised under any of the heads now mentioned.

Electricity; Correlation of, Mechanical Equivalent of. In the second Dissertation prefixed to this volume, as well as in articles FORCE, CORRELATION AND CONSERVATION OF, the grand principle now lying at the foundation of all physics, has been illustrated and enforced. The special question at present is, whether this correlation holds in the case of Electricity; whether other forces, as they seem to disappear, may assume its form; whether, when it seems to disappear, it only takes on some other mode of energy; and, finally, whether, under every variety of circumstance, it has a definite, invariable, and ascertainable mechanical equivalent? As to the general fact of correlation, it has long been established as belonging to the electric force. Friction or mechanical effort evolves it; and, in its turn, it can realize important mechanical effects. Heat evolves it, and the force so produced evolves heat. Electricity calls into existence potent magnetic polarities; and the magnet produces powerful electric currents in a coil of wire. The current loosens chemical affinities, and sometimes compels bodies to combine; while those changes in affinities, that constitute the source of energy in the pile or the galvanic battery, give rise to electric energies which are well nigh irresistible—energies that dissolve the closest and firmest atomic unions, and which in the telegraph are going far to annihilate both space and time. The main question, however, remains,—amid all such transformations do we recognize the disappearance and production of forces *that are sensibly equivalent:* unless equivalence is established, there is no proof of identity. It is only just to state that science owes the starting, and by far the greater part of the elucidation of this inquiry, to the sagacity, the accuracy, and unwearied patience of Mr. Joule, sometimes working apart, and at other times in connection with his friend and fellow-discoverer, Professor William Thomson. We can give here but the briefest outline of the results arrived at by Mr. Joule and a few other physicists.—In a recent interesting letter to the editor of the *Philosophical Magazine*, Mr. Grove announces a curious experiment, inaugurating apparently a novel mode of investigating this subject. In substance it seems this: Take a Leyden jar fully charged, and therefore on the point of discharge by the spark: bring into its neighbourhood a Cuthbertson's electrometer—it will repel the ball of the electrometer, and so exercise a mechanical effect. That moment the tension diminishes, and the jar is no longer on the point of discharge. A definite quantity of electricity has thus disappeared, and we have instead a visible and definite mechanical effect. In the

same manner, whenever an electric current causes the attraction of two pieces of an apparatus, if these two pieces, yielding to this attraction, are set in motion, a suitable and definite diminution of the intensity of the current is observed: the inverse also holds true.—But the phenomena may be better illustrated by special detail. It is well known that when the current passes through a thin wire, heat is generated. Now that heat may be measured by surrounding the wire with water. The increase of the temperature of the water is the measure of the heat. But by surrounding a piece of soft iron by this wire in a coil, magnetic force is produced; and it can be proved that the quantity of electricity which, when transformed into heat by the resistance of the medium, is enough to raise the temperature of a pound of water by 1 degree, produces an attractive magnetic force by which a weight of 772 pounds may be raised a foot high—the exact mechanical equivalent of that definite amount of heat. Again, when the conductor is cut in two, and its two ends dipped into a vessel of water, electricity manifests itself as an analytic force, and decomposes the water into its aerial elements, oxygen and hydrogen. These may be re-united, and on their re-union they produce heat. Now, accurate investigation has established, that an electric current of known strength, which, when transmuted by a conductor into heat, is enough to heat one pound of water 1°, liberates—when employed on the decomposition of water—an amount of hydrogen by which, when it is burnt, exactly one pound of water can be raised from 0° to 1°.—Illustrations of this sort could be brought without end, showing that electricity, like the other energies, merely transforms itself,—that with regard to it also, the maxim holds, "Force never dies."—It is highly important to know, in reference to this class of inquiries, that alike through Joule's experiments and theoretical deductions from chemical principles, the electromotive force of any battery can be laid down with accuracy in thermal units, so that, beginning at its origin, we can trace the action and history of the entire force until it evolves its final effect. See on this subject an instructive paper by Professor W. Thomson, in *Philosophical Magazine* for December, 1856.

Electricity, Atmospheric. 1. It may be premised, to avoid circumlocution in this article, that every body in communication with the earth by means of matter possessing electric conductivity enough to prevent its electric potential * from differing sensibly from that of the earth, will be called part of the earth. Moist stone, and rock of all kinds, and all vegetable and animal bodies, in their natural conditions, except in circumstances of extraordinary dryness, possess, either superficially or throughout their substance, the requisite conductivity to fulfil that condition. On the other hand, various natural minerals and artificial compounds, such as glass,—various vegetable gums, such as India-rubber, gutta percha, rosin,—and various animal products, such as silk and gossamer fibre,—when either in a very dry natural or in an artificially dried atmosphere, resist electrical conduction so strongly that they may support a body, or otherwise form a material communication between it and the earth, and yet allow it to remain charged with electricity to a potential sensibly differing from the earth's, for fractions of a second, for minutes, for hours, for days, or even for years, without any fresh excitation or continued source of electricity. Again, air, whether dry or saturated with vapour of water, and probably all gases and vapours, unless ruptured by too strong an electromotive force, are very thoroughly destitute of conductivity—that is to say, are very perfectly endowed with the property of resisting the tendency of electricity to pass and establish equality of potential between two bodies not otherwise materially connected.—2. Hence, when "the surface of the earth" is spoken of, the surface separating the solids and liquids of the earth from the air will be meant; and when the more qualified expression "outer surface of the earth" is used, inner surfaces of vesicles, or the surfaces bounding completely enclosed spaces of air, must be understood to be excluded. Thus, the surface of a mountain peak; the surface of a cave, up to the inmost recesses of the most intricate passages; the surface of a tunnel; the surface of the sea, or of a lake or river; all the surface of a sheet of unbroken water in such a fall as that of Niagara; the surface of blades of grass and flowers, and of soil below; in a wood, the surface of soil, and of trunks and leaves of trees; the surface of any animal resting on the earth; the outside of the roof of a house; the whole inside surface of a room with an open window; all belong to the outer surface of the earth.—3. On the other hand, the moon, meteoric stones, birds or insects flying, leaves or fruit falling, seed wafted through the air, spray breaking away from a cascade or from waves of the sea, the liquid particles of a cloud or a fog, present surfaces not belonging to the earth, and between which and the earth's surface differences of potential, and lines of electric force, may and generally do exist. 4. The whole surface of the earth, as defined above (§ 2), is at every moment electrified in every part, with the exception of neutral lines dividing portions which are negatively (resinously) from portions which are positively (vitreously) electrified. The negatively electrified portions are of very much greater extent, at all times, than those positively electrified;

* Two conducting bodies are said to be of the same electric potential when, if put in conducting communication with the two electrodes of an electrometer, no electric effect is produced. When, on the other hand, the electrometer shows an effect, the amount of this effect measures the difference of potentials between the two bodies thus tested. Difference of potentials is also called electromotive force.

and there may be times when the whole surface is negatively electrified, because in all localities in which electrical observations have been hitherto made, with possibly one remarkable exception,* the earth's surface is always found negative, day and night, during fair weather, and only occasionally positive in broken weather, or during an actual fall of rain in the immediate neighbourhood, if not exactly on the place of observation. If, then, at any one time there chances to be fair weather over the whole earth, it may be presumed that the whole outer surface of the earth is then negatively electrified, unless, judging from the possible exception above alluded to, we are still to expect positive electrification in some extreme positions.—5. As yet nothing is known regarding the electrification of air itself, or of clouds or other matter suspended in the air, except what can be inferred (see below, § 6) from the electrification of the earth's surface, and its variations, with which alone, as Peltier has remarked, the observations of "atmospheric electricity" hitherto published have dealt (see below, §§ 17–19). It is impossible, in the nature of things, to investigate the bodily electrification of a non-conductor by any observation whatever of electric action without it,† or in any way whatever, except by something equivalent to a determination of the magnitude and direction of the resultant force at every point of its mass.‡ Towards this thorough investigation of the distribution of electricity within a non-conducting mass, it may be remarked, that a determination of the normal component of the force all round a closed surface is just sufficient to show the aggregate quantity of electricity possessed by all the matter situated within it.§ Hence observation in positions all round a mass of air is necessary for determining the quantity of electricity which it contains; and, therefore, the balloon must be put in requisition if knowledge of the distribution of electricity through the atmosphere is to be sought for.—6. Without leaving the earth, however, although we cannot thoroughly investigate the electrification of the air, we can make important inferences about it from observations of the electric density over the earth's surface, by a principle of judging which may be thus explained:—If the earth were simply an electrified body, placed in a perfectly insulating medium of indefinite extent, and not sensibly influenced by any other electrified matter, or by reflex influence from any conductor or dielectric in its vicinity, its electricity would be distributed over its surface according to a perfectly definite law, depending solely on the form of the surface, and deducible by a sufficiently powerful mathematical analysis from sufficiently perfect data of "geometry" (in the primitive sense of the term), or of what, in more modern language, is called geodesy. If the surface of the earth were truly spherical, this law would be simply uniformity. A truly elliptic oblateness of the earth would give, instead of uniformity, a distribution of electric density, in simple proportion to the perpendicular distance between a tangent (that is, horizontal) plane through any point and the earth's centre; according to which the electric density at the equator would be greatest, and would exceed that at either pole, where it would be least, by $\frac{1}{305}$, a difference which, for the present, we may disregard.—7. The whole amount of electricity over the surface of any great region of mountainous country, or of forest land, or of soil and vegetation of any kind, or of streets and houses in a town, or of rough sea, would be very approximately the same as that on an area of unruffled ocean, equal to the "reduced" area of the irregular surface; but the distribution of the electricity over hill and valley, over the leaves and trunks of trees, and the surfaces of plants generally, and on the soil beneath them, over

* At Guajara station, on the Peak of Teneriffe. "During the whole period of observation, by day and night, the electricity was moderate in quantity, and always resinous. This was during the period of N. E. trade wind, and within its influence, though above its clouds." [Professor Piazzi Smyth's Account of the Teneriffe Astronomical Experiment, *Philosophical Transactions*, 1858, and separate publication ordered by the Lords of the Admiralty.] The "electricity" here referred to was that acquired by an insulated conductor carrying a burning match in the air at some distance from the earth. If it were really negative, the earth's electrification at the place must have been positive; but the test as to quality may have been deceptive, owing to the highly insulating condition of both outer and inner surfaces of the glass shade inclosing the gold leaves, and to the circumstance of the testing piece of rubbed sealing wax having been applied possibly too near the gold leaves, instead of beside a remote part of the insulated rod. Professor Smyth assures the writer, that he considers the electrical experiment as not sufficiently complete or confirmed to allow any conclusion to be built on it, and regards it rather as an indication of the importance of making electrical observations with better apparatus, and more available time for using it, than the first Teneriffe scientific expedition afforded.

† According to Green's remarkable theorems, triply rediscovered by Gauss, Chasles, and the writer of this article, all different distributions of electricity within a solid, which produce the same potential at its surface, produce the same force at every point without it, and the problem of finding a distribution of electricity within the interior to produce a given distribution of potential at the surface, is indeterminate.

‡ Let X, Y, Z, be the components of the resultant force on a unit of electricity, if placed at any point x, y, z, in a mass of air or other non-conductor; and let ρ denote the electrical density of the substance, that is to say, the quantity of electricity per unit of bulk actually possessed by the air in the neighbourhood of this point. Then, by a well known proposition of the mathematical theory of attraction, we have

$$\rho = \frac{1}{4\pi}\left(\frac{dX}{dx} + \frac{dY}{dy} + \frac{dZ}{dz}\right).$$

§ Let N be the normal component of the force at any point of a closed surface, ds an element of the surface, $\int$ the sign of integration for the whole surface, and Q the whole quantity of electricity within it. Then, by a well known theorem of Green's, rediscovered as alluded to in a preceding note, we have

$$Q = \frac{1}{4\pi}\int N\,ds.$$

the roofs, perpendicular walls, and overhanging or overshaded surfaces of buildings, and the surfaces of streets and enclosed courts between them, and over the hollows and crests of waves in a stormy sea, would be extremely irregular, with, in general, greater electric density on the more prominent and convex portions of surfaces, and less on the more covered and concave—quite insensible, indeed, in any such position as the interior of a cave, the soil below trees in a forest—even where considerable angular openings of sky are presented, or the roof or floor of a tunnel, or covered chamber, even although open to a considerable angle of sky.—8. If thus a perfect electro-geodesy gave a "reduced" electric density equal over the whole earth, we might infer that the electrification of the earth is not influenced by any electricity in the air. According to what has been stated above, there might in that case be either no electricity in the air, from the earth's atmosphere to the remotest star, and (according to Faraday's views) the lines of electric force rising from the earth might terminate in the surfaces of the moon, meteoric stones, sun, planets, and stars; or there might be, at any distance considerably exceeding the height of the highest mountain, a uniformly electrified stratum of equal quantity and opposite kind to the earth's, balancing through all the exterior space the force due to the terrestrial electricity, and limiting the manifestations of electric force to the atmosphere within it; or there might be any of the infinite variety of distributions of electricity in space round the earth, by which the electric density at the earth's surface would be uninfluenced.—9. But, in reality, the electric density varies greatly, even in serene weather, over the earth's surface at any one time, as we may infer from (1.) the facts (established for Europe, and probably true in all the temperate zones of both hemispheres), that in any one place the electric density of the surface observed during serene weather is much greater in winter than in summer, and that it varies according to something of a regular periodicity with the hours of the day and night; and (2.) the consideration that there is often serene weather of day and night, and of summer and winter, at one and the same time, in different temperate portions of the earth. We may, therefore, consider it as quite established, that even in serene weather, the electrification of the earth's surface is largely influenced by external electrified matter. Although we cannot (§ 5) discover the exact locality and distribution of this influencing electricity from its effects at the earth's surface alone, yet it is possible, from the character of the distribution of the terrestrial electric density as influenced by it, to assign a superior limit to its height.* If at any one instant the electric density reduced to the sea level were distributed according to a simple "harmonic" law, or, more generally, according to a certain definite character of non-abruptness of variation easily specified in mathematical language,† the external influencing electricity might be at any distance, however great, for all we could discover by observations near the earth's surface. But, little as we know yet regarding the diurnal law of electric variation in serene weather, it is, we may say with almost perfect certainty, not such as could give at any instant a distribution over the whole earth possessing any such gradual character as that referred to; and, therefore, we may, in all probability, from the character of the diurnal variation itself, say that its electric origin is not at a distance of many radii from the surface. On the other hand, when we consider that in temperate regions the velocity with which the earth's surface is carried round in its diurnal course is from 500 to 900 miles per hour, we see clearly that any law of diurnal electric variation, established on observations even so frequent as once every hour, could not possibly fix the locality of the origin to within 100 miles of the surface; and as we have as yet nothing to go upon in the way of published observations more frequent than three or four times a day, towards establishing either the existence or the character of the diurnal law, we cannot consider it as proved by observation that the influencing electricity which produces it is even as near as the 50 or 100 miles limit which is commonly (but in the opinion of the writer of this article, most unreasonably) assigned as an end to the earth's atmosphere.—10. The great suddenness of the electric variations during broken weather, and their close correspondence with beginnings, changes, and cessations of rain, hail, or snow, compel us (by a *common sense* estimate founded on an unconscious application of the mathematical law stated in the footnotes to the preceding § 9) to believe that their origin agrees in position with that of the showers, and to give it a "local habitation" and a name—Thunder-cloud.—11. The writer of this article has observed extremely rapid variations of terrestrial electrification during perfectly serene weather. Thus, in a calm summer night, with an unvarying cloudless sky overhead, and not the faintest ap-

* If at any instant the co-efficients of the series of "Laplace's functions," expressing the terrestrial electric density reduced to the sea level, converged ultimately with less rapidity than the geometrical series $1, \frac{1}{m}, \frac{1}{m^2}, \ldots$ we might be sure that there is electricity in the air at some distance from the centre of the earth, not exceeding m times the radius of the earth's surface. For the principles on which this assertion is founded, see a short article, entitled "Note on Certain Points in the Theory of Heat," *Cambridge Mathematical Journal*, November, 1843.

† For instance, if in simple proportion to the cosine of the angular distance from any point of the earth's surface, or more generally, if expressible by any finite number of "Laplace's functions," or still more generally, if expressible by a series of "Laplace's functions," with co-efficients converging ultimately more rapidly than any geometrical series.

pearance of auroral light to be seen, he has, in a temporary electric observatory in the Island of Arran, found large variations (as much as from a certain degree to double and back) in the course of a minute of time. The influencing electricity by which these variations were produced, cannot possibly (unless on the extremely improbable hypothesis of their being due to highly electrified extra-terrestrial matter moving very rapidly with reference to the earth) have been very far removed from the earth's surface. It is not impossible, and we have as yet nothing to make it decidedly improbable, that they were due to fluctuations up and down of aërial strata, perhaps those of the great atmospheric currents, in high regions of the atmosphere. Judging, however, from still more recent observations referred to below (§ 14), we may think it more probable that these remarkable variations in the observed electric force were due chiefly to positively or negatively electrified masses moving along within a few miles of the locality of observation.—12. Returning to the subject of the distribution of electricity over the earth's surface at any instant, we may remark, that if over an area of several miles in diameter, of perfectly level, bare country, or of sea, the electrical density is sensibly uniform, we could not, without going up in a balloon, and observing the electric force at points in the air above, form any judgment whatever as to the distance from the earth at which the influencing electricity is situated. If, on the other hand, we find a very sensible variation in the electric density between two points of a piece of level, open country, or at sea, not many miles apart, we may infer as quite certain that there is influencing electricity not many miles up in the air, and not uniformly distributed in level strata. Nothing can be easier than to make this trial—only to observe simultaneously with similar instruments, similarly placed, at two neighbouring stations, in a suitable locality—and most interesting and important results are to be derived from it, as soon as arrangements can be made for continuing the requisite observations day and night, during various vicissitudes of weather, especially during a time of perfect serenity.—13. Corresponding statements apply to a mountainous country, with this modification, that a very varied, instead of a uniform distribution of electric density, is, in such a locality, as explained above in § 7, the natural consequence of freedom from the disturbing influence of near electrified masses of air or cloud. The problem of accurately determining, from purely geometric data (§ 6), this undisturbed distribution over even the smoothest hillside, would infinitely transcend human mathematical power, although an approximate solution may be readily given for any piece of country over the whole of which both the inclination and the ratio of the height above the general level to the radius of curvature of the surface are small. For a rugged mountainous country, the most perfect geometric data, and the most strenuous mathematical efforts, could scarcely lead us towards an approximate estimate of the inequalities of electric density which different localities must present without any disturbance from near electrified atmosphere. Hence, in a mountainous country—unless we find electricity strong in some locality where from the configuration of the surface, we correctly judge it ought to be weak if undisturbed, or weak where it ought to be strong, or unless, at least, we find some very decided deviation from any such amount of difference between two stations as, without being able to make a precise calculation, we can estimate for the difference due to figure—we cannot judge as to the influence of aërial electrification from simultaneous absolute determinations at any one instant alone. But of one thing we may be sure, that although the absolute amounts of the electrification at any two stations not far apart may differ largely, they must remain in an absolutely constant proportion to one another, if there is no electrified air or cloud near.—14. Hence, if we find observations made simultaneously by two electrometers in neighbouring positions, in a mountainous country, to bear always the same mutual proportion, we may not be able to draw any inference as to electrified air; but if, on the contrary, we find their proportion varying, we may be perfectly certain that there are varying electrified masses of air or cloud not far off. A first application of this test is described in the following extract from the Proceedings of the Literary and Philosophical Society of Manchester for October 18, 1859:—

"The following extract of a letter received from Professor W. Thomson, F.R.S., Glasgow, Honorary Member of the Society, &c., was read by Dr. Joule:—

"'I have a very simple "domestic" apparatus by which I can observe atmospheric electricity in an easy way. It consists merely of an insulated can of water set on a table or window sill *inside*, and discharging by a small pipe through a fine nozzle two or three feet from the wall. With only about ten inches head of water and a discharge so slow as to give no trouble in replenishing the can with water, the atmospheric effect is collected so quickly that any difference of potentials between the insulated conductor and the air at the place where the stream from the nozzle breaks into drops is done away with at the rate of five per cent. per half second, or even faster. Hence a very moderate degree of insulation is sensibly as good as perfect, so far as observing the atmospheric effect is concerned. It is easy, by my plan of drying the atmosphere round the insulating stems by means of pumice stone moistened with sulphuric acid, to insure a degree of insulation in all weathers, by which there need not be more than five per cent. per hour lost by it from the atmospheric apparatus at any time. A little attention

to keep the outer part of the conductor clear of spider lines is necessary. The apparatus I employed at Invercloy stood on a table beside a window on the second floor, which was kept open about an inch to let the discharging tube project out without coming in contact with the frame. The nozzle was only about two feet and a-half

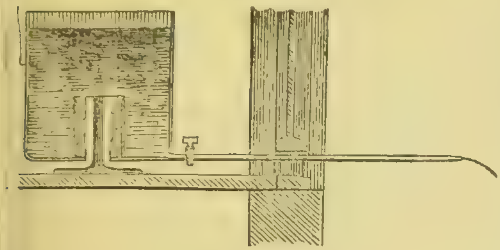


Fig. 1.

from the wall, and nearly on a level with the window sill. The divided ring electrometer stood on the table beside it, and acted in a very satisfactory way (as I had supplied it with a Leyden phial, consisting of a common thin white glass shade, which insulated remarkably well, instead of the German glass jar—the second of the kind which I had tried, and which would not hold its

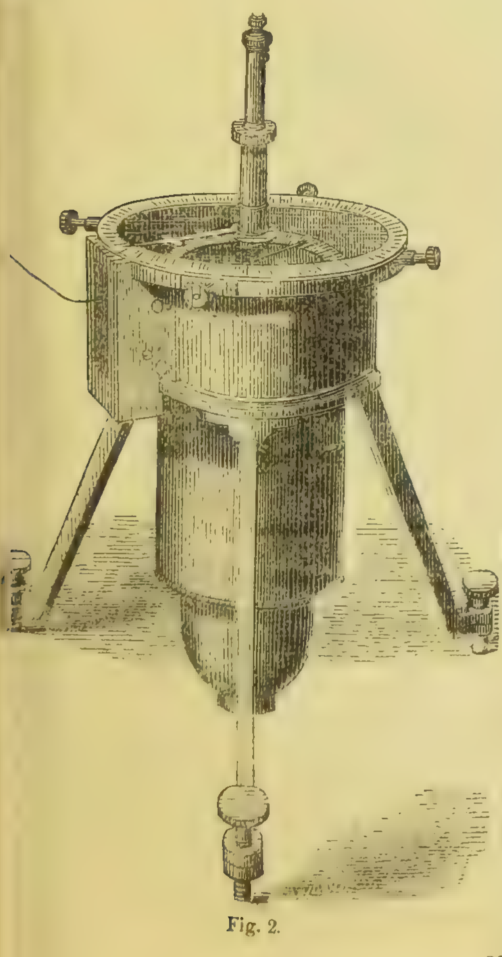


Fig. 2.

charge for half a day).—I found from $13\frac{1}{4}^\circ$ to 14° of torsion required to bring the index to zero, when urged aside by the electromotive force of ten zinc-copper water cells. The Leyden phial held so well, that the sensibility of the electrometer, measured in that way, did not fall more than from $13\frac{1}{2}^\circ$ to $13\frac{1}{4}^\circ$ in three days. The atmospheric effect ranged from 30° to above 420° during the four days which I had to test it; that is to say, the electromotive force per foot of air, measured horizontally from the side of the house, was from 9 to above 126 zinc-copper water cells. The weather was almost perfectly settled, either calm, or with slight east wind, and in general an easterly haze in the air. The electrometer twice within half an hour went above 420°, there being at the time a fresh temporary breeze from the east. What I had previously observed regarding the effect of east wind was amply confirmed. Invariably the electrometer showed very high positive in fine weather, before and during east wind. It generally rose very much shortly before a slight puff of wind from that quarter, and continued high till the breeze would begin to abate. I never once observed the electrometer going up unusually high during fair weather without east wind following immediately One evening in August I did not perceive the east wind at all, when warned by the electrometer to expect it; but I took the precaution of bringing my boat up to a safe part of the beach, and immediately found by waves coming in that the wind must be blowing a short distance out at sea, although it did not get so far as the shore.—I made a slight commencement of the *electro-geodesy* which I pointed out as desirable at the British Association, and in the course of two days, namely, October 10th and 11th, got some very decided results. Macfarlane, and one of my former laboratory and Agamemnon assistants, Russell, came down to Arran for that purpose. Mr. Russell and I went up Goatfell on the 10th instant, with the portable electrometer (see Fig. 3), and made observations, while Mr. Macfarlane remained at Invercloy, constantly observing and recording the indications of the house electrometer. On the 11th instant the same process was continued, to observe simultaneously at the house and at one or other of several stations on the way up Goatfell. I have not yet reduced all the observations; but I see enough to leave no doubt whatever but that cloudless masses of air at no great distance from the earth, certainly not more than a mile or two, influence the electrometer largely by electricity which they carry. This I conclude because I find no constancy in the relation between the simultaneous electrometric indications at the different stations. Between the house and the nearest station the relative variation was least. Between the house and a station about half way up Goatfell, at a distance estimated at two miles and a-half in a right line, the number expressing the ratio varied from

about 113 to 360 in the course of about three hours. On two different mornings the ratio of the house to a station about sixty yards distant on the road beside the sea was 97 and 96 respectively. On the afternoon of the 11th instant, during a fresh temporary breeze of east wind, blowing up a little spray as far as the road station, most of which would fall short of the house, the ratio was 108 in favour of the house electrometer—both standing at the time very high—the house about 350°. I have little doubt but that this was owing to the negative electricity carried by the spray from the sea, which would diminish relatively the indications of the road electrometer.'"—15. The electrometers referred to in the preceding extract were on two different plans. The first, or "divided ring electrometer," consists of—(1), A ring of metal divided into sectors, of which some—one or more—are insulated and connected with the conductor to be electrically tested, and the remainder connected with the earth. (2), An index of metal supported by a glass fibre, or a wire, stretched in the line of the axis of the ring, and capable of having its fixed end turned through angles measured by a circle and pointer. (3), A Leyden phial, with its insulated coating electrically connected with the index. (4), A case to protect the index from currents of air, and to keep an artificially dried atmosphere round the insulating supports—glazed to allow the index to be seen from without, but with the inner surface of the glass screened (electrically) by wire cloth, perforated metal, or tinfoil, to do away with irregular reflections on the index. In the instrument represented in the drawing (No. 2) above, the ring is divided into only two parts, which are equal, and separated by a space of air about $\frac{1}{20}$ of an inch. Each of these half rings is supported on two glass pillars; and by means of screws acting on a foot which bears these pillars, it is adjusted and fixed in its proper position. The index is of thin sheet aluminium, and projects in only one direction from the glass fibre bearing it. A stiff vertical wire, rigidly connected with it, nearly in the prolongation of the fibre, bears a counterpoise considerably below the level of the index, and heavy enough to keep the index horizontal. A thin platinum wire hooked to the lower end of this vertical wire, dips in sulphuric acid in the bottom of the Leyden phial. The Leyden phial is charged either positively or negatively; and is found to retain its charge for months, losing, however, gradually, at some slow rate, less generally than one per cent. per day of its amount. The index is thus, when the instrument is in use, kept in a state of charge corresponding to the potential of the inside coating of the phial. When one of the half rings is connected with the earth, and a charge of electricity communicated to the other, the index moves from or towards the latter, according as the charge communicated to it is of the same or the opposite kind to that of the index. This instrument, as an electroscope, possesses extreme sensibility—much greater than that of any other hitherto constructed; and by the aid of the torsion arrangement, it may be made to give accurate metrical results. There are some difficulties in the use of it, especially as regards the comparison of the indications obtained with different degrees of electrification of the index, and the reduction of the results to absolute measure, hitherto obviated only by a daily application of Delmann's method of reference to a zinc-copper water battery, which Delmann himself applies once for all, to one of his electrometers (unless his glass fibre breaks, when he must make a fresh determination of the sensibility of the instrument with its new fibre). The high sensibility of

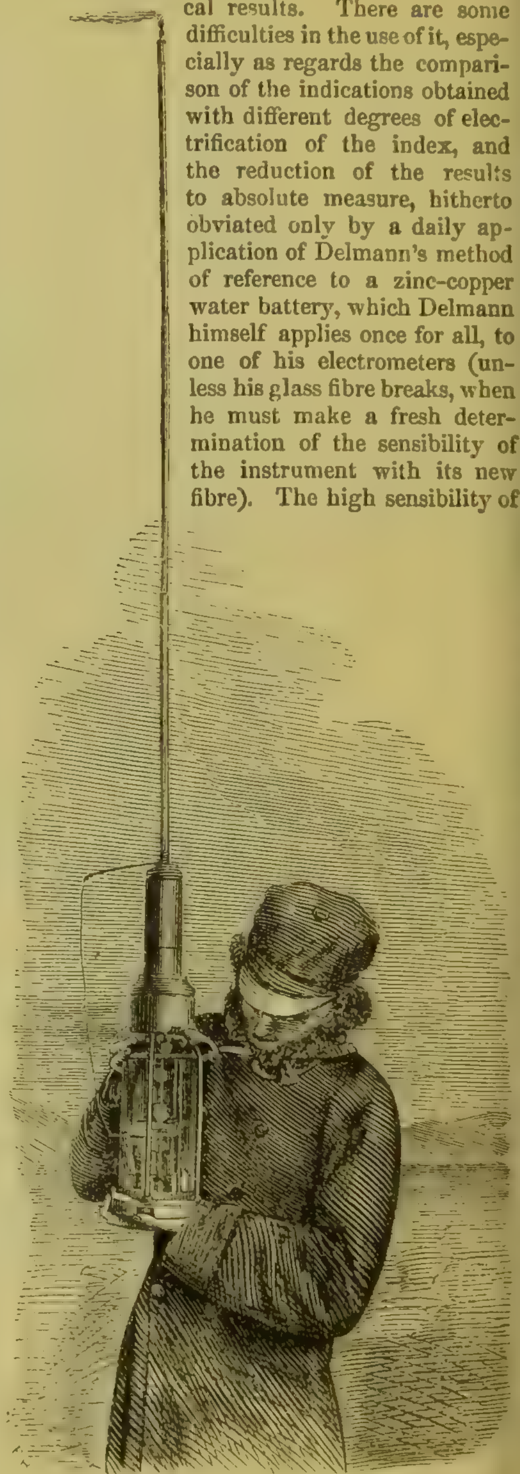


Fig. 3.—Portable Atmospheric Electrometer.

the divided ring electrometer renders this test really very easy, as not more than from ten to

.wenty cells are required; and a comparison with ı few good cells of Daniell's may be made by its ıid, to ascertain the absolute value and the con- :tancy of the water cells. The difficulty thus met s altogether done away with in another kind of lectrometer, also "heterostatic," of which only me has yet been constructed—the electrometer of the portable apparatus shown in the third lrawing. In it the index is attached at right ıngles to the middle of a fine platinum wire, irmly stretched between the inside coatings f two Leyden phials, and consists simply of a ery light bar of aluminium, extending equally on the two sides of the supporting wire. It is epelled by two short bars of metal, fixed on the wo sides of the top of a metal tube, which is sup- orted by the inside coating of the lower phial, nd has the fine wire in its axis. A conductor of uitable shape, bearing an electrode, to connect vith the body to be tested, insulated inside the ase of the instrument, in the neighbourhood of he index, and when electrified in the same way, r the contrary way, to the inside coatings of the eyden phials, causes, by its influence, the re- ulsion between the index and the fixed bars to e diminished or increased. The upper Leyden hial is moveable about a fixed axis, through ngles measured by a pointer and circle, and ıus the amount of torsion, in one-half of the earing wire, required to bring the index to a onstant position, in any case, is measured. The quare root of the number of degrees of torsion ıeasures the difference of potentials between the onductor tested and the inner coating of the eyden phial. In using the instrument, the onductor tested is first put in connection with ıe earth, and the torsion required to bring the ıdex to its fixed position is read off. This is alled the zero, or earth reading. The tested onductor is then electrified, and the torsion ading taken. In the atmospheric application, ıis is called the air reading. The excess— ositive or negative—of its square root, above ıat of the zero reading, measures the electro- ıotive force between the earth and the point of r tested. This result, when positive shows itreous, when negative resinous potential in the r; if the index is resinous. By the aid of arlow's table of square roots, the indications of ıe instrument may thus be reduced to definite ıeasure of potential, almost as quickly as they ın be written down. Once for all, the sensi- ility of the instrument can be determined by omparison with an absolute electrometer, or a alvanic battery. In the portable apparatus a urning match is used—instead of the water- ropping system, which the writer finds more onvenient than any other for a fixed appara- ıs—to reduce the insulated conductor to the ıme potential as the air at its end.—16. As as been remarked above (§ 4), it is the lectrification of the earth's surface which has ther directly or virtually been the subject of measurement in all observations on atmospheric electricity hitherto made. The methods which have been followed may be divided into two classes—(1), Those in which means are taken to reduce the potential of an insulated conductor to the same as that of the air, at some point, a few feet or yards distant from the earth. (2), Those in which a portion of the earth (see above, § 1) is insulated, removed from its position, and tested by an electrometer, in a different position, or under cover. The first method was very imper- fectly carried out by Beccaria with his long "ex- ploring wire," stretched between insulating sup- ports, on elevated portions of buildings, tree tops, or other prominent positions of the earth (see above, § 1); also, very imperfectly by means of "Volta's lantern"—an enclosed flame, sup- ported on the top of an insulated conductor. On the other hand, it is put in practice very per- fectly, by means of a match, or flame burning in the open air, on the top of a well-insulated con- ductor—a plan adopted, after Volta's suggestion, by many observers; also, even more decidedly, by means of the water-dropping system—de- scribed in the preceding extract—which has re- cently occurred to the writer, and has been found by him both to be very satisfactory in its action, and extremely easy and convenient in practice. The principle of each of these methods of the first class may be explained best by first consider- ing the methods of the second class, as follows:— 17. If a large sheet of metal were laid on the earth in a perfectly level district, and if a circu- lar area of the same metal were laid upon it, and, after the manner of Coulomb's proof plane, were lifted by an insulated handle, and removed to an electrometer within doors, a measure of the earth's electrification, at the time, would be obtained; or, if a ball, placed on the top of a conducting rod in the open air, were lifted from that position by an insulating support, and carried to an electro- meter within doors, we should also have, on pre- cisely the same principle, a measure of the earth's electrification at the time. If the height of the ball in this second plan were equal to one sixteenth of the circumference of the disc used in the first plan, the electrometric indications would be the same, provided the diameter of the ball is small, in comparison with the height to which it is raised in the air, and the electrostatic capacity of the electrometer is small enough not to take any considerable proportion of the electricity from the ball in its application. The idea of experiment- ing by means of a disc laid flat on the earth, is merely suggested for the sake of illustration, and would obviously be most inconvenient in prac- tice. On the other hand, the method, by a car- rier ball, instead of a proof plane, is precisely the method by which, on a small scale, Faraday investigated the distribution of electricity in- duced on the earth's surface (see above, § 1), by a piece of rubbed shellac; and the same method, applied on a suitable scale, for testing the natural

electrification of the earth in the open air, has given, in the hands of Delmann of Creuznach, the most accurate results hitherto published in the way of electro-meteorological observation.*—18. If, now, we conceive an elevated conductor, first belonging to the earth (§ 1), to become insulated, and to be made to throw off, and to continue throwing off, portions from an exposed position of its own surface, this part of its surface will quickly be reduced to a state of no electrification, and the whole conductor will be brought to such a potential as will allow it to remain in electrical equilibrium in the air, with that portion of its surface neutral. In other words, the potential throughout the insulated conductor is brought to be the same as that of the particular equi-potential surface in the air, which passes through the point of it from which matter breaks away. A flame, or the heated gas passing from a burning match, does precisely this: the flame itself, or the highly-heated gas close to the match being a conductor which is constantly extending out, and gradually becoming a non-conductor. The drops into which the jet issuing from the insulated conductor, on the plan introduced by the writer, produce the same effects, with more pointed decision, and with more of dynamical energy to remove the rejected matter with the electricity which it carries from the neighbourhood of the fixed conductor.

Electricity: Velocity of.—The velocity of the transmission of electricity has been a subject of careful inquiry, and of extremely interesting experiments, on the part of several distinguished *Physicists;* foremost among whom rank Professor Wheatstone and MM. Fizeau and Gonnelle. The nature of the apparatus used has been alluded to under CHRONOSCOPE; and also, at some length, under LIGHT, VELOCITY OF. The chief results are the following:—Mr. Wheatstone found that electricity can travel at a rate *one and a-half times* greater than that of light: in other words, "that the electric spark would go round our globe between seven and eight times in one *second.*" According to Fizeau and Gonnelle (whose results are virtually confirmed by Mr. Mitchel of Cincinnati), the beginning of an electric *current* may be transmitted along a copper wire at a velocity which is not greater than *three-fifths* the velocity of light.

Still greater discrepancies are shown by extensions of experiments, with the same object in view, to varied circumstances of insulation and length in the conductors experimented on. For instance, trials in Queenstown Harbour, in July, 1856, when the two portions of the first Atlantic cable, on board H.M.S. Agamemnon and the U.S. steam frigate Niagara, were for the first time joined into one conductor, 2,500 statute miles in length, gave about $1\frac{3}{4}$ seconds as the time of transmission of a signal from induction coils, through that length; corresponding to a velocity of 1,400 miles per second. The following table contains some of the chief results hitherto published as evaluations of the "velocity" of the transmission of electricity:—

	Miles per second.
† Wheatstone in 1834, with copper wire,	288,000
† Walker in America with telegraph iron wire,	18,780
† O'Mitchell, ditto, ditto,	28,524
† Fizeau and Gonnelle (copper wire),	112,680
† Ditto, (iron wire),	62,600
‡ A. B. G. (copper) London and Brussels telegraph,	2,700
‡ Ditto (copper) London and Edinburgh telegraph,	7,600
Induction coils through 2,500 miles Atlantic cable, tested by heavy needle galvanometer, Queenstown, 1857,	1,430
Daniell's battery through 3,000 miles Atlantic cable, tested by mirror galvanometer, Devonport, 1858,	3,000

Now it is obvious, from the results which have been quoted, that the supposed "velocity" of transmission of electric signals is not a definite constant like the velocity of light, even when one definite substance, copper, is the transmitting medium, but is largely influenced by the circumstances in which the conductor is placed, being, for instance, much greater when the wire is insulated in air on poles than when it is surrounded by gutta percha and iron sheathing, and either submerged, or in coils as on board ship. Further, it is to be remarked that even in conductors, in precisely similar lateral circumstances, the apparent "velocity" is greater the smaller the length of the conductor used. Lastly, we may allude to the fact that some experimenters and writers have maintained, that the velocity of the transmission of electric signals differs with the source of excitation, being on the whole greater, the more intense and sudden is the electric impulse applied; while, on the highest authority, it has been maintained that the velocity of transmission is quite independent of the intensity of the source. Among all these discrepancies between the statements of careful experimenters and writers of high intelligence, how is the truth to be under-

* Through some misapprehension, Mr. Delmann himself has not perceived that his own method of observation really consists in removing a portion of the earth, and bringing it insulated with the electricity which it possessed *in situ*, to be tested within doors, otherwise, he could not have objected, as he has, to Peltier's view.

† Liebig and Kopp's Report, 1850 (translated), p. 168.

‡ Athenæum, 14th January, 1854, p. 54.

tood? We cannot doubt the general accuracy f their results; and if their statements and conlusions are contradictory, we may be sure that n explanation is to be found in a comprehenive theory of the subject. It would carry us much beyond the limits of the present article to o more than sketch very slightly the chief oints of electro-dynamic theory which are inolved.

In the first place, it must be considered that hree properties of electricity, in the present state f science not understood except as quite distinct om one another, are concerned in the transmison of an electric signal along an insulated conuctor:—(1), "Charge" or electrical accumulation a conductor subjected in any way to the pross of electrification. (2), "Electro-magnetic inuction," or electromotive force excited in a nductor by variations of electric currents, either adjacent conductors or in different parts of own length. (3), Resistance to conduction rough a solid. We may illustrate these three operties of electricity in an elongated conductor, ch as a telegraph wire, by considering their drodynamical analogies for water in a canal or a tube:—(1), Accumulation of a greater or less antity of water in any part of the canal or tube.), Inertia of the water. (3), Viscosity or fluid ction. If the first did not exist, as would be e case if the water were incompressible, and if were inclosed in a perfectly rigid channel or be, completely filled by it, the velocity of water wing along the canal would necessarily be the ne throughout its length. In these imaginary cumstances, if a piston is pressed into the tube one end, the effect in moving the water must nmence simultaneously along the whole length, l the velocity of transmission of a water pressure nal must be infinite (although, of course, the ximum strength of current producible by the ce applied cannot be acquired in an instant, bese of the inertia of the water). But, in reality, ter is somewhat compressible, and therefore, n in a perfectly rigid tube, the immediate conuence of pushing forward a piston at one end, to cause a condensation, and therefore accuulation of water in the near parts; and, accord- to the dynamical theory of sound, the first ct only reaches a distant part of the tube after inite time, corresponding to a constant velo-, called the velocity of sound, which depends ly on the compressibility and the inertia of fluid. If the tube inclosing the fluid, instead being perfectly rigid, be, as every real subce is, to a greater or less degree, somewhat ansible, the transmission of an impulse will modified—in general retarded—and to a very at degree, if it consist of such a substance as ia-rubber, when the compressibility of the er itself will not come sensibly into play, and first appreciable impulse, received at the ree end, will come in a time depending on the tia of the fluid and the lateral yielding of the tube, and corresponding to a velocity much inferior to that of sound in the fluid itself. Nearly the same law of motion will be followed under the influence of gravitation, instead of elasticity, by water in a canal, when a quantity of water, not enough at any time to increase or diminish the depth of the canal in any part by a difference considerable in proportion to the whole depth, is admitted or drawn off from either end. If, lastly, the viscosity of the fluid is taken into account in any of these cases—and if, to make the law of resistance to the motion of the fluid through its channel be the same as that of the resistance to conduction of electricity through a solid wire, we suppose the whole interior of the channel to be filled with porous or spongy matter, or to be closely set with transverse barriers, filled with minute apertures, the hydro-dynamical problem presents precisely the same elements for a mathematical calculation of the results, and the law of motion is expressed by the same partial differential equation as we have in the electrical problem to determine the laws of the transmission of electric signals through telegraphic lines, either extended on poles through the air, or insulated under the sea in the usual manner of submarine cables. In a line insulated in the air on poles the electrostatic capacity is extremely small, and the transmission of signals follows laws agreeing closely in character with those of the transmission of pressure impulses through water or air contained in a long rigid tube. Accordingly, a definite velocity of propagation of electric impulses, depending on the inertia and the capacity for charge, is to be looked for, as has been done in a first article, published by Kirchof, on the subject; and a law of extinction, identical with that of sound in a rigid tube, when sensibly influenced by the viscosity of the fluid, will be required to complete the theory by expressing the effect which resistance to conduction produces on the motion of electricity through the wire, as the same able mathematician has found in a subsequent investigation, recently published. This theory points to a velocity of propagation for electric signals in a telegraph wire considerably greater than that of light, and is so far in accordance with Wheatstone's observation; but it must be admitted that the foundation is incomplete on one important point—the electromagnetic induction which determines what we have called the "electric inertia;" and until this lacuna is filled up, it cannot be considered that we have a precise determination of the velocity of an electrical impulse, or of the time of an electrical oscillation through a telegraph wire in any circumstances. That it must be so great as not sensibly to contribute to the retardations of signals, or in any other way affect the practical working of submarine lines, is a fundamental assumption made by W. Thomson in his mathematical theory of the submarine telegraph, and justified by the fol-

lowing considerations:—(1), That any agency depending on electric inertia must give rise to retardations and certain other effects, in simple proportion to the lengths of line used; and therefore, that if any such effects bear a considerable part in the remarkable phenomena observed in signalling through submarine lines of 300 miles and upwards in length, they must, in lines of shorter length, give results very different from those actually observed. (2), That the character of the phenomena actually observed in submarine lines presents no feature attributable to electric inertia. (3), That a mathematical investigation, not yet published, showed that mutual electro-magnetic induction between the different conductors either of an ordinary multiple wire cable (in which the gutta percha coats of the different wires are each moistened all around by the sea water), or of a cable with two or more wires insulated in one continuous mass of gutta percha, and consequently electro-magnetic "inertia," in a single submarine conductor, cannot be sensible in comparison to what he called "peristaltic induction,"* if the length of the line be more than one hundred miles; although in lines of ten miles or less, effects of the former class, being in proportion to the length of the line, may actually preponderate over effects of the latter, which are in proportion to the square of the length of the line.

The theory founded upon this assumption, excluding the second of the three properties of electric action mentioned above, must of course rest on the first and third; and its fundamental principles are therefore the law of charge and the law of conduction. The retardations which it shows (depending on slowness of viscosity, not slowness of "inertia") follow the law which Fourier long ago discovered in his beautiful mathematical theory of the conduction of heat through a long bar (the "linear propagation of heat"). Thus an instantaneous application of electromotive force at one end of an indefinitely extended line, gives rise to a long gradual swell, and still more gradual subsidence, of electric current through any distant part of the conductor, the instant when the maximum strength of this current, or its maximum rate of increase, or its maximum rate of diminution, or when any stated proportion of the maximum is reached by the rising or falling flow at any point of the line, is later than the time of the initial impulse by an interval increasing in proportion to the square of the distance from the origin. The beginning of the current is instantaneous all along the line, according to this theory;—is in reality delayed only by electric inertia, and that not at all sensibly, and is practically observable after a smaller and smaller interval, the more sensitive the instrument employed to detect it.

Thus, in Queenstown Harbour, in 1857, the ordinary telegraph galvanometers and relays employed in observing the transmission of signals through the 2,500 miles of cable on board the two ships, gave their indications after a retardation of $1\frac{3}{4}$ seconds from the instant of the application of an electro-magnetic impulse at the remote end. At Keyham, in 1858, before the cable was again taken to sea, a quicker and more sensitive instrument—Thomson's mirror galvanometer—gave a sensible indication of the rising current at one end of 3,000 miles of cable about a second after the application of a Daniell's battery at the other.

Observations with the same instrument, or with different instruments of the same degree of sensibility, and with coils presenting no sensible resistances in comparison with the whole resistance in the line, would, if the insulation of the line were perfect, show retardations proportional to the squares of the distances travelled over by the impulse, provided the battery power is varied in simple proportion to the distance. In other words, the "velocity" of propagation might be said to be inversely proportional to the distance travelled. A rigorous verification of this law cannot be obtained in practice, because perfect insulation is unattainable; but one striking fact, alluded to above, is clearly explained by it—that, on the whole, the greater the length of line used, the less has been found the apparent velocity.

With reference to the velocity of propagation of regularly continued periodic impulses, whether from battery or from induction coils, applied at one end of a long submarine wire, the mathematical theory has given results identical with those which Fourier found in his investigation of the propagation of the summer heat and winter cold into the earth; and it thus appears that, in a telegraphic line of indefinite length, with a regular harmonic variation of potential applied at one point—(1), The retardations of maxima, of zeros, and of minima of potential and of current, are in simple proportion to the distances along the line. (2), The magnitudes of the effect diminish in geometrical progression, at equal intervals of greater and greater distance along the line. (3), The velocity of propagation of the phases mentioned in No. 1 is inversely proportional to the square root of the periodic time.

For further information on this subject, see ELECTRIC TELEGRAPH.

REFERENCES.—Faraday, Lecture to Royal Institution, January 20, 1854; *Journal* of the Institution and *Philosophical Magazine*; W. Thomson, Proceedings of the Royal Society (regularly republished a few months after each date in the *Philosophical Magazine*), "On the Theory of the Electric Telegraph," May, 1855; "On Mutual Peristaltic Induction between the wires of a multiple electric cable," May or June, 1856; Letter to the *Athenæum*, October, 1856; Kirchof, Poggendorf's *Annalen*, Band C., page 193, Band CII., page 529.

* Induction of charge by electrostatic force.

Electric Light. The light termed Electric, manifests itself under two sets of circumstances:—*First*, if a conductor be charged to a state of over-tension, a disruption or discharge takes place, accompanied by a snap more or less loud. This is finely illustrated by the Leyden jar. This spark varies in colour and character according to the medium through which the discharge takes place, be that atmospheric air or any other gas, and it varies also with the metals of which the conductor and the body receiving it are composed. *Secondly*, from the terminals of the wires from the two poles of a galvanic arrangement a succession of sparks so quick as to appear a continuous stream, may be obtained, provided the arrangement be powerful. Thin wire placed between the terminals may be raised to a white heat, or even melted, and if those terminals be connected with pieces of any inflammable substance—say of charcoal, combustion takes place, accompanied by the evolution of light of astonishing splendour. Wartmann produced an illumination in this way, in one of his experiments, equal to that of 300 gas jets. Bunsen computed that in one of his, light was evolved equivalent to that of 572 candles, and Fizeau and Foucault—employing 46 elements of Bunsen's pile—elevated the light to an intensity of about one-third or one-fourth that of the unclouded sun. Now the highest brilliancy ever produced by the Drummond light, or the combustion of lime in a jet of oxygen and hydrogen, is only about the hundred and fiftieth part of that proceeding from our luminary. Considering that the expense required to evolve this extraordinary illumination is really trifling, one cannot be surprised at the existence of a general and strong desire to turn it to economic uses. A mechanical difficulty stood at first in the way, originating in the waste of the charcoal. But means were found to move one terminal, so that the charcoal points might be kept at a uniform distance. The *Regulators* of Starke and Petrie in this country, of Achereau, Foucault, Breton, and, best of all, of Dubosq, in France, are all that can be desired: but the ordinary use of this light is forbidden by its very splendour. It quite dazzles every one near it, and can be endured only at a considerable distance. It will never therefore be applied to light up our houses or streets, but it would be invaluable in lighthouses.—Let us proceed, however, to a scientific analysis of the Electric Light. This analysis, as already hinted, must consist of two parts—an analysis of the Electric Spark, and an analysis of the Light produced by *Currents*. Of course the instrument of analysis is the prism; we must examine the electric spectra, exactly as we do the spectrum yielded by a solar ray.

I. The Electric Spark.—The spectrum yielded by the electric spark consists of two spectra overlapping each other—one belonging to the gas through which the spark passes, and the other to the metal of the conductor. The two are easily separated by varying the conductor while the gas or air remains the same.

(1.) The spectrum that is independent of the metal of the conductor, is like the solar spectrum traversed by a great multitude of lines, which, however, are the reverse of Frauenhofer's, inasmuch as while the latter are dark lines, the former are brilliant ones. The comparison between the two spectra will be made more easy by aid of the annexed diagram executed by Angström, in which they are placed side by side—the upper being the electric spectrum. The luminous lines have the colour of the space in the

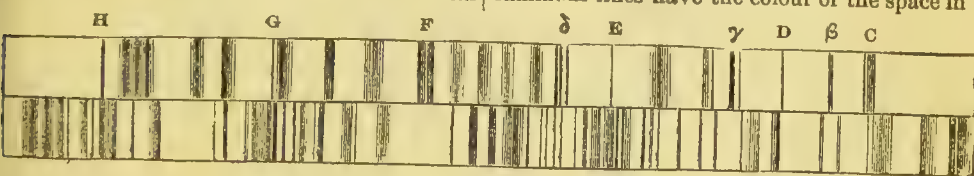


Fig. 1.

spectrum to which they belong.—On inspecting these two spectra, a general resemblance is apparent—sufficient to convince one that the explanation of the dark lines must carry with it in substance an explanation of the bright lines also; nevertheless there is very little coincidence of individual bands. Such lines as C, D, E, G, and also in the electric spectrum, nearly correspond with lines in that yielded by the solar ray; but to the two electric bands which far exceed all the others in brightness, viz., γ and δ, there is no dark analogon whatever.—The electric spectrum, figured above, is that evolved from a spark passing through our common atmosphere; but on every account it would be interesting to know what occurs in the case of its passing through different gases. We are indebted to Angström for another diagram, illustrating this part of the inquiry, which we also subjoin.—These diagrams

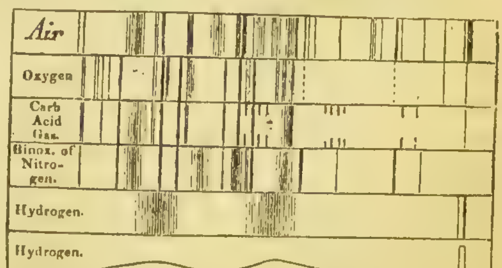


Fig. 2

are exceedingly curious and suggestive. It will be seen that some of the brightest of the lines in

the air spectrum disappear from the oxygen one, and reappear in that yielded by the spark passing through nitrogen. The hydrogen spectrum is exceedingly peculiar, and to render it more definite, Angström places a waving line below, indicating the degree of the illumination of each bright region.—One general remark, we think, may be hazarded, as fairly deducible from these appearances, viz.:—the whole phenomena must have a close relation to the aeriform bodies through which the dispersed light passes; so that the theory of Frauenhofer's dark lines ought to be sought for in actions of the Earth's atmosphere, and of the atmosphere of the Sun.

(2.) The second spectrum, or that derived from the combustion of the metallic particles carried from either pole by the electric discharge, and often, if not always volatilized, is an exceedingly curious one. This spectrum does not yield complete transverse lines, but only parts of such, appearing to proceed from both edges of the prism, and becoming extinguished before they reach the centre. Each metal has lines peculiar to itself. One or two lines, indeed, such as *n* and *m*, seem common to all the metals, but on the whole they are very independent of each other. Wheatstone—to whom this class of researches is very largely indebted—observed, that when the poles are formed of different metals, the spectrum contains the lines of both; and when a chemical composition of two metals is employed the spectrum also yields the lines of both, and not a new system.—It were useless to speculate, in our present state of knowledge, on the causes of these most curious appearances.

II. Light from the Electric Current.—The best mode of studying the phenomena of this species of electric illumination, is through its manifestations in the Electrical Egg (*q. v.*), or vacuum tubes. There are two sets of circumstances here also that merit attention.

(1.) The student who has read the article just referred to, will recollect, that around the two poles, lights of different colours are collected during the discharge—a light of fiery red adhering to the positive pole, while—separated from the red light by an obscure space—a blue tinge envelops the negative or warmth pole. Neither of these lights are homogeneous, as may be readily proved by the method of absorption; but Dové of Berlin appears to have first thought of studying them by aid of their spectra. He easily disposes of the idea that any differential phenomena can, in this case, be due to peculiar collections of gases around these poles; for when the poles are altered on the instant by the commutator of Rhumkorff's machine, the spectra change with them. The following are the appearances he witnessed:—The current being passed through a pear-shaped Geissler tube, in which the electrodes are platinum points, the spectrum of the blue light showed a large black streak in the blue, a second similar one at the limits of the blue and green, a very small streak at the limits of the yellow, and nothing in the red. The light at the positive pole gave a crimson violet and blue band, several small streaks in the green, a very black streak at the limits of the yellow, and a small dark streak in the middle of the red. Changing the tube for one of another shape, the spectrum of the blue light remained unaltered, but in the positive spectrum two additional small dark streaks appeared within the blue band. If brass electrodes (points) are employed in the common electrical egg, the phenomena are altered in a very few particulars only, viz.: the negative spectrum displays no new bands, but between its two broad bands the light appears greenish; and a few more bands appear in the blue of the spectrum of the red. Substituting a ball for a point at the negative pole, it seemed as if the two lights came to contain a portion of each other; nevertheless in every case the spectra remained different and distinct.—Dové does not attempt to speculate concerning the causes of these singular appearances, but he suggests a valuable practical application of them. It has long been a favourite conjecture, that the northern aurora is really due to electric currents passing through a highly attenuated atmosphere—the fiery red at the positive pole of a vacuum tube intimately resembling the red which characterizes many auroras. "Now," says Dové, "the peculiarities presented by the electric light *in vacuo* are so marked, that it appears easy to decide definitively by prismatic analysis, whether the light of the Aurora borealis is or is not of an electrical nature." An assimilation, to a certain extent—not precisely as Dové indicated—has been accomplished by Dr. Robinson, of Armagh. Under article Electrical Egg we have noticed the marked presence of fluorescence in the light produced by electric discharges. Dr. Robinson writes as follows:—"On the occasion of an aurora of more than average brightness, on 14th March, 1858, I availed myself of the opportunity to try whether this light was rich in those highly refrangible rays which produce fluorescence, and which are so abundant in the light of the electric discharges; and I found it to be so. A drop of desulphate of quinine on a porcelain tablet seemed like a luminous patch on a faint ground; and crystals of platino-cyanide of potassium were so bright that the label on the tube which contained them (and which by lamp-light could not be distinguished from the salt at a little distance) seemed almost black by contrast. Their effects were so strong in relation to the actual intensity of the light, that they appear to afford an additional evidence of the electric origin of the phenomena."

(2.) The spectra produced by the light evolved by the current passing through rarefied gases that show the stratifications, have been recently examined by Plücker. They are exceedingly brilliant, and differ wholly from those belonging to the electrical arch of light in the air, or in

gases of ordinary density. They are besides so distinctive, that Plücker sees in them the surest mode of determining which gas it is that remains in the tube. The Geissler tube used was one of a peculiar shape. In length above two feet, it had in the middle an ellipsoidal or cylindrical widening. The ends through which the platinum electrodes passed, were wider and shorter cylinders; and these were connected with the middle piece by two tubes of different diameter, the narrower being a rather wide thermometer tube. In the spectra, from this tube and all others, the dark intervals of the stratification are of course changed to longitudinal ones, traversing all the colours of the spectrum; and every such spectrum differs from the ordinary solar spectrum in this,—the colours do not merge into one another. On the contrary, they are sharply demarcated; and the separate spaces of colour are also divided into well defined lighter and darker stripes. The following are Plücker's notes concerning the spectra from these stratifications, as manifested by attenuated hydrogen, oxygen, and fluoride of boron:—1. *Hydrogen* gave a comparatively simple spectrum, in which five bright bands of almost equal breadths were prominent;—A, a bright violet band beyond the limits of the spectrum; three bands in the green, of which one, B, bounded the green towards the violet, D forming the boundary on the other side, and C between these last about twice as far from B as from D: finally, a beautiful yellow band, E. The order of intensity of the bands in the green is D, C, B, D being the brightest, and of a yellowish tinge. The red is very prominent; a thick black line occurs near its farther boundary; it is separated from the yellow band, E, by another broad black band. D is separated from E by a broad gray interval. The violet light is confined to the band A. The space between A and B is divided into a completely black space, and one of an indefinite dark colour. The black space sharply bounding band A is three times as broad as a third of the whole space between A and B, or between B and D.—How far is science as yet from being entitled even to venture a guess concerning the causes of such phenomena!—2. As to *Oxygen*, other puzzling circumstances occurred during the scrutiny of its spectrum. A pale flesh coloured light in the narrow part of the tube gave a remarkably bright band at the end of the red, and two beautiful orange coloured bands, separated by a narrower and a perfectly black one. In the green, bright bands appeared; the violet was very dark. *But the spectrum began to change.* The violet grew more intense, black streaks appearing in it, and the bright line to which the red was originally confined became paler and paler. Bright red bands appeared over a wider space alternating with dark ones. By increasing the power of the induction current, the spectrum was rendered constant for a short time, but it soon began again to change, diminishing rapidly in intensity; while the light in the narrow tube passed into violet. The discharge grew discontinuous, and finally the current *entirely ceased* to traverse the tube. The spectrum first seen was clearly that due to attenuated oxygen, and these changes must have been owing to its gradually losing its free condition under continued electric influence. Perhaps it may have first passed into ozone; at all events, in the end the tube behaved as a non-conducting vacuum. —3. The spectrum of gaseous *Fluoride of Boron* differs greatly from both the former, and is exceedingly beautiful. While it shows bright colours throughout, *red* together with *orange* and *yellow*, takes up about a fifth of the whole space: of the other four-fifths, two are occupied by the green, the remainder by the violet. There is no apparent transition from the violet to the green—the blue seeming entirely wanting. Yellow and orange form two sharply bounded bright bands of about equal breadth, both together being about half as wide as the red;—from this latter the orange is separated by a strong black line. Near the boundary of the red there is a second great black line. The space of the red between these two black lines is divided into six equal parts by five *fine* black lines; and on the other side of the strong black line first mentioned, there is in the orange and yellow also the same number of fine black lines, at the same distance from one another. In the green, about twice as far from the violet as from the yellow boundary, there is a bright green band, about as broad as the yellow band. This green band divides the green space into two parts, which differ importantly from one another. That part which lies next the yellow has a bright shining band in its middle, and the green on either side of this band is not uniform, but becomes gradually darker towards its extremities. The remaining portion of the green and violet have quite a characteristic appearance. This space seems divided into *sixteen* bands, *ten* of which belong to the violet. Each separate band is brighter towards the red, becoming gradually darker in the opposite direction. These bands are broadest towards the middle of the violet. The broadest of all is on the one side of a bright shining violet; on the other side it is completely black. At this part, the spectrum presents to the telescope the appearance of a strongly illumined fluted column. —The three spectra now described are essentially different from those belonging to the electrical arch of light in the air, and from metals glowing and burning in it. The electrical arch of light in the air is never free from matter (carbon or metal), whose incandescence gives rise to new bright lines in the spectrum; but Plücker thinks, with evident justice, that the varied appearances described above are closely connected with the chemical constitution of the attenuated gases. He concludes his memoir with the following provocative questions:—

How may the spectrum of a mixed gas be derived from the spectra of its constituents?—How are the spectra of a compound gas related to one another before and after its chemical decomposition by the current?—How does the chemical combination which the gas effects with the electrode, influence the spectrum?—Do isomeric gases give rise to similar spectra?—The student is further referred to these curious memoirs by Plücker; to the original researches by Angström (both of which are reprinted in the *Phil. Mag.*), and to the previous memoirs by Wheatstone. He will find further interesting information of another kind in Feddersen's paper on the Electric Spark, in *Phil. Mag.*, Supplementary No., Jan., 1859.

Electric Light, Stratification of. See Electrical Egg.

Electro-Chemistry. That electricity possesses the power of effecting chemical changes, may be seen in the instance of the combination of oxygen and hydrogen, when a spark from the common electrical machine is passed through a mixture of those gases. The same thing is noticed in the formation of nitric acid from the constituents of the atmosphere during thunderstorms, and the peculiar odour experienced during the working of an electrical machine, believed to indicate the formation of ozone. These may be given as instances of chemical action produced by the agency of electricity or electro-chemical effects; and the science which treats of them might be called Electro-Chemistry. But this term has more particularly reference to a far larger and more important series of actions, which are produced by the agency of the voltaic current—actions which have been found to be regulated by laws definite and most important in their practical applications in the arts, and also as regards the extension of chemistry itself, both in theory and processes. It is more particularly in effecting separation of the constituents of a compound, or in chemical decomposition that the chemical effects of the voltaic current are manifested. Bodies differ greatly as to the facility with which they yield to the decomposing powers of electricity. Thus, iodide of potassium readily yields to the action of a single pair of small plates, while water requires the application of a battery, and sulphuric acid can scarcely be decomposed even by the most energetic currents. The decomposition of water may be taken as a type of the mode in which electro-chemical decompositions are effected. When the conducting wires from the two poles or electrodes of a battery are immersed in water at a distance of half an inch from each other, bubbles of gas immediately begin to appear on the wires, and rise through the liquid. They may be collected in separate tubes held over the wires, and can be proved to possess the properties of oxygen and hydrogen, and when mixed and exploded, to be capable again of producing water. The quantity of water decomposed, and, of course, of gases produced, is found to be always proportional to the energy of the current, and may be taken as one of the readiest and best measures of its power. Annexed is a figure of the instrument known by the name of a voltameter, or indicator of the power of a battery. At the upper part of the figure are represented two small brass cups containing mercury, into which the conducting wires of the battery are dipped. From these cups descend, through a cork, two pieces of copper wire, to which are soldered two strips of platinum foil, of one or two square inches of surface. The cork is fitted to a wide-necked bottle, filled with water, to which 1-8th of sulphuric acid has been added. A bent tube to carry off the mixed gases is inserted in the cork. The platinum plates may be about one-fourth of an inch apart. The gases are measured in a graduated pneumatic jar, and the energy of the battery judged of by the volume given off in a given time. The sulphuric acid does not undergo decomposition, but is found greatly to assist the decomposition of the water—probably by promoting its conducting power. Steel plates may be substituted for platinum, if the water be saturated with carbonate of potash instead of sulphuric acid. Proposals have at different times been made to employ the galvanic battery as a mover of machinery, by exploding the gases evolved by it, from water, in a cylinder under a piston, but hitherto no practicable result has followed. It might be expected that, as each molecule of water is composed of oxygen and hydrogen, the decomposition of water would occur at some one point of the interval between the two plates or electrodes, and that the transfer of the liberated gases to the poles might be observed; yet the most minute inspection of the intervening space by microscopic means has failed to detect any such transport or the slightest indication of a current among the particles of the liquid. It is undoubtedly possible, considering how infinitesimally minute we know the ultimate atoms of substances to be, that the decomposition of water may be, at every instant, occurring, not merely at one point of the intervening chain of liquid particles between the two poles, but simultaneously at all the points, and that thus there would be equal and opposite sets of currents of gaseous molecules proceeding towards their respective poles, so that the one series of mechanical effects would counteract the other, and prevent the appearance of a visible stream among the liquid particles. This will not appear so improbable if we remember the excessive

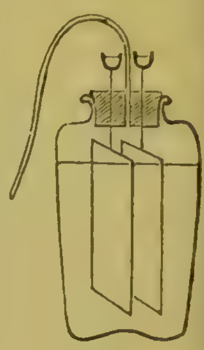


Fig. 1.

umber of molecules, which must, from the known iinuteness of the atoms of matter, go to form ven the smallest visible globule of one of the ases. The absence of any perceptible current etween the poles during the decomposition of a iemical compound is generally explained as llows :—Supposing, as before, the case of water, hich is composed of oxygen and hydrogen, and iat we have a number of molecules whose com- osition before the current has passed is symbol- .ed in the upper line of letters, and after the

OH OH OH OH
O HO HO HO H

rst passage of the current by the lower line. the two terminal molecules are acted on by the larity of the poles of the battery in contact ith them, and by alteration of their electric lations, the two gaseous molecules of each, separ- e from each other, the oxygen remaining at .e positive pole, and the hydrogen at the nega- ve; but the oxygen at the negative pole, in- ead of passing across the whole line of liquid to .e positive pole, merely by its altered electric ate decomposing the next atom of water, unit- g with its hydrogen and setting its oxygen ee to decompose the next, and so on in succes- on, thus transmitting a wave of force from one le to the other rather than a mechanical trans- rence. Indeed it is now believed that the pas- ge of current electricity along any conductor, , for instance, a wire, consists of this transfer- .ce of force from one particle to another, or rhaps by a series of decompositions and recom- sitions in a way somewhat similar to this, ther than by the passage of a fluid, as has so ng been the favourite explanation. Substances pable of being decomposed by electric currents e termed electrolytes, and the process of decom- sition is often called electrolysis. Binary com- unds are separated into their elements, the one ssing to the positive pole being called electro- gative, and the other called, with reference to electro-positive, going to the negative pole. lts and Haloid substances are decomposed into eir proximate constituents; for instance, a lution of sulphate of soda being subjected to e current, after a short time free sulphuric id will be found at the one pole and soda at e other. The chief electro-chemical laws iich have been discovered are as follow :—The me elementary substance in separating from a ven compound always passes to the same pole. ie quantity of a compound which is decomposed proportional to the quantity of electricity which sses. The quantity, by weight, of different mpounds which can in the same time be de- mposed by the same current is proportioned to e number representing the chemical equivalents atomic weights of the compounds. Thus 80 ains of sulphate of copper will be decomposed the same current which will in the same time compose 9 grains of water, or 58 of common lt. Difficulties are often experienced in tracing out the direct effects of the current, owing to what are called secondary results, that is, the combination of the liberated substances with the materials composing the poles of the battery, or with others mixed with the liquid. Thus, if in the apparatus for the decomposition of water, the pole where the oxygen ought to appear were composed of an oxidisable substance instead of platinum, as, for instance, zinc, then an oxide of zinc would be formed, and little or no oxygen would appear in the form of gas. This circumstance of the electrodes having an affinity for the newly liberated or nascent elements can be taken advantage of for the production of effects otherwise scarcely attainable, and also for enabling a small battery, or even a single pair of plates, to effect the decomposition of compounds which only yield to exceedingly powerful currents in the ordinary apparatus. Thus the most interesting and important results of Electro-Chemistry, are the decompositions of the alkalies, soda, potassa, and the earths, lime, magnesia, silica, &c., into their constituent metals. This was accomplished by Davy by means of the colossal batteries of the Royal Institution of London, and still requires by the best ordinary arrangements, currents of great energy. The little double-celled apparatus of Becquerel, as modified by Golding Bird, effects the decomposition of the alkalies and earths, and indeed of most other compounds, by the long-continued action of a feeble current, aided by well-chosen affinities, in an elegant and most convenient manner. This little apparatus is represented in the annexed woodcut. A glass

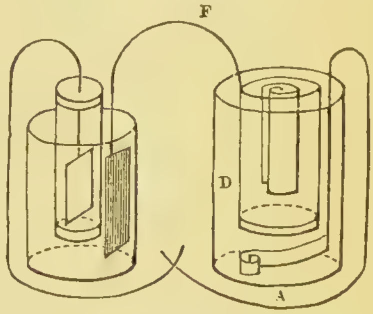


Fig. 2.

cylinder D, 1·5-inch in diameter and 4 inches in length, is closed at the lower end by a plug of plaster of Paris 0·7-inch thick. This cylinder is fixed by means of corks inside a cylindrical glass vessel A, about 8 inches deep, and 2 inches in diameter. A piece of sheet-copper, 6 inches long and 3 inches wide, having a copper conducting wire F, soldered to it, is coiled up and placed in the small cylinder with the plaster bottom; a piece of sheet zinc of equal size is loosely coiled up, and placed in the external cylinder, being furnished with a conducting wire like the other. The larger cylinder being then filled with weak salt and water (common salt), and the smaller with a saturated solution of sulphate of copper,

the apparatus constitutes a small battery, a modification of that known as Daniels' (see BATTERY), which will keep in action for several weeks, and may be used for decomposing such solutions as acetate of lead, iodide of potassium, &c., but may be made efficacious for more important effects by combining it with the second arrangement, as seen in the cut. The vessels of this are arranged precisely as in the case described, only into the external one is placed a weak solution of common salt, and into it is immersed a small plate of amalgamated zinc (see BATTERY) soldered to the wire coming from the copper of the small cell in the other apparatus. The inner cell is filled with the solution to be experimented on, and into it is immersed a plate of platinum foil, soldered to the wire from the zinc of the other cell. If solutions of the muriatic of tin, zinc, antimony, silver, &c., be introduced into the smaller tube beside the platinum, after a time varying from a few minutes to some weeks, the reduced metal appears on the platinum plate. If a solution of fluoride of silicon, prepared by passing a current of the gaseous fluoride into alcohol, be introduced into the apparatus, the platinum in contact with it in the course of a few hours becomes tarnished, and in a few hours more is covered with a dense coating of metallic silicon, which soon becomes converted into a white powder of pure silica by combination with the oxygen of the air. Metallic potassium and sodium can likewise be obtained from the apparatus, by putting a small capsule of mercury in the bottom of the decomposing cell, and pouring over it a solution of chloride of potassium, and bringing the platinum in contact with the mercury. The metals enter into amalgamation with the mercury, which is afterwards driven off by heat. Under the head ELECTROTYPE will be found some information regarding the practical application of Electro-Chemistry to the arts. And besides these applications, it may be mentioned that it is now extensively useful in the processes of preparing different metals on the large scale from their ores, and also that proposals have recently been made whereby different metallic compounds, such as those of mercury, and lead may be extracted from the living system in cases where they have been introduced as medicine or as poison. Becquerel and others have made an interesting and important use of the chemical action of electric currents in obtaining, by their means, the formations of crystals, many of which had previously been found in nature, but could not be formed by any artificial process, leading to the supposition that the electric currents which traverse the earth may have in many instances been the cause of their deposition. Many of the simple or compound crystals referred to can only be formed by the long-continued action of an exceedingly feeble current. As an example of one of Becquerel's arrangements, the following may be mentioned:—A glass tube, in the form of an inverted syphon is taken, and a small plug of moist clay inserted at the junction of the two limbs. Into the one limb is poured a solution of bicarbonate of soda, and into the other a solution of sulphate of copper to an equal height. A bent film of sheet-copper is plunged, one end into each solution. After a time, silken crystals of the double carbonate of soda and copper are seen floating in the solution. Such investigations have an obvious bearing on geological speculation.

Electrode. Synonymous with the term *pole* in galvanic arrangements: it is the substance, or rather surface, whether of air, water, metal, or any other body, which bounds the extent of the decomposing matter in the direction of the electric current. See ANODE, ANION, &c.

Electro-Dynamics. This term is applied to that branch of Electric Science which treats of the effects which are ascribed to the *motion* of electricity as contradistinguished from those due to its mere accumulation as in Electro-Statics. Strictly speaking, many of the effects treated of under the head of Electro-Statics are due to the transference of electricity from point to point, such as the combustion of wires, the luminous and heating effect of the spark, the sensation of the electric shock, and many others. Still, generally, the term Electro-Dynamics is confined to what relates to the Voltaic, Thermo-Electric and Magneto-Electric currents. This science is entirely of modern origin, dating from no farther back than 1790, the period of Galvani's great discovery, and in the interval has extended so rapidly and in so many directions, that it will be impossible in the limited space which can be devoted to it in this work to attempt a complete exposition of all its branches. Under the head BATTERY will be found an account of the means by which electric currents are produced, so we shall here confine ourselves chiefly to an explanation of the laws which govern them, their effects, and some of the practical uses to which they have been put. The world has long been familiar with the celebrated experiments of Galvani, which led to the discovery of the voltaic pile, and the different opinions which were entertained as to the origin and nature of the qualities and powers possessed by this remarkable instrument. Galvani himself entertained the idea, that the new force was nothing else than the nervous influence, or life-giving current, which passed in the metallic circuit, and produced the phenomena brought to light by his investigations. Volta, with keener perception seizing on the fact of the metals in contact being of different kind, attributed the effects of the pile to the action of electricity, which he asserted was rendered free by the contact of the metals. More recent investigators, observing that the powers possessed by voltaic combinations were greater, as the chemical actions accompanying them were more energetic, adopted the theory, that while the newly discovered power was electricity, yet, that

he electricity was not the effect of the contact f the different metals, but was due to the hemical action which accompanied it. Notvithstanding of the many attempts which have een made to construct a rational and consistent heory of the voltaic pile, and while it has been roved that the powers possessed by the pile are ue to electricity, it cannot yet be said, that the uestion regarding the origin of this electrical xcitement has been satisfactorily answered. 'wo theories may be said at present to divide 1e opinions of Electricians. The one is a modication of that of Volta. It was adopted by the lustrious Ampere, and is still in various degrees 1 favour with many investigators. Its fundaiental hypothesis is, that the elementary moleiles of matter possess, inherent in their substance, 1d inseparable from them, portions of electric uid. Those of the elements which possess egative electricity, are called electro-negative, 1d have a tendency to appear at the positive ole of the battery in Electrolytical decomositions; and those that possess positive electrity, or electro-positive bodies, separate at the egative pole. Of the first, or electro-negatives, e Oxygen, Chlorine, Iodine, Fluorine, &c.; 1d of electro-positives, Hydrogen, the metals, &c. ut as elementary bodies do not, except in their iscent states, exhibit electrical properties, it necessary to suppose that each molecule attracts und it an atmosphere of the opposite electricity its own, which disguises its presence and prents the properties of free electricity being obrved. Supposing now that two particles of xygen and hydrogen should be brought into ch other's vicinity, the two would be similar to o charged Leyden jars, only that the positive ectricity would be in the interior of the one, 1d on the exterior of the other, and *vice versa.* t moderate distances all electric action would disguised, but if pressure or other cause of proximation be applied, the two opposite atmoheres might be brought so close together, as to nite in opposition to the disguising power of the iids belonging to the interior molecules, then e atmospheres being rendered neutral could no nger disguise the action of the opposite electriies of the molecules themselves, which would us unite, and if equal quantities of electricity as possessed, each would be rendered neutral, if the quantities were unequal, might have an cess of positive or negative fluid, which would tract round itself an atmosphere. This theory rther assumes, that by the contact of different bstances electricity is rendered free, and the nsion of this free electricity, constitutes what called the *Electro-motive force* of the combinaon, and that the accompanying chemical action only a result of the electro-motive force. Thus the case of the ordinary voltaic pair, consisting copper and zinc with acidulated water between em, the contact of the acid with the zinc gives se to an electro-motive force, by which the positive electricity is impelled towards the zinc, while the contact of the acid with the copper, produces an electro-motive force, which accumulates negative electricity in the copper, and these two forces thus conspiring, the metals are put into states of opposite electric tension, which state is imparted to the wires in contact with them. This tension, though in all cases feeble compared with the manifestations of frictional electricity, yet can be clearly evidenced by the electroscope. If we now suppose the two wires to be introduced into a substance such as water, consisting of a combination of an electro-positive and an electro-negative molecule, the negative pole will attract the positive molecule, viz., the hydrogen, and if its tension be strong enough, will separate it from the oxygen, and at the same time communicate to it as much negative electricity as to form its atmosphere. The hydrogen, therefore, in a neutral state, will escape from the combination as free gas, while the particle of liberated oxygen, being still in an undisguised negative state, will act on the nearest molecule of water, and being assisted by the repulsion of the negative pole, will effect its decomposition uniting with its hydrogen and setting free its oxygen, which effects a similar decomposition on the molecule in its vicinity, and so on along the whole line of molecules between the poles, till at last a molecule of oxygen appears free at the positive pole from which it derives positive fluid to form its atmosphere and escapes as neutral gas. According to this view the passage of electricity along a compound chain of molecules capable of decomposition consists of a series of compositions and decompositions along the line from particle to particle, and not of the transport of one particle from one pole to the other. In the same way the passage of the current along a line of homogeneous particles, such as a metallic wire, consists of a similar series of electrical transferences: thus the atoms of copper, being positive, are surrounded with atmospheres of negative fluid, and the whole chain of particles is in a state of equilibrium from the disguising power of the equal quantities of the two fluids; but when a voltaic pole is put into communication with one end of the wire, say that it is the positive pole, it will attract the atmosphere from the particle of copper, which will thus be left free and positively excited to act on the atmosphere of its next neighbour, of which it will partially deprive it, rendering it in its turn active to act on the next, and so on along the whole line of particles till the last, which will be left in a state of positive tension, or in other words, partially deprived of its disguising atmosphere. In this state it would remain, and the whole wire would be in a state of tension, but if the negative pole be applied to the last molecule this tension will be instantly relieved by the communication of the requisite quantity of negative fluid to form the atmosphere, and thus the same series of

actions may recommence and so on continuously constituting the *current.* The same result would ensue were we to suppose the commencement of decompositions to have occurred at the negative pole of the battery. We have seen that the result of two oppositely electrical particles being pushed into each other's vicinity would be the union of the two atmospheres, and then the attraction of the molecules themselves into close alliance as a compound molecule. It is clear that if the two uniting molecules possessed equal quantities of opposite fluids the compound would itself be neutral and would require no atmosphere or excess of opposite fluid to disguise its electrical state, but if one of the uniting molecules possessed an excess of fluid, then the compound would not be neutral, but would require a partial atmosphere for its neutralization. If we suppose the uniting particles to be of the same kind, that is, both positive or both negative, such as two metals, then the two atmospheres would expand over the united molecules, in an equal degree on each if the two were of equal original tension, but if the one were more positive than the other, as in the case of copper and zinc particles brought into contact, then the atmospheres would pass from the points of contact and expand themselves on each particle, but not in an equal degree nor yet in the same degree as before contact, but in a less degree on the particle of greatest tension: and if the two were removed from contact an excess of atmosphere would remain on the least positive of the two, in this case the copper, and thus the copper having on its particles an excess of negative atmosphere, would be left, after separation, negative with regard to the zinc and to other bodies, giving an explanation of the electromotive force produced by the contact of heterogeneous substances. According to this view then, the source of the electricity of the voltaic pile is in the electro-motive force developed by the contact of the different substances, and the accompanying chemical action only serves continually to discharge the tension so produced by affording means in the formation of the atmospheres of the new products for the escape of the liberated electricity, so as to allow of the continued development of more by contact, and thus the *current* or *continual charge* and *discharge* is kept up. The other view, to which allusion has been made, regards the chemical action as the cause of the development of the electricity. Be this as it may, it fortunately happens that the laws which regulate the strength of the current when the electro-motive power has once been produced have been ascertained in a precise manner, and are independent of either theory. These laws have been named the laws of Ohm, after their discoverer. Before Ohm's investigation, it had been noticed that the efficiency of a current in producing any of its characteristic effects, such as deflecting a magnetic needle or producing chemical action, was enfeebled more and more as the conducting wire of the circuit was longer, but yet it could not be precisely stated that the strength of the current was less in the same proportion as the wire was lengthened. Ohm first took into consideration the resistance not only of the wire but of the materials of the pile or battery itself, and taking care properly to estimate each of these, he found that the law could be exactly stated that *the force of the current was inversely proportional to the sum of all the resistances.* As may be supposed also, the force of the current varies according to the energy of the source of electricity itself, that is to the nature and the activity of the galvanic combination. Thus if E be taken to denote the energy of the source of the current or the electro-motive force, and R to represent the whole resistance encountered by the current in the circuit, then F, the force of the current, will be denoted thus

$$F = \frac{E}{R}$$

F denotes the efficiency of the current to produce magnetism or chemical action or heat or any of its other effects, and is called the force of the current. E, or the electro-motive force, does not depend on the size of the plates of the battery but denotes its state of activity, which will of course vary according to the combination employed and the strengths of the liquid solutions, but would be as great for any plate, however small, as for the largest possible. R, the resistance, includes the resistance of the wires, the interposed liquid, if such be included in the circuit as in the voltameter (see ELECTRO-CHEMISTRY), and the resistance of the metals and the liquid of the battery itself. This resistance of the battery can be ascertained in any case by experiment. It is found that this portion of the resistance is diminished by increasing the size of the plates, and in exact proportion as the surface is increased; so that in this way large plates, though they do not increase the electro-motive force, yet greatly contribute to the force of the current by diminishing the resistance. At least this is the way of expressing their manner of action, and it answers perfectly in calculation. In regard of the wire, experiment proves that by doubling the length the resistance is doubled and the current proportionally enfeebled; by trebling the length the current is diminished in a threefold degree, so that the resistance of the wire is directly as the length. Experiment also proves that a wire gives a more energetic current the thicker it is, and exactly in proportion as the wire is rendered thinner the current it conveys from a given battery is enfeebled, so that the resistance of the wire is inversely proportional to its section. If we now denote the whole resistance by R, as before, and the resistance of the battery itself by B, and that of the wire by W, we shall have, supposing no other obstacles to be included in the circuit, $R = B + W$.

t B varies directly as the distance between the tes which we denote by d, and inversely as surface of the plates S, and we may put $= \frac{d}{S}$ Again W varies directly as the length the wire, and inversely as the area of its sec- n, it may therefore be represented by $\frac{l}{s}$, en l is the length of the wire and s its section. now we introduce these values of W and B into formula for F, we shall have F expressed in ms of the elements upon which it depends, giving

$$F = \frac{E}{\frac{d}{S} + \frac{l}{s}}$$

om this formula it is obvious that we can in- ase the force of the current by increasing the ckness of the wire or diminishing its length, if these be not practicable, by increasing the face of the plates of the voltaic elements or ninishing the distance between them, or lastly, increasing E by employing more energetic ments. The formula just given which embraces m's laws, serves for predicting the effect of nging the length or thickness of the conduct- wire or varying the size and distance of the tes of a voltaic pair in any case where we p to the same kind of element and conducting re; but if we wish to construct a formula ich shall enable us to calculate the effect of voltaic element or conducting wire from those another, then we must introduce two other mbols which may be replaced by the real antities which they denote as determined by periment. These are the resistance of the parti- lar liquid used, which we shall denote by A, d the specific resistance of the kind of metal ming the circuit, which we may denote by m, en we shall have

$$F = \frac{E}{\frac{A\,d}{S} + \frac{m\,l}{s}}$$

ie resistances of different metals are of course versely proportional to their conducting powers, ich may therefore readily be got from the fol- wing table of conducting powers:—

Mercury	100
Iron	650
Platinum	855
Copper	3840
Gold	3960
Silver	4000
Palladium	5790

will thus be seen that a similar thickness and ngth of copper wire would enfeeble a current of ectricity much less than one of iron; and from hat has been said above, it is evident that to et, with the same length, as strong a current om a given battery by means of an iron wire, a copper one, we would require to use an iron ire of greater section than that of copper in the ratio of 3840 to 650 or nearly 6 times as great, which if the wires were round would be given by a diameter nearly $2\frac{1}{2}$ times as great. The formula just given applies to the case of a single voltaic pair, and must be modified in order to be adapted to the calculation of the force of a battery composed of any given number of such pairs joined together in the manner described in the article BATTERY. It is evident that as each pair contributes its own electro-motive force to the current, the whole electro-motive force will be proportional to the number of pairs, so if we denote by n the number of pairs, then the whole electro-motive force will be n E, where E represents that of a single pair and all are equal. Although the electro-motive force be n times as strong the current will not be n times as forcible, because the resistance is also increased, the current now having to move through the whole liquid of the different pairs, which will be n times as great a resistance as the liquid of one pair. For a battery, then, the form for the force of the current will be

$$F = \frac{n\,E}{\frac{n\,A\,d}{S} + \frac{ml}{s}}.$$

This will serve in all cases, where the force of a single element or pair is given and the length of conducting wire, to determine the force of a battery of any number of pairs. In compound circuits, that is where the conducting wire is composed of different thicknesses or of different metals joined together, it is found that the force of the current is everywhere the same. It is convenient to refer all resistances of wires or liquids or other matters through which the current passes to certain definite equivalents on a specific standard; this is generally taken at so many units of length of a standard copper wire of specified thickness. The length of this wire, or the number of units of length of it, which afford the same resistance as any conductor, is called the reduced length of that conductor. If we represent the length, conducting power, and section of the substance forming the circuit by l, c, s, and the reduced or equivalent length of the standard wire by l', its conducting power and section being c' and s', then, as the force in each case is represented by

$$f = \frac{c\,s}{l} \text{ and } f' = \frac{c'\,s'}{l},$$

and it is required that the force of current given by the one is to be equal to that given by the other, we must have

$$f = f' \text{ or } \frac{c\,s}{l} = \frac{c'\,s'}{l'} \text{ or } l' = \frac{c'\,s'\,l}{c\,s}.$$

By this means it is easy to find the reduced length of any given length of any wire, and in the case of a compound conductor it is only necessary to calculate the reduced length of each

part and add the whole together to have the equivalent length of standard wire which, were it substituted for the given circuit, would produce no alteration on the force of the current.—I ouillet, Fechner, and Wheatstone have made many experiments confirmatory of Ohm's laws. Wheatstone employs a very useful instrument of his own invention, called a Rheostat, which essentially consists of two cylinders so arranged that it is easy to roll off any length of wire from one to the other and thus introduce it into the circuit, while at the same time its length is measured by the number of turns made by the cylinders. In this way it is easy by means of a galvanometer to value any given resistance in terms of the length of wire equivalent to it, or which, when introduced into the circuit, reduces the needle of the galvanometer to the same position. We can thus readily compare the electromotive forces of any two voltaic arrangements. It is evident that if the resistance in the one Electro-motor be as much greater than that of the other as the electro-motive force is greater, then the current should have the same force in the two cases, for, in the same proportion as the electromotive force is greater so it is required to overcome a greater resistance. If n be the ratio of the one electro-motive force to the other, then E being the one electro-motive force, n E is the other, and if R be the one resistance and n R the other, then

$$F = \frac{E}{R} = \frac{n E}{n R}.$$

and if by means of the Rheostat a certain known resistance r, in length of wire be added to the circuit whose resistance is R and electro-motive force E, then the force of the current is

$$F' = \frac{E}{R + r}.$$

It is evident that in the other circuit whose resistance has been previously determined, or supposing it to be as much greater than R as the electro-motive force is greater, which is common enough, then if we add by means of the Rheostat a certain length of wire in order to bring the galvanometer needle when acted on by the second current to the same point, and we find this length to be n times as great as that added to the first circuit, the two currents are equal in force, so their expression ought to be equal, and the forces of the currents can only be equal if the electromotive forces have the same ratio as the resistances. So we have

$$\frac{E}{R + r} = \frac{n E}{n R + n r}.$$

But n is known, and it is the ratio of the electro-motive forces. An exceedingly important principle following from the estimation of the resistances of different parts of the circuit, consists in this, that if the whole resistance be very great, as in the case of the long wires of a telegraph, then the addition of a large number of coils of a galvanometer wire, or the wire of an electro-magnet, produces very little resistance compared with the whole, and yet by increasing very much the number of turns, we increase in the same degree the power of the current in giving motion to the needles or magnetizing iron without perceptibly enfeebling its force. This may at once be seen from the general formulas for the force of the currents, or it may be stated thus, that though the addition of one mile of wire to a short circuit would make a very great alteration on the force of the current, yet the same length added to a long circuit of 100 miles, would produce very little diminution, hence it is beneficial to multiply the effect on the needles of telegraphs, by employing a great many turns in the coils. By means of the laws of Ohm, we are enabled to resolve, *à priori*, many problems which could not otherwise be accomplished; for instance, in the case that a branch wire is taken from the main circuit so as to cause the current, which passes along the single wire to the point of division, to divide itself into two for the remainder of the course, or for any part of it till the two again join; it is required to determine in all cases of difference of length, thickness, and conducting powers, what would be the comparative force of current in the different parts of such a compound circuit. Let N D O P in fig. 1, be the principal circuit, and D L P the additional one, then N D is called the partial circuit, D O P the circuit of derivative, and D L P the derived circuit. The currents in these parts receive the same names. The current which would pass in N D O P if there were no derived current, is called the primitive current. It is not the same as the partial current in N D.

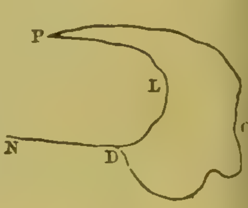


Fig. 1.

The lengths, conducting powers, and sections of the different wires, and the force of the primitive current being given, it is required to determine the force of the partial current, which we may denote by x, the derivative current y, and the derived current z. If the wires have different conducting powers or sections we can reduce them to the same by a formula previously given. Suppose then l to represent the length of the partial current, l' that of the derived current, and l'' that of the derivative current, f being the force of the primitive current. It is evident that we can substitute a wire instead of D L P the derived circuit, which shall be of the same length as D O P the derivative circuit, and which shall produce no alteration on the currents, provided that in the same proportion as we shorten or lengthen the derived wire, we diminish or

rease its diameter, as this follows from Ohm's ' of the resistance. Then to find this section have

$$s' : s :: l' : l''$$

ignifying the original, and s′ the calculated :ion of the derived circuit, giving

$$s' = \frac{l' s}{l''}.$$

› two wires, viz.: the derived and the deriva- : circuit, may now be reckoned of the same ;th, so the two will be equivalent to a single › of this length, and of a section equal to the ı of these sections, which will be, if we reckon sections of the original wire, unity

$$s'' = 1 + \frac{l'}{l''}.$$

can evidently now calculate the reduced ;th of such a wire, or the length of wire ıe original circuit, which might be substituted it, without altering the current. Its length ld require to be greater in proportion, as the ion is greater by this law. Hence

$$1 + \frac{l'}{l''} : 1 : l' : l''';$$

$$\text{giving } l''' = \frac{l' l''}{l' + l''}$$

ı therefore is the length of wire which may ubstituted for the divided part of the circuit, out altering the current. It is now easy to ılate the partial current which would exist ıe circuit so completed, by joining this length :o the partial circuit. The total length of . a circuit would be

$$l + \frac{l' l''}{l' + l''},$$

the force of the current in such a circuit ıpared with the force in the primitive circuit, ıld be inversely proportional to their lengths. ce,

$$x : f :: l + l' : l + \frac{l' l''}{l' + l''};$$

$$\text{ıg } x = \frac{f(l + l')}{l + \frac{l' l''}{l' + l''}} \text{ or } x = f\frac{(l + l')(l' + l'')}{l(l' + l'') + l' l''}$$

ıing the force of the primitive current. Again, ınd the forces y and z, we know that their must be equal to the partial current x, also that x will divide itself between the ved and the derivative circuits, in the inverse of their reduced lengths. The first condition s,

$$x = y + z.$$

second, gives

$$y : z :: l' : l''.$$

These two equations give by elimination,

$$z = \frac{x l'}{l'' + l'}$$

$$y = z \frac{l''}{l'}$$

which by introducing the formerly found value of x, gives

$$y = f \frac{l''(l' + l)}{l(l' + l'') + l' l''}$$

$$z = f \frac{l'(l' + l)}{l(l' + l'') + l' l''}$$

x, y, and z, being the forces of the partial, the derivative, and the derived current, and all expressed in terms of the force of the primitive current, and the lengths of the different circuits It is evident that as l and l' become equal, the force in the derivative and the derived circuit would become equal, as then the expressions for y and z become identical. Under the same condition we find, that the force of the derived and derivative current will be each half the force of the partial current, as the expression for y and z each become half that of x. Again, it is obvious, that as the derivative current disappears the derived current vanishes, because the length of the derivative circuit is a factor of the numerator of the expression for the force of the derived circuit. The latter therefore vanishes along with the former. As an instance of the practical application of these formulas for the forces of derived currents, we may cite the mode, by this means, of arriving very easily and expeditiously at a knowledge of the spot where a rupture of the insulating coating of the wires of a subterranean or subaqueous telegraph has occurred, without the necessity of examining the whole wire piece by piece. To deduce a formula for this purpose from those above given, suppose that a single telegraphic wire only is used, that is to say, that the return current takes place by the earth. Let a current be sent along the wire while a galvanometer is included in the circuit at each end, then it is clear that the galvanometer at the end next the battery will receive the full strength of the undivided current, while that at the extreme end will only experience the effect of the derivative current, the derived current passing through the leak in the coating of the wire at the point of rupture. It is evident that the comparative strengths of these currents will be dependent on the lengths of the circuits, that is, on the position of the point of leakage, or the point of derivation. Let S and S′ (in fig. 2) be the stations, and R′ the point of leakage; suppose first, that the battery be placed at S, and let l denote the length of wire intervening between S and R, l' being the remainder of the wire, and l'' the length of the derived circuit, that is in this case, the length of earth current intervening between R and the plate

of metal immersed in the ground, as at S, and in communication with the battery there. In this

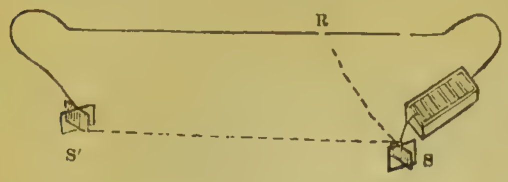


Fig. 2.

case it is evident, that we must add to the mere lengths of these currents the reduced lengths of any additional resistances which may be interposed in the course of the respective currents. These are the resistances the currents experience in passing from the immersed plates to the earth, which, for simplicity of calculation we may neglect, and suppose that the current experiences a resistance r in passing through the leak in the coating of the wire. As the earth offers no resistance to the passage of the current, it is evident, that the derived current only suffers the resistance r, and that this therefore takes the place of l'' in the formulas for the forces of the respective currents. If we denote by f the force of the primitive current, or that which would occur were there no leakage, and as before, x and y may denote the force of the partial current acting on the galvanometer nearest to the battery, and the derivative current acting on that at the other end of the wire, we shall have

$$x = f\frac{(l+l')(l'+r)}{l(l'+r)+l'r} \quad y = f\frac{r(l+l')}{l(l'+r+l'r)}$$

$$\text{or } \frac{x}{y} = \frac{l'+r}{r}$$

Again, removing the battery to the opposite end of the line and denoting by x' and y' the forces in the galvanometer nearest, and that most remote, we shall have

$$x' = f\frac{(l'+l)(l+r)}{l'(l+r)+lr} \quad y' = f\frac{r}{l'(l+r)+lr}$$

$$\text{or } \frac{x'}{y'} = \frac{l+r}{r}$$

By means of these two equations, containing the three unknown quantities l, l', and r, we can eliminate r and get an equation between l and l'. From the first, we get

$$r = \frac{l'y}{x-y}$$

and from the second

$$r = \frac{ly'}{x'-y'}$$

Equating these two values of r we have

$$\frac{l'y}{x-y} = \frac{ly'}{x'-y'}$$

$$\text{or } \quad l = l'\,\frac{x'-y'}{x-y}\cdot\frac{y}{y'}$$

which exhibits the ratio of l to l' or the proportions of the parts of the wire between either end and the point of leakage in terms of the observed force of the currents.

In experiments which have been made with a view to facilitate and simplify the electric telegraph, it has been found that the earth may be substituted as the return part of the circuit instead of, as was supposed, its being necessary that the complete circuit to and from the distant station should be formed of wire. All that is required is that the battery should have one of its poles connected with a metallic plate buried in moist ground, and that the other pole should be put in communication with a single wire which passes along the line on insulating supports, and after coiling round the electro-magnets or needles at the distant station should be joined to a wire from another plate there buried likewise in the ground. An anomalous fact observed in regard of this mode of completing the circuit is that if the distance between the two plates be small, say a few yards, then the intervening earth seems to present a perceptible resistance to the current, but if the distance between the plates be increased this resistance ceases and the current is as strong as if the earth were not included in this circuit, or had no resistance. This may be accounted for from Ohm's law, that the resistance is inversely as the section of the conductor, as in this case, the section may be regarded as infinite and of course the conducting power infinite or the resistance infinitely small. But whence should it happen that where the earth circuit is short a resistance is experienced? According to Ohm's law of the resistance being inversely proportional to the length, in this case the resistance ought to diminish whereas it increases. In order to the explanation of this circumstance many investigators have adopted the view that the earth does not act as a conductor returning the identical current that passed from the pile, but rather as a reservoir into which the tension of the two poles might be freely and perfectly discharged, thus accomplishing the same end as if the two poles of the pile were joined together by a good conductor, as this mutual discharge, constantly kept up, is what constitutes the current in the conductor. It is said by those who adopt this view that the reason that a short distance of earth intervening between the two poles apparently presents a resistance is, that it is not sufficient to discharge the electricities of the wires perfectly and instantaneously, so that a diminution of the current is thereby caused. Be this as it may, certainly that of this remarkable property of the earth with reference to telegraphic currents is one of the most important discoveries of recent times. Allusion has already been made to the probability of the current being a series of polari-

s by induction, produced consecutively along line of molecules of a conductor, and it may ily be supposed that as the exact nature of actions which give rise to these induced po- ities of the natural electricities of the mole- es is not ascertained, it would be difficult to luce precisely the laws which govern the speed h which the current would propagate itself ng conductors. Accordingly it is to experi- nt chiefly that application must be made when object is to ascertain these laws. As in the e of sound, where for waves of all lengths the same medium the velocity is the same, so this case it is probable that in the same sub- nce electric waves of all intensities will travel h the same speed, and also that the time taken un over a certain space will be proportioned the space, that is that the velocity will be form. These inferences have been confirmed experiment, yet it does not follow that though electric impulse or wave should travel with same speed in circuits of all lengths, yet that intensity of electric charge necessary to pro- e a given effect such as making a telegraphic nal, should do the same. It would seem in- d that this is not the case, and that though to nsmit an electric impulse along a wire 500 es would require five times as long as to nsmit it 100 miles, yet to raise the 500 miles vire to the intensity requisite for telegraphic poses might require much more than five times ong in the one case as in the other, a circum- nce that, if true, would exercise a most impor- t influence on telegraphic communication over g distances such as that across the Atlantic. eems also to be ascertained that the thickness nature of the insulating envelope, in the case submerged wire, exercises an influence on the e taken by the electric current in the wire to to intensity enough to produce its character- effects.

Many efforts have been made both on the graphic wires of Europe and America to ertain the velocity of the electric current in erent metals and under various conditions. observers agree that the velocity is very it, but owing to the vast rapidity, difficulty been found in making out its exact amount. S. C. Walker in the following way made ny experiments on the telegraphic wires of United States, and deduced a velocity of from 000 to 16,000 miles per second. Two cylin- s of metal were placed, one at each end of the e and metallically connected with it and with pole of a battery while the other pole of each tery completes the circuit with an electro- gnet, which raises and depresses a steel point inst a piece of paper placed on the cylinder h time that the current is passed. The cylinders are made to revolve with exactly same speed by clock work, and it will be dent that if an operator placed at each cylinder by a touch of his finger complete the connec- tion with the battery and animate each of the electro-magnets so as to cause them to press the steel point against the paper, and if they shall agree to transmit a signal alternately at a given interval, say three seconds, after the signal from the other arrives: now remembering that the transmitted signal registers itself on the cylinder whence it is transmitted as well as on the other, if we denote by a that made by the observer A on his own cylinder and a' that made by him on the opposite cylinder, b and b' denoting the same for the signals made by B, then in the case of no time being lost in the passage of the signal along the wire the following arrangement would be seen on the two cylinders:—

$$\begin{array}{ccc} a' & b & a' \\ a & b' & a \end{array}$$

But if a certain amount of time elapse, then the arrangement of the signals on the two papers would not be at equal intervals, but something such as

$$\begin{array}{cccccc} & a' & b & & & a' \\ a & & & b' & a & \end{array}$$

The difference of distance between a' b and a b' on the two cylinders being the space the surface of the cylinder revolves in twice the time occu- pied by the current in traversing the wire. Hav- ing ascertained this length, which we denote by d in inches, it is easy to deduce the velocity thus. Let c denote in inches the circumference of the cylinder, n the number of seconds occupied in completing a revolution, then the number of se- conds spent by the current in rushing along the wire is represented thus:—

$$t = \frac{n\,d}{2\,c}$$

And if l denote the length of the wire in miles

$$v = \frac{l}{t} = \frac{l\,2\,c}{n\,d}$$

where v represents the velocity in miles or the number of miles run over in a second. It must be observed that this method takes for granted that the signals can be made at exactly equal intervals, and also that the electro-magnets used to make the impressions either act instantaneously or at least always with the same loss of time, a circumstance that is not fulfilled unless the cur- rent should remain of exactly the same intensity. These difficulties can be got over by taking the average of a vast number of results.

MM. Fizeau and Gounelle have employed a different and very ingenious method of ascertain- ing the velocity of the electric current. Their ex- periments were made on the Rouen and Amiens telegraphs. They employed two discs of metal fixed upon the same axis, each disc being divided into a certain number of parts at its circumference,

the half of which were filled with pieces of wood or ivory as represented by the dotted lines in the cut. The axis is cut into two parts and united by wood, so as to prevent communications of a current from one disc to the other. One axis is put in connection with a pole of the pile, the other pole put into connection with a metallic plate buried in the earth. A pointed piece of platinum A, presses on the circumference of the disc and has in metallic junction with it the end of the telegraphic wire, the return wire being similarly joined to the platinum point B, which touches the circumference of the other wheel or disc.

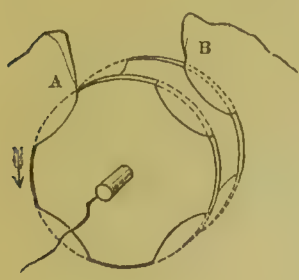


Fig. 3.

From the axis of this second disc there passes a wire of a galvanometer, the opposite end of which is in connection with another plate of metal buried in the earth. A and B are so fixed that when the wheel is turned as indicated by the arrow, one of the metallic divisions is leaving A exactly as a metallic division of the other wheel is coming in contact with B. It is evident by this arrangement, that so long as the wheel remains at rest in any position, the current cannot pass, and the galvanometer needle will remain at zero. And the same will be the case whatever degree of rotation be given to the discs, if the current pass perfectly instantaneously along the whole intervening wire, because at no turn is the circuit ever complete, as the points A and B can never be both on metal at the same instant. But if a certain time elapses from the moment the last electric wave leaves A as it passes from the metal, till the same wave arrives at B, then B may have entered on the metal, and may complete the circuit down to the galvanometer. It is further obvious that, as the rotation of the discs becomes more rapid, a greater amount of current may get past to the galvanometer, and that if the rotation be such that the whole of the divisions passes under the platinum while the current is occupied in passing along the wire, then the half of the whole current will pass to the galvanometer, but that if the speed of the disc be increased still more, then the force on the galvanometer will again diminish, as a greater interruption will take place to the current. If then, at the time when the maximum influence is perceived on the galvanometer, the number of turns per second of the discs be observed and denoted by n, the circumference of the disc in inches by C, the number of divisions by m, and the length of the wire by l, v the velocity in miles per second, l being also expressed in miles, is given by $v = l\,m\,n$; for as the whole circumference passes in $\frac{1}{n}$th of a second the length of one division, or $\frac{1}{m}$th of the circumference will pass in $\frac{1}{m}$th of $\frac{1}{n}$th, or $t = l\,m\,n$; but t, the time of one division passing, was also the time of the current running the whole length of the wire, so $t = \frac{1}{m\,n}$, but $v = \frac{l}{t}$, so $v = l\,m\,n$. In this way it appears that the electric current traverses iron wire at a rate of 63,000 miles per second, and copper wire at a rate of 111,000 miles per second. The nature of the pile, or its size, exercises no influence on the speed of the current. If, however, what was said with reference to the time required for a wire to rise to different degrees of electric tension, be true, it is evident that these numbers cannot be taken as absolute measures of the velocities of the electric wave, but only as indicating, that, with wires of a certain length and diameter, a force of current, sufficient to affect the needles of the galvanometer, passed throughout this extent at this speed. It is obvious that the subject of the propagation of electric currents in conductors, still remains in much obscurity.

As intimately connected with the theory of the transmission of electricity along conductors, and the velocity of such transmission, it may be well here shortly to allude to the interesting and novel experiments made within the past year by Faraday. These experiments were chiefly made by means of powerful batteries on long lines of immersed submarine telegraphic cable, the wires of which were each coated with gutta percha. He employed an insulated voltaic battery of 380 pairs. The distant end of the telegraphic wire was in communication with a plate of metal buried in the earth, as was also one pole of the battery. When the other pole of the battery was put in connection with the near end of the submarine wire, and the contact then broken, a vivid spark ensued, and if the two wires were held in the hands, the operator received a most violent shock, which continued for some time; by gently touching the end of the submarine wire again and again, it was possible to take away a series of more gentle discharges, though there had been no intermediate connection with the battery. As many as 30 or 40 shocks could thus be got in the same way, as if the wire had continually again the power to charge itself. If some time was allowed to elapse, after communication with the battery had been withdrawn from the wire, before the shocks or sparks were taken from it, they had evidently begun to grow more feeble, but still, after a number of minutes, they were abundantly perceptible. Not only were powerful shocks and sparks got from the submarine wire which had been separated from connection with the battery, but it could inflame gunpowder, and this even for a period

five or six seconds after the rupture of contact. ·hen a galvanometer was interposed in the ·cuit between the end of the wire and the rth, after its communication with the battery d been broken, the needles were violently ıirled round, even at the end of a period of ıny minutes.

In other experiments, the galvanometer had e end of its coil fastened to the submarine ble, while the other end was put in communi- :ion with the battery, the remaining pole of ɜ battery being still in communication with ɜ soil; in these circumstances, the needles wly deviated as if the electricity of the battery re pouring itself into the cable. When the :tery was separated from the galvanometer, l the latter touched by the finger, or put into nmunication with the earth, the needle de- .ted in a contrary direction, indicating an ·erse current, or that the electricity which had ·ssed into the cable was returning. The same ɜnomena were produced, whether the com- nications of the cable with the earth were ·ken at one or both ends at the same instant, l they took place at either end of the wire, or h either pole of the battery. A battery of at power was necessary: thus, 12 pairs of tes in a high state of activity produced no h effects. It was also noticed, that a simila· le suspended in the air, produced none of the ve described phenomena, though its insula- ı was as perfect, whence, it would seem, that water which surrounded the gutta percha :elope was one of the necessary conditions. is probable, indeed, that the wire and the :er, separated as they were by the film of ta percha, formed a disguising arrangement ilar to the armatures and glass of a Leyden (see Battery, Electric), whereby great ımulation would occur, the battery serving ı constant source of supply, as if it were an tric machine. On calculation, it was easy to from the length and diameter of the wires, their envelopes, that a Leyden battery was ɜ formed of 1,000 square yards of coated sur- · Thus, though the tension of the electric ·ge could not rise to that of the ordinary tric machine; being no higher than that of pole of the battery of 380 pairs; still, its ıtity was enormous, being the full charge of ı0 yards of surface.

everal galvanometers were next introduced the circuit, as, for instance, one at each end he cable, and one in the middle. It was ı observed, that the galvanometer nearest the ery was affected the instant the circuit was pleted, while the one in the middle was some t time in indicating the arrival of the cur- , and that at the extreme end only showed presence of electricity after several seconds, never reached the same amount of deviation ıe other instruments. When all the needles ɜ in a state of deviation, if the connection with the battery was broken off, the nearest galvanometer instantly showed a total cessation of current, the one end of its wire being insulated from the earth, while the other two galvanometers continued for some time to indicate a current, showing that the whole electric charge of the wire was flowing down into the earth by the extreme end of the cable. If, instead of leaving the end of the galvanometer coil which had served for establishing the communication of the whole with the battery free, it be, as soon as it is separated from it, suddenly put into connection with the earth, the whole of the electricity does not then escape by the distant end of the wire, but part of it returns, causing an inverse deviation of the near galvanometer, and exhibiting the curious spectacle of the current passing along the two ends of the wire in opposite directions at the same instant.

Such experiments show that electricity will not freely escape from a wire surrounded by a nonconducting and round that a conducting envelope constituting a condensing apparatus, until so much of the current has entered as to raise its tension to be nearly equal to that of the battery, and that this takes a longer and longer time as the length of wire is greater.

The effects of the electricity may be enumerated as follows:—1st, Chemical; 2d, Thermal or heating; 3d, Magnetic, as evidenced in the mutual influence exerted between a magnetic and an electric current constituting Electro-Magnetism; 4th, Induction, as shown in the induction of magnetism in iron and other similar bodies placed in the neighbourhood of the current, and also in the induction of electric currents in neighbouring conductors. This arrangement is far from faultless, as it by no means embodies all that relates to the current, and some of the subdivisions encroach on and interfere with others, but it serves in some degree as a basis on which to proceed. The first head will be found discussed at some length under Electro-Chemistry.

The wire of an active galvanic battery becomes heated by the passage of the current; and it is observed that the amount of heating rises more rapidly than the power of the battery; that is, that a doubly powerful battery will heat a given wire with more than double effect. It is also noticed, that more heat is produced by a given battery if the conducting wire be thick than if it be thin, and these relations have been stated thus, that the amount of heat produced is inversely proportional to the diameter or the conducting power of the wire, and proportional also to the square of the quantity of electricity which actually passes. It follows that, by increasing the power of the battery, or by diminishing the thickness of the wire, we might get any temperature required, and this is so far practicable that it is easy to attain a white heat in the wire itself besides the development of a

large amount of heat in the battery, as evinced by the whole apparatus rising in temperature frequently to near the boiling point.—It is found that iron and platinum are much more efficacious as heat-producing channels than silver or copper, and, as some say, because they are more imperfect conductors; but, were this the true explanation, then it would follow that silk or gum lac, or glass would be still more efficacious, which is far from being the case. The true explanation no doubt is, that by some peculiarity of structure they are more efficacious in converting the energy of the current, whatever that may be, into the vibratory motion which constitutes heat, in the same way as some substances become more powerfully heated by a blow or by friction than others, by transmitting less of the applied mechanical energy to other bodies, and converting more of it into vibratory motion among the constituent molecules. A pair of plates, on Wollaston's or Smees' principle, of 4 inches square, moderately excited, are sufficient to ignite a steel wire of the thickness used for the hair springs of watches. The arrangement represented in the cut answers well for such experiments. A and B are two wires of copper covered with silk or cotton thread, and twisted for steadiness, while the thin wire is stretched between their points. When the poles of the battery are put into communication with A and B, the current passes through the thin wire, which instantly becomes heated to redness, if the battery is powerful enough, or is even dissipated in sparks of oxide by combustion. Gunpowder or other inflammable substances may of course be readily inflamed by being placed in contact with such a wire and the current passed. This fact has been taken advantage of in electric blasting, and, from its applicability in situations where the miner's fuse could not be used, has been of much service in practice. Under-water blasting is now altogether carried on by this means. It is also useful because of the possibility of insuring the simultaneous action of many charges of powder placed at a distance from each other for the purpose of effecting the dislodgment of large masses of rock or earth, as was the case recently at Dover, where nine tons of gunpowder in three charges were simultaneously exploded in cavities cut at distances of seventy feet from the sea cliff, and detached with perfect safety 600,000 tons of rock.—All that is necessary in such a case is to enclose in each separate charge of powder such an apparatus as that shown in the cut, and connect them all with wires led up through the packing of the mine to a powerful galvanic battery with which they are to be put into connection at the moment when it is wished that the explosion should take place. In military operations it has been found, by experiments which have been carried on by the Russian government through the instrumentality of Prof. Jacobi, that electric blasting may be exceedingly useful. It is believed to be the means in use in some of the "infernal machines" sunk off Cronstadt, and it has been recently used with success under the fortifications of Sebastopol.

Fig. 4.

It is found that large single pair voltaic arrangements are more powerful in producing heat, if no great length of wires is to be traversed, than numerous small plates formed with a battery. Accordingly, coiled plates of 400 square feet have been constructed, for the purpose of exhibiting on a great scale these effects. The names of Calorimotors or Deflagrators are given to them. If from one of the wires of such an apparatus, or even from that of a small battery, a thin leaf of metal be suspended, and a plate of copper in connection with the other wire be brought into contact with its edge, (fig. 5,) brilliant combustion will ensue, and part of the metal will be dissipated in vapour. Gold leaf gives a whitish light; silver, a bright green; zinc, Dutch gold, and tin, displaying different characteristic colours. If these experiments be made with a powerful battery, and in darkness, they constitute some of the most imposing of electrical phenomena. A steel point in connection with one pole, and dragged across a file of the same metal in connection with the other, gives out copious showers of red sparks, similar to those emitted under the operations of the blacksmith.


Fig. 5.

By far the most splendid exhibition of the heating effect of the voltaic current consists in the light emitted from two charcoal points, brought so closely together when in connection with the wires of a powerful battery, as to allow of the passage of the current between them. It ought to be understood that this is not a light directly from the current itself, but rather that it proceeds from its conversion into heat in the charcoal, which thus becomes intensely luminous. Charcoal recently prepared from some dense wood, such as boxwood, answers well for this purpose, but is on the whole less effective than the dense coke got from the roofs of the retorts, in coal gas works. This substance is either cut at once into pencils, or having been powdered, is pressed and hammered into moulds to give it density and form, and is then put into connection with the poles and steadied, the two points opposite each other by some arrangement, such as that represented in fig. 6, facility being afforded for regulating the distance between the points, so that the maximum light may be produced. That the light is not give

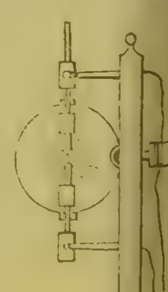


Fig. 6.

ıt by combustion of the charcoal, may be seen
' immersing the whole in water, when the light
ill still be produced, though more feeble, or
a vacuum, by withdrawing the air from a
ass globe surrounding the points, when the
me will become even more intense than in air.
ıe greatest barrier hitherto experienced in
e application of this most brilliant of all arti-
ial lights, to the purposes of illumination, has
nsisted in the difficulty of regulating with
fficient nicety the distance of the charcoal
ints from each other, as the substance of one
both irregularly wastes away by the contact
the air, and by sublimation of the particles.
is has been to some extent overcome by auto-
tic arrangements of electro-magnets, as a
ostitute for the human hand, and to a con-
erable degree successfully. If to this we add
recently opened prospects, that the processes
many chemical manufactures may be made
means of affording abundant supplies of
ctricity, in a form to fit it for this purpose, we
y exercise the hope that another brilliant
ievement by science and art will be added to
ir many former contributions to human com-
. and adornment.

)n the subject of the heating effects of Elec-
currents, it will be only necessary to add,
t they have been recently used in con-
ction with the concentrated sun's rays, to
ct the fusion of refractory minerals. The dia-
ıd has readily given way to this treatment,
melted into a globular bead, and according
ome, plumbago and coke when thus treated,
be converted into the precious stone, not
beautiful as the real it may be, but giving
e approximation to many of its characters.

Ve may next give a short exposition of the
netic relations of the current, constituting
t is called ELECTRO-MAGNETISM. The
lamental fact upon which this science is based,
he following:—If a magnetic needle free to
e on its pivot, be brought into the neighbour-
l of a wire carrying the voltaic current, the
which, if it be a non-magnetic metal, such
copper, had previously no action on the
net, now, by reason of the current, causes it
iove, and in a way of which it is not at first
to perceive the law. For example, if, as is
esented in the cut, (fig. 7,) the current be placed parallel to the needle and above it, the needle moves round on its axis to a new position, as shown by the dotted lines, and if the current be further approximated or be rendered more energetic, the needle will turn still more round if the force is powerful enough, a position of librium is assumed at right angles to the direction of the current, the directive force of the earth having been completely overcome. Farther round than this, no increase in the energy of the current can impel it. The only effect being, a greater pertinacity in retaining the new position in opposition to any force tending again to twist it towards parallelism with the current. If now we remove the current to a position below the needle instead of above, the poles will be turned round in the opposite direction, as is represented in fig. 8. If the direction of the current in the wire be reversed while it remains in the same position, the motion of the needle is instantly changed to the opposite direction, from which circumstances it follows, that, if the same current be carried in one direction, above the needle and back again, in the opposite direction below it, as in fig. 9, the action of the two portions of the current will conspire to give the same new position of equilibrium, and that the twisting effect will be doubled, and it is evident, that if the same current were again brought round, a fourfold effect might be produced, so that a weak current might be made by this principle of *multiplication* to produce almost any amount of action, and that this might be used as an index of the comparative strengths of different currents, which is accordingly taken advantage of, in the instrument called a galvanometer. If instead of placing the current above or below the needle, it be placed alongside of it, and in the same direction as in fig. 10, it will be seen, that the same tendency to place itself across the direction of the current, will still be manifested by the one pole rising and the other being depressed; and if the needle were only moveable in a vertical plane it would finish, if the current were strong enough, by placing itself vertically across the horizontal wire, but in the case of a magnet suspended on a point, this is not what occurs, for no sooner in the last mentioned arrange-

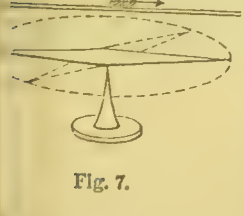


Fig. 7.

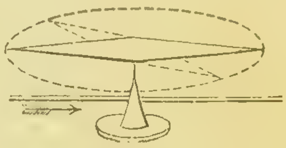


Fig. 8.

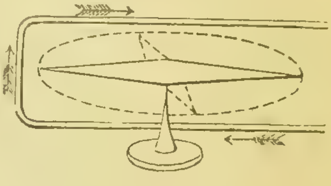


Fig. 9.

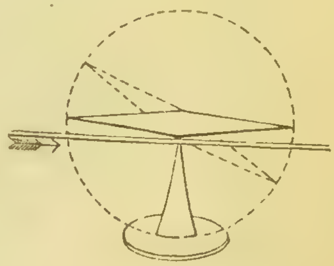


Fig. 10.

ment does the one pole rise above the current and the other pass below its level, than they immediately begin to move horizontally, and tend to twist the magnet spirally on the wire. By a careful inspection of the appearances, this can readily be noticed. After much difficulty, when these observations were first made, it became apparent that the influence, whatever it might be, tended to propel each pole of the magnet continually round the line of the current, but the two in opposite directions, (fig. 11.) It is clear, unless the needle were capable of being bent like a thread, the two poles could not continuously obey the forces tending respectively to twist them in opposite directions round the wire, and that the only way in which they can manifest their *tendency* is, by taking up the cross position which we have seen the needle to assume.

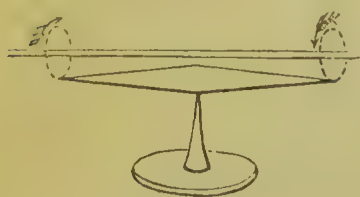


Fig. 11.

If one pole of the magnet could be liberated from the other, a continued circulation round the wire might be expected. This cannot be done, but the other may be rendered inactive by preventing the current from acting on it by leading it away from the needle after it has passed along only one of its ends. Were we to use a bent wire for this purpose, it is evident that the bent portion would interfere with the motion of the magnetic pole round the remainder of the wire. The difficulty, however, has been overcome by substituting a liquid conductor instead of a wire for the bent portion of the circuit. The magnet is bent as is represented in the cut so as to enable it to be supported in a vertical position on a pivot. It carries a small cup of mercury on its upper surface, into which the point of one of the battery wires dips, transmitting the current in a parallel direction near one of the halves of the magnet; it is then carried along a bent wire which dips into a circular channel of mercury, whence it passes by the small mercurial cup on the right away to the other wire of the battery, thus having only passed along one of the halves of the magnet, the other half having no current remains indifferent, and easily follows round in a continuous circuit after the other, as its opposite rotation is not called into action. Thus the remarkable spectacle is presented of a body continuously whirled round a fixed line by what appears to be a single force derived from that line in a direction always along the tangent to the circles described round it as a centre, (fig. 13.) We shall afterward see how this apparent physical anomaly has been reconciled with the ordinary laws of mechanics.

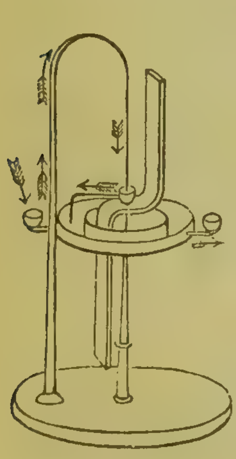


Fig. 12.

Fig. 13.

As may easily be supposed, from the universal laws of the equality of action and reaction, the wire is affected by an equal tendency to pass across the magnet, and this may be rendered evident by suspending a wire in the manner indicated in the woodcut where the ends dip into mercurial cups A B, in metallic communication with other cups into which the wires of the battery are introduced, giving us a wire carrying a current and yet being free to turn itself on the pivot at B. If a magnetic bar be placed over B and parallel to the wire, it (the wire) will turn itself round to a direction at right angles, when it again stands stationary till the magnet is again moved or the current reversed, when a new rectangular position is assumed. By presenting a single magnetic pole to the wire in different positions it becomes evident from the directions of the attractions and repulsions that the real tendency is to rotate in a certain definite direction round the magnet, and that, if perfect freedom of motion were allowed, this result would be produced. Practically this is easy of actual accomplishment by means of even a current from a comparatively small battery, by an arrangement similar to that delineated in fig. 15. The current is made to descend from the cup P, through the moveable wire W, which dips into the circular basin of mercury below, from which a wire carries it to the cup at Q, into which the wire from the opposite pole of the battery is made to dip. A magnet is bent to the form shown in the figure, and has one of its poles introduced through the bottom of the basin of mercury, while the other is placed at a distance, as at N, so as to remove its action on the wire. If the connections be complete and the current sufficiently powerful the wire performs continued

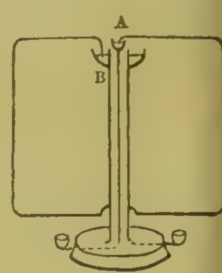


Fig. 14.

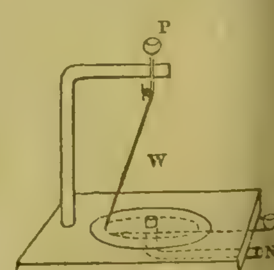


Fig. 15.

volutions round the pole s, in the same way as e magnetic pole itself did in the former experiments when the wire was fixed and the magnet ee to move. Many curious varieties of these ectro-magnetic rotatory apparatus have been different times invented, one of the simplest which is that where the battery itself is made revolve by the influence transmitted from a agnetic pole placed in its interior, as repreented, and round which it is free to turn. The gure represents a section of such an arrangement of about half the working size. The copper portion of the bottom pair is formed, on the principle of Hart's battery, to contain the acid, and to leave a free space in the interior, up which the magnet is passed, upon which the copper hangs by means of a pivot attached to an arch of wire, as shown at C. The zinc is also formed into a cylinder, and is suspended free to turn in the acid by means of a pivot resting on the wire of the copper and in clean metallic contact with it, so as to allow of the passage of the current. It is evident that the current in ssing from the copper to the zinc ascends in first metal and descends in the other, so t opposite currents equal in quantity and ergy are flowing parallel to the magnetic . So they ought to be impelled in contrary ections, which is really produced, and forms once one of the most striking and at the me time most certain (so far as freedom from bility to failure is concerned) exhibitions of actions of electro-magnetic rotatory forces. e Earth being a magnet, it might be conctured from what has gone before, that a mutual influence must be exerted between the earth and a wire carrying a current, and that this is the case can be rendered manifest by means of the rectangularly bent and freely moveable wire used for examining the action of a common magnet, for no sooner is the current sent through that tangle than it turns itself as if a magnet had n presented to it, and arranges itself at rest pendicular to the direction of the compass dle: and further than this, Faraday has succded in devising an arrangement by which a wire n be caused when carrying an energetic electric current to make a complete and continuous revolution by means of terrestrial magnetism alone. A slender copper wire is fixed to a hook and rendered buoyant by a small cork ball at the lower end, which dips into a basin of mercury placed in connection with the opposite pole of the battery. When the current is sufficiently powerful the wire performs a conical revolution round the lines of magnetic force, which are in our latitude inclined at an angle of 70°. At the equator, where the lines of force are horizontal, this experiment could not in this way be made. This arrangement is shown in fig. 17.

Fig. 16.


Fig. 17.

The inductive effects of the electric current may be next noticed. These are of two kinds—1st, the induction of other electric currents in conducting bodies, that is, bodies capable of conducting electricity, in their neighbourhood; and 2d, the induction of magnetism in iron and other bodies capable of taking on magnetic polarity. The second, viz., the induction of magnetism in iron, is treated of in the article ELECTRO-MAGNET, and need not be here further alluded to. The induction of electric currents in neighbouring conductors is well illustrated in an experiment of the following kind. Let two coils of insulated copper wire (that is copper wire covered with silk or cotton thread to prevent metallic contact with other portions which may touch any particular part) be taken, and one connected with a galvanometer, while the other has one of its ends, as at N in the figure, connected with one of the poles of a battery; and is placed parallel to and near the first coil, but not in metallic contact with it. The galvanometer being properly arranged, as in fig. 18, so that the needle when at rest stands

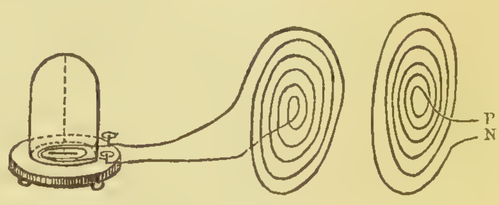


Fig. 18.

parallel to the wires which surround it, let the other end, P, of the coil be connected with the remaining pole of the battery, it will be seen that on the instant of completing the circuit in the first coil, the needles of the galvanometer are moved, indicating a current in the second coil, though no such current can have come directly from the battery, as no metallic or conducting channel exists between them. It will also be noticed that after oscillating for a while the galvanometer needles will return to their first position, indicating the cessation of the current in its wire. Let now the connection with the battery be broken, and it will be found that the needles are again moved by a sudden current in their wire, which, however, like the first, ceases immediately.

If the connection be again made with the battery another momentary current is indicated by the galvanometer and so on, and it is also noticed that the current which occurs for a moment in the second when the primary current is established in the first coil, is in a direction contrary to the course of the current in that coil, and that the one produced on breaking the circuit is in the same direction as the primary current. These are called momentary, secondary, or inductive currents. That they are of high intensity, nay, of far higher intensity than the primary current from the battery, can be easily perceived by the influence on the senses on applying the ends of the wires of the two coils respectively to the tongue or to the moist hands merely if the coils be long. Again, if the two coils be brought near to each other and the current let on to the primary, as has been said, the induced current in the secondary wire will only continue for a moment, and after a few oscillations the needle will return again to its zero point. While this is the case, let the secondary coil be rapidly moved away from the primary, which is still carrying the battery current, and instantly the needles will move indicating that the motion of the wire in the neighbourhood of the current has served to excite a current in the secondary, which ceases on the coil being allowed to rest for a little and is again roused up by even the slightest movement. It is more energetic the more active the movements, especially of approach and recession, are of the two coils from each other, a contrary current being produced by approach to that developed by recession.—If a third coil be added in the neighbourhood of the second, connected also with another galvanometer, we should expect to have two currents in the tertiary every time that the current in the primary is made or broken; for, as the making of the current in the primary gives rise to a momentary current in the secondary, which, short in duration though it be, must have both a beginning and an end; each of these we might expect would excite a current in the tertiary wire, which two currents ought to be in contrary direction and the one closely following the other; or perhaps they might neutralize each other's effects, and no current might take place. Neither of these, however, occurs, for only one current is observed in the tertiary wire at the time of making or breaking the current in the primary, and the current is sufficiently strong to show that neutralization by means of equal and opposite currents at the same instant by no means occurs in the tertiary wire. To account for this, it has been conjectured by recent experimenters that for some reason the current in one direction is much stronger than that in the contrary direction, and it is the effect of it alone that is observed.—Recently, series of five or six coils have been experimented on, so as to discover the kind of inductive currents they give rise to in each other, when mutually excited, and the influence of the primary current transmitted along from one to the other. Those who are interested in such researches, will find in the published papers of Dove, Weber, and Abria, the information they desire. It ought to be understood that the form of a coil is in no way necessary to the development of electrical currents in one conductor, at the moment when another conductor in the neighbourhood has a current sent through it. All that is necessary, is only that the two conductors should be in each other's vicinity, and if they be wires, that they be not perpendicular the one to the other. The action of many turns of a coil only multiplies the effect which would result from the length of wire of one turn. As to the cause of these secondary currents, the most probable supposition that has been made is, that they result from the induction arising from the wave of electric tension which rushes along the wire, when the battery connection is first established, acting on the neutral electricity of this conductor, repelling one portion and attracting the opposite kind, and thus disturbing the electric repose, and causing a momentary rush in opposite directions of the two electricities, just as occurs in a body excited by the inductive action of another excited by frictional electricity, where a current of electricity occurs between different parts of the body till the new state of equilibrium is attained, when repose again ensues, until the inductive body is withdrawn, giving rise to the currents due to the return of matters to their original state. It will easily be supposed that the currents induced in one conductor by the presence of another carrying a current will be greater, as the two conductors are more closely approximated. Accordingly, one of the best as well as most convenient modes of arrangement for procuring these induced currents, is to coil the first wire on a reel or bobbin in two or three layers, and then over these wrap on the secondary coil, as is represented in the annexed cut. As has been said, the secondary currents are much more energetic, though very short in duration, than the primary current from the battery, and for some purposes, where continuity of action is not required, and only great intensity is chiefly desirable, as for many medical purposes, they are useful and efficient, and exceedingly easily procured, compared with the costly and ponderous batteries which would be requisite to produce similar currents by the more ordinary voltaic arrangements. To construct a coil capable of giving currents powerful enough for medical purposes, all that is required is, to wind on a wooden bobbin with a hollow axis about one inch in diameter, about thirty yards of common bell copper wire, covered with cotton or silk thread, leaving its terminations

Fig. 19.

›e to be connected with the battery. Above is, let about four hundred yards of fine copper ›re about $\frac{1}{50}$ of an inch in diameter, also in-lated, be wound on in successive coils, leaving e ends free, and attaching to them pieces of tin, brass tube, to act as conductors. If now small battery, or even a single voltaic pair four or five inches square, be connected with e ends of the thick wire, and the human hands oistened with water, be placed on the conducting bes of the small wire, a shock will be felt each ne that the connection with the battery is made broken. It will also be found that a piece of ft iron introduced into the interior of the coils, ll greatly increase the intensity of the shock, d still more will this be the case, if a faggot of ›n wires be used for this purpose. In order to ›ke and break the connection with the battery, as to give a rapidly intermitting current in the ›mary wire, a simple automatic apparatus is ›resented in the figure. S is a plate of soft iron attached to a piece of watch spring, fastened at the opposite end to a stud rising from the bobbin, and having one pole of the battery in connection with it. On the lower surface of the

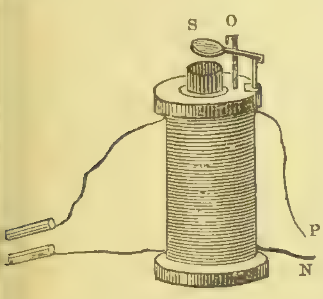


Fig. 20.

tch spring is soldered a small slip of platinum, ich when S is not dragged down on the faggot wires by their attraction, rests against a tinum wire at O, in connection with one end the primary coil. The other end of the pri-ry coil is put in communication with the ›aining pole of the battery, so that when the es of the battery are attached to N and P, current passes round the primary coil, and gnetizes the iron wires, which instantly drag vn S, separate the platinum surfaces, thus in-rupting the primary current and causing a ect current in the secondary wire, the reverse ›rent having been induced when the connection s completed. The plate S being no longer racted, is lifted by the spring into contact with platinum point, and again establishes the ›rent, causing the inverse secondary induction, l also magnetizing the faggot of wires, which tantly again draws down S and interrupts the ›nection, giving a rapid succession of momentary ›rents. The platinum surfaces are necessary, order to maintain unoxidated points of contact. has been found, that tubes of different non-gnetic metals introduced into the interior of bobbin, altogether intercept the effect pro-›ced by the iron placed in their interior, and er considerable difficulty, this curious effect has been traced to the development of circular currents in the substance of the tube, by the inductive action which is the subject of which we are now treating, and that these currents re-act injuriously on the secondary wire, as they are in the opposite direction to the primary current in the coil, and tend to neutralize its effects on the secondary wire. That this is the true cause, is proved by dividing the tube by a longitudinal slit from end to end, which totally prevents its screening action. The superficial currents caused in the same way on a rod of iron introduced into the coil, also act injuriously, and accordingly it has been found better to employ wires, and still better to use them covered with thread to prevent mutual communication. As to the explanation of this increase of the secondary currents, by the introduction of iron into the coils, while other metals have no such effects, the reader is referred to a further stage of this article, when the object of magneto-electricity is treated of. This circumstance of the induction of a secondary current of electricity in neighbouring bodies, explains the fact so often observed, that, though by simply joining the wires of a small galvanic pair, no spark is produced, yet, if a coil of wire be made part of the current, a vivid spark appears on making and breaking the connections. It may be supposed that this was owing to the wire acting as a reservoir, but the observation, that the same increase of effect does not take place if the wire is merely extended, and that the form of a closely folded coil is necessary, points to the true action which gives rise to the momentary movement of a large quantity of electricity, as the reaction by induction of one layer of the coil over another, according to the principles just laid down. A piece of iron introduced into the coil of wire also in this case, increases the intensity of the light. It will be remembered, that the secondary currents is in the reverse direction to the original current when the current is made, and in the same direction as the current when the connection is broken, so that in the case of the action of one part of the primary wire acting on another part, we would expect that the induced current would tend to oppose the primary on making the connection, and would assist or enforce it, by moving in the same direction when contact with the battery is broken, and this is found to be the case, for it is with the latter operation that the most vivid spark is seen. It is observed in the construction of coil machines for medical purposes, that the character of the shock is influenced by the thickness of the wire used as the secondary coil. M. Ruhmkorff of Paris, by using a condenser composed of oil-silk coated on each side with tin foil, has constructed induction coils of very great power.

One of the most remarkable discoveries of modern times, is that due to Faraday, of what is called *magneto-electric induction* It can be most easily observed, by connecting the two

ends of a coil of insulated copper wire with a galvanometer, placed at a distance of a yard or two, and introducing suddenly into the interior of the coil the end of a magnetic bar; a current will instantly be indicated in the wire, which, however, will immediately cease if the magnet be allowed to rest in the coil, the galvanometer needle returning to zero. If then the magnet be

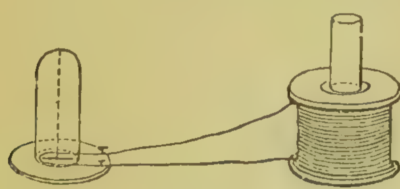


Fig. 21.

withdrawn from the coil, another momentary current in a reverse direction will be exhibited by the galvanometer. The intensity of these induced currents varies as the strength of the magnets, the length of the coil, and the rapidity with which the magnet is moved. It is easy to get currents intense enough to be felt as a severe shock through the human body. These currents, which are the effect of the reaction of the magnet on the wire, (as magnetism is produced by an electric current, so here a magnet produces electricity,) are most easily explained on the theory of Ampere, that a magnet consists of an arrangement of electric currents, making this case only a variety of the last considered, viz.:—the induction of one electric current by the sudden presence or removal of another in the neighbourhood of a conductor. Instead of producing magnetism in the interior of the coil of wire, by introducing suddenly a magnet, the same thing might be produced by temporarily magnetizing a bar of soft iron round which the coil had been wound, by bringing in contact with its end the pole of a magnet, and then withdrawing it. If the steel magnet be of the form of a horse-shoe, then the iron bar may be bent into a similar form, and have the wire wound in two coils for facility of arrangement, as represented in the annexed figure. If instead of removing the magnet, it be suddenly twisted half round on its axis A B, the induced magnetism in the iron will cease, and a current be produced in the coil. If the revolution be further continued till the plane of the two metals coincide, the polarity of the iron will be again produced, but in an opposite direction, giving rise to a contrary current and so on. In order that these currents be produced, it is necessary that the ends of the coil of wire should

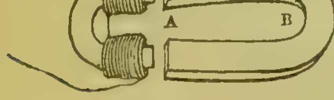


Fig. 22.

ELE

be joined by some good conductor, or form what is called a closed circuit. This is done by the galvanometer wire, in the case of experiments made with it to detect the currents. If, however, we wish to see the spark which they are capable of producing, the complete metallic circuit of the wire must be broken, and it is of importance that this should be done at the moment when the iron is being magnetized or unmagnetized, as it is then that the current exists. Many arrangements have been devised for rapidly whirling the iron with its coils in front of the magnet, and for breaking the continuity of the wire of the coil, at the proper parts of the revolution, that is when the iron within the coil is being magnetized, or unmagnetized. Fig. 23, represents one of the most

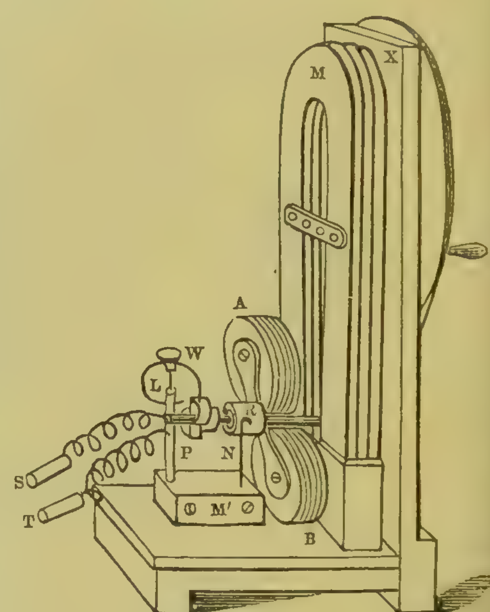


Fig. 23.

useful of these magneto-electric machines, as they are called. M is a powerful magnetic battery, consisting of three or four strong horse-shoe magnets, fastened to the upright board X, by a clamp which allows of adjustment that they may be brought more or less forward so as to be applied closely, but without contact, to the ends of the short soft iron bars enclosed in the coils A and B. These coils consist of copper wire covered with silk thread, and wound on the soft iron cores, which again are united so as to form a kind of horse-shoe, by means of the iron bar seen stretching from A to B. Through this bar there passes the axle which carries the coils and breaks, and is driven by the multiplying wheel at a high velocity. Near the point of the axle is fixed the break P, which is composed generally of brass, and has one of the ends of the coil in connection with it, while at the same time it is pressed on by the bent wire from the metallic pillar L. The pillar L is in connection with the metal of the support M',

›m which the wire N rises, and rests against the ıg R. This ring is separated from the axis by : insulating piece of ivory or dry wood, and has e other end of the coil soldered to it, so that e end of the coil being in communication with e axis, carrying the break and the other end .th this ring R, the current will be complete rough the wire N and P and W, between the ·o ends of the coil, when the wire W is in ntact with the break. From what has pre- led, it will be understood that, during one mplete revolution of the axis, the cores of the ıls being twice magnetized and twice demagnet- :d, there will be four momentary currents cited in the coils, and that two of these will be one direction, and two in the opposite. The ırk is most clearly seen, if the break be so ·anged, that the current is broken when the e joining the centre of the two coils is vertical. r chemical decomposition; it would be im- :tant, that only currents in one direction should employed. This will be accomplished by owing the current to be closed during one-half : revolution, thus confining the current to the ·ls themselves, and not permitting them to ›ss to the point of operation; but the question ses, during which half of the revolution the ne direction of the current will be maintained. ·w it is evident that as demagnetizing and :gnetizing by an opposite pole, both give a ·erse current, they will agree in direction among :mselves, so that as either coil passes from one e of the magnet towards the other, the currents y be used by breaking the connection, and ›wing them to pass to effect chemical action; : that the current must be closed while the l passes again from that pole and towards the ɛt, the current being then completed in the wire, ough the break. It is found, that coils formed thick wire, give bright sparks and currents ›able of producing heat and magnetism, ile very great lengths of exceedingly thin ·e produce much greater chemical power and more energetic shocks when the human hands applied to conductors in communication with ends of the coil. The first are called quantity, l the second intensity armatures. For the ·pose of communicating the shock or applying se currents medically, the brass conductors ınd T, are used and applied, as in the case :he galvanic current from an ordinary battery, ile the coils are driven round. The wires ached to these conductors pass the one to pillar while the other has its end pushed into a hole the end of the axis. The strength of the rent increases as the rapidity of revolution reases, agreeing with what was said, as to the :se of these currents being the induction pro- :ed by the sudden presence of the magnetic ɜ, or according to Ampere, the electric currents :hat pole. This magneto-electric machine is :eedingly useful in the medical application of electric current. It can be regulated in intensity with the greatest nicety, by applying a piece of iron across, between the limbs of the magnet, and sliding it more or less near the poles. It is always ready, night or day, and does not need the same amount of preparation as a battery, or even the ordinary coil machine, where, at least, a single voltaic pair is required. It has been most useful as a means of resuscitation in drowning, in threatened death from chloroform, and in many other similar cases. It has been proposed as a means of corporal punishment in the army, communicating pain without mutilation. It has been, and is still, used on a large scale in electro-metallurgy. It is in constant use as a substitute for batteries in the Magnetic Telegraph, and may be regarded as one of the most remarkable of human inventions.

As important both for their theoretical and practical bearings, it may be proper here to refer to the phenomena depending on magneto-electric induction, brought to light by the researches of Arago, Babbage, and Herschel. If a disc of any metal be placed over a freely moveable magnet, as in fig. 24, and set into circular motion, the needle will gradually begin to follow the motion of the disc, and if the rapidity be sufficiently increased and the plate be placed near enough, will ultimately revolve with greater and greater rapidity. That this effect is not due to the currents of air Arago proved by interposing a plate of paper or glass, and he conjectured that rotation was a source of magnetism or, at least, of electrical currents. Herschel and Babbage showed

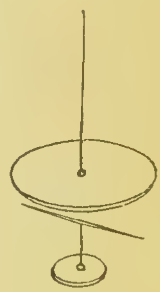


Fig. 24.

that if a number of slits or cuts in a radial direction are made in the disc of metal, the revolving motion is greatly enfeebled, and that different metals revolve with energy proportional to their conducting power. Faraday ultimately succeeded in proving that these phenomena were wholly due to the currents induced in a moving conductor by the presence of a magnet, in the way explained in the preceding paragraphs. As may be at once inferred from such experiments, caution must be exercised in drawing conclusions as to the direct magnetic or non-magnetic properties of bodies from their movements in the neighbourhood of magnets. The mode of oscillations must be used with suspicion only. There can be no doubt that metallic pendulums must be affected in the rapidity of their oscillations to some extent by the influences of terrestrial magnetism in the way here referred to. A copper ball suspended between the poles of a powerful electro-magnet can be easily made to turn itself rapidly round by magnetizing and unmagnetizing the iron, and it ought to be recollected that these motions may in many cases mask or modify the appearances presented in researches on the properties of the

magnetic field and on diamagnetism.—As a practical example of the manner in which Arago's discovery has been turned to account, the mode of reducing the oscillations and thus steadying the needles of the mariner's compass may be referred to. A ring of copper or other good conducting non-magnetic metal is placed round the compass box and as close to the needle as practicable consistent with free motion. The effect of this is that the oscillations are greatly reduced and the uncertainty in steering on an agitated sea considerably diminished. The theory of this action can be at once understood from what has already been mentioned: the needle tends to drag round the copper ring after it in its excursions, and as action and reaction are equal and opposite, the needle itself is acted on by a corresponding force dragging it back, so that rest, after any disturbance is much more rapidly produced than if no such action existed. Copper is used as the ring on account of its high conducting qualities, at the same time that it can be got pure and free from mixture with iron or other magnetic material which would act injuriously on the compass needle by producing contending polarities in its neighbourhood and interfere with the directive force of the earth in the manner known as local attraction.

We shall now give some account of the different suppositions that have been made as to the nature of the mutual influence which is exerted between an electric current and a magnet. Any general theory must account for such phenomena as the attraction of iron filings by a wire carrying a current, the magnetic properties of coiled currents, the directive power of a current on a magnet or a magnet on a current, and the continued rotation of a magnetic pole round a current and *vice versa*. The most obvious and the earliest supposition was, that, by the conflict of the two electricities within the wire, magnetic properties were acquired with the polar directions at right angles to the current, so that a section of the wire carrying a current might be assimilated to a series of small magnets laid round in a circle as in the annexed figure. A magnetic needle in the neighbourhood of this series would, from the known laws of magnetic action, be caused to place itself parallel and with opposite polar arrangement to the nearest magnet. To this it may be objected that the whole would be analogous to a closed magnetic circle or to a magnetized ring which would have no action on external magnets, and besides this objection no explanation is afforded by this hypothesis of the rotation of a magnetic pole round the wire. To remedy this Wollaston and others adopted the supposition that by the passage of the electricities in the wire, the two magnetic fluids that accompanied them were put into a spiral or vortigenous motion in opposite directions, and that they thus tended to draw along with them the magnetic poles. Thus the real force exerted on the magnet by the wire was in a circular direction round the course of the current or always in the direction of a tangent to the circle described round the wire, hence it was called a tangential force. The magnetic pole was thus driven round in the same way as a ball would be impelled by a revolving wheel if it were placed between its spokes. To account for these attractions and repulsions exerted between two currents it was supposed that magnetic fluids of the same name attracted each other if they moved in opposite directions, but repelled if they moved in the same direction, and thus by a highly artificial and cumbrous series of gratuitous suppositions most of the phenomena of electro-magnetism could be accounted for. It may be remarked, however, that there is no other instance among physical forces of the existence of a tangential force as the primitive influence exerted between two bodies, there being in all other cases an attraction or repulsion only along the straight line which joins the acting points. It is to the illustrious Frenchman, Ampere, that science is indebted for a theory at once simple and comprehensive, which, while it has served to render all the phenomena of electro-magnetism subject to rigid calculation, and is therefore in this respect far preferable to any of the others, yet demands no hypothesis so repugnant to the previously ascertained mechanical principles as those necessarily implied in the others. Ampere's theory, which has gained for its author the title of the Newton of electricity, now plays so important a part in physical science that it cannot here be passed over without an attempt to expound its nature and some of its consequences. Ampere discovered by experiment that electric currents attract and repel each other, and he makes this the foundation of his theory, assuming that round the constituent molecules of magnets there are perpetually circulating electric currents, and that the attractions and repulsions exerted between magnets and electric currents are due alone to the mutual actions between the currents, thus setting aside, *in toto*, the ordinary hypotheses of magnetic fluids and all tangential and transverse magnetic forces. He thus, successfully, by calculation and experiment, referred the whole class of electro-magnetic rotations, magnetic properties of coils, the inductive actions of magnets on neighbouring conductors, and even the magnetic properties of the earth itself, to one sole hypothesis and a fact well ascertained by experiment: the hypothesis being that a magnet consists of a series of perpetually circulating electric currents, and the fact, that electric currents attract or repel each other. The first point necessary for the establishment of this theory was to ascertain the laws of the force by which one electric current acts on another; or, in other words, to ascertain the form and the value of the constants in the expressions which

Fig. 25.

›resent the force with which under all circumnces of length, distance, intensity, shape, and sition, any one current acts on another. In ler to this it can readily be ascertained by ·ans of a wire carrying a current and rendered ›veable in any manner, as, for instance, in the y shown in the figure, that if another current brought parallel to it they will repel each other if they be moving in the same direction, but attract if they move in the opposite directions; and, moreover, that the attraction and repulsion is less as the angle contained between the positions of the wires increases till when they are at right angles it ceases, as is evident from the fact that it must change from at-

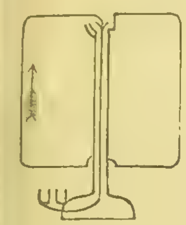


Fig. 26.

ction to repulsion or *vice versa*, so that in › position the function which represents the :tual action must be zero. It may also be umed that the action of each current is pro-.tional to its intensity, and consequently when se two act on each other the mutual in-·nce will be proportional to the product of se intensities. It is evident that the force l vary with the distance between the acting nts of the currents, and therefore it will impossible to obtain a general expression for › mutual influence for any but small portions :urrents, so small that their length may be re-ded as evanescent compared with their mutual ance. It is impossible to experiment with ·ts of currents answering these conditions and .he same time excluding the action of the re-inder of the currents; but it may be assumed t the action of *elements* or currents so small t the distance of all their parts may be reck-d constant and designated by d, varies in-sely as d^2. This is more particularly probable :e, if it be the case, we know from the inves-itions of Laplace and Biot that if each element ın indefinite rectilineal current act on a point its neighbourhood with a force varying in-:ely as the square of the distance, then the ›le line will act with a force varying inversely the simple distance merely, and experiments h currents can be made in circumstances ilar to this, and the results confirm the position. Also, the same law of force is ly deduced from the fact demonstrated by ›eriment that the intensities being equal, if length and number of the currents be pro-tional to the distance, the effect of any series l be the same on an element, or small portion a current: for instance, in fig. 27, C will be much affected by the single short current at as by the two doubly long currents at R, or four currents each four times as long at S, a tance four times as great as the single current at P, the unity of distance, and it is easy to show that according to no other law than the inverse square would this follow. For, representing by n the undetermined power of the distance according to which the force in each element acts, then we should have the force at any distance, d represented by an expression proportional to d^2, because the length of each current increases as d, and the number of currents also increase as d, so, for these two reasons we should have d^2 as the law of the variation of the

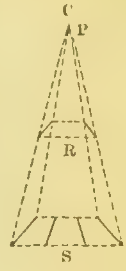


Fig. 27.

inherent strength of the force in such an arrangement as that in the figure, but, by supposition, its action on m is to be proportional to $\frac{1}{d^n}$ so its total action will be represented by $\frac{d^2}{d^n}$ and this at the unity of distance becomes merely unity. But by experiment, this is the same at all distances, so we have $\frac{d^2}{d} = 1$ or $d^2 = d^n$ or $n = 2$, proving that the inverse square is the true law, according to which the action of the force varies with the distance in the case of elementary portions of currents acting on each other. This reasoning may appear to be vitiated by being founded on the case of currents of finite length, but the reader will easily perceive that this is not the fact, as integration would give the same result. In attempting to construct an expression which shall embody what has preceded into a general formula for calculation, difficulty was experienced, from the circumstance that the influence which extends between the elements of two electrical currents, varies not only with their distance and mutual position, but also with the relative directions of the electrical currents with reference to the line joining their centres. The simplest case of this kind is where the currents are parallel to each other, and perpendicular to the line joining their centres, as in fig. 28, in which case they attract or repel each other with the maximum force. In

Fig. 28.

the case, however, that either or both the currents should be oblique to the line joining their centres, as seen in the position of B′ in fig. 29, we know that the maximum force of A′ is exerted on B′ along the line joining their centres, but experiment proves that the maximum force of B′ is not exerted along this line, but along a line perpendicular to itself, as along S P, and we wish to know how much of that force is really

Fig. 29.

exerted along oblique lines joining the centres of the two elements. To this Ampere was assisted by the results of experiments which prove that a twisted or sinuous current, of any form, exercises the same action on another current, as a straight current terminated at the same points would do. In the figure, the effect of the current passing along the straight and twisted wire would be null, and thus it was easy to see that the inclined elements might be replaced by two component currents, one in the direction of the line joining the centres of the two elements, and which, experiment proves, has no effect on that element, and another perpendicular to it, which has the full effect due to its shortened length. Thus, the single straight current C B″, fig. 31, is replaced by the two currents C D and D B″, which, experiment proves, would produce exactly the same effect, while it, at the same time, proves that D B″ alone would exercise no effect on a current in the direction of its own length, or, at least, none of which we need at present take account, and so it may be neglected, and the full effect deduced from C D. But, as the effects of C D and C B″ will be to each other as their lengths, and these are as the lines C D and C B″; and C D = C B″ sin. D B″ C, that is, the force in the direction of the line joining the centres is equal to the whole force of the element multiplied by the sine of the angle which its direction makes with that line. The same result must follow if the other elements be also oblique to this line.—If e denote the length of the one element, and i its intensity, e' and i' representing the same for the other element, while d represents the distance of their centres and θ, θ' the angles which these directions respectively make with the line joining their centres, then, because the mutual action of two forces is equal to the product of each separate action, we should have, by what has preceded

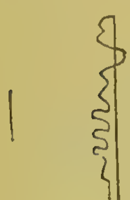


Fig. 30.

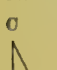


Fig. 31.

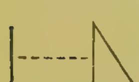


Fig. 32.

$$f = \frac{e\, i \sin.\ \theta\ e'\ i' \sin.\ \theta'}{d^2}$$

or

$$f = \frac{e\, e'\, i\, i' \sin.\ \theta \sin.\ \theta'}{d^2}$$

But this, be it observed, only applies to the case of the two elements of currents being in the same plane, and in order to render it applicable to every position, we must decompose one of the forces into two components, one of them parallel to the plane of the other element, and another perpendicular to that plane, which last, as it exercises in general little effect, may till afterwards be neglected. The first component, viz. that parallel to the plane of the other element, may be got in the way which may be understood from fig. 33, where P Q R S represents a plane passing through one of the elements and through the line which joins the centres of the two, and T U V W is a plane passing through the other element and the line which joins them; then A L exhibits the one element, and B C the one in the other plane. If C D be projected on the plane P Q R S, B D may represent this projection, and if ϕ denote the angle contained between these two planes, then B C cos. ϕ will be its component in the other plane, which would then, were it not perpendicular to the line joining the centre, be affected by the sine of the angle which it makes with it as has already been indicated; so we arrive at the general form for all possible positions of the two elements whether in the same plane or not

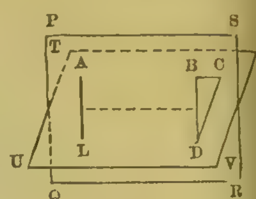


Fig. 33.

$$f = \frac{i\, i'\, e\, e' \sin.\ \theta \sin.\ \theta' \cos.\ \phi}{d^2}$$

It is evident by inspection of this expression, that the force vanishes where θ or θ' disappear, that is, when either of the elements becomes parallel to the line joining their centres, and this, whatever may be the direction of the other element. Now, experiment proves that this is not the fact except in the single case when the other element is at right angles to the line joining the centres, or in other words, when the two elements are at right angles, and at the same time the length of one of the elements is directly pointed at the centre of the other. It is easily proved by experiment that the parts of two currents, or two parts of the same current repel each other if they are in the same straight line: as, for instance, in fig. 34, where the two elements are in the position A and B″. Also, it appears that this repulsion does not in any degree exist, or at least, that it is at its minimum when the two elements are parallel, as at A and B. No indication of this is found in the expression for the force, which must, therefore, be imperfect. An additional term will remedy this if it be so constructed that it shall vanish when either of the two currents are perpendicular to the line joining their centres, and that it shall be a maximum when they both coincide with that line. Now, the cosines of the angles θ and θ', have these qualities, and it is probable that this term ought to be some function of these

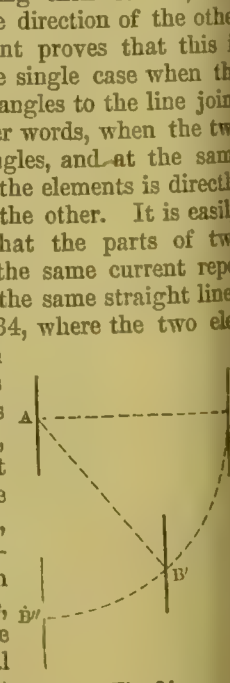


Fig. 34.

sines. For the purpose of ascertaining the ıe form and constants of this term, Ampere opted the product of those cosines affected with undetermined coefficient, which we may note by m, and afterwards calculated, by inte- ating the expression, the rotatory action exerted r a closed current on an element of another cular conductor, and, as experiment proves at action to be null, he equated this result to ro, and thus gets

$$2 + 2\,m - 1 = o$$
$$m = -\tfrac{1}{2}.$$

r substituting this value for m, we get as the nplete expression of the attraction between o elements of electric currents in any direc- n, and of any intensity—

$$= \frac{i\,i'\,e\,e'}{d^2}\ (\sin.\theta,\ \sin.\theta',\ \cos.\varphi - \tfrac{1}{2}\cos.\theta,\ \cos.\theta'.)$$

hen calculation is to be made of the attraction finite lengths of current, integration must be orted to, in which case, if l and l' be the gths of currents, $d\,l$ and $d\,l'$ would be sub- uted for e and e'.—A multitude of results have en deduced from this expression by Ampere, vary, and more recently by Plana. We can ; briefly point out some of its consequences. must be observed, that in order that the ex- ssion may include the property of electric rents attracting each other when in the same ection, and repelling when in opposite, it is essary to adopt a corresponding convention h reference to the angles θ and θ', in order t in the proper circumstances these sines may negative or positive, so as that the expression y indicate attraction when positive, or repul- ı when negative. This will be accomplished he angle θ be always measured between the t of the current produced in its own direction : the part of the straight line joining the ntres of the two elements produced; thus, with ard to the two elements B E and A K, fig. 35, going in the direction of the arrows we take as the angles θ and θ', E D F and A C P; then those on the upper and right side of the joining line will be reckoned positive, and those on the left and lower side negative.

Fig. 35.

One of the most interesting results of Ampere's investigation was the explanation r afforded of the rotation of a wire round ıagnet, or of a magnet round a wire. This once follows from the general expression, if admit that a magnet is composed of circular ents circulating round the molecules, and nged in planes perpendicular to the magnetic , as may be represented in section in fig. 36. an be shown that, as the contiguous portions he elementary molecular currents being in opposite directions, will oppose each other, and produce a null effect, the whole system will be merely equivalent to an external current circulating in the plane of the section, and in the same direction as the molecular currents; so a magnet, in this theory, is represented by a contiguous series of closed currents arranged perpendicular to its axis, and of the same diameter as the magnet itself.

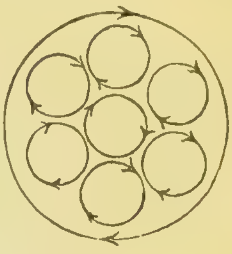


Fig. 36.

Such an arrangement Ampere designates as a solenoid, and shows by calculation that it may in all cases take the place of a magnet. In this way, we, in attempting to account for the rotation of a wire round a magnet, substitute for the magnetic pole a circular current of the same diameter. Let us represent a small portion of this current by the straight line A E, fig. 37, and an element of the current which is to be made to rotate round it by L, then we find, by the formula, with regard to any element of the current to the right of L, as for instance, B, that its action on L is repulsive, for, θ, the angle at L, is negative, and the angle θ', at B, is positive, hence sin. θ is negative, and sin. θ' positive, and when substituted in the formula they give a negative attraction or a repulsion, by which the element L would be impelled in a direction from right to left by B. It is clear that the same result must follow for all the elements of the current to the right of L. But any element, as A on the left of L, will give attraction; the angle θ, at L, being negative, its sine is negative; and the angle θ', at A, being also negative, these two negatives being multiplied together, would give a positive result when substituted in the expression for f, which is therefore positive. As in each case the angle φ, may be neglected, so in this one, L would be drawn towards A, which attraction would therefore conspire with the repulsion from B, and give a tendency to move in a direction against the current in A and E. As this would be the result for every element of these currents when integration was applied, the whole actions would conspire to produce a continued rotation round the magnetic pole, and thus we get altogether rid of the improbable supposition of a tangential force, by the substitution of a mere attraction or repulsion. A little consideration will show that if the current were fixed, and the magnet moveable, the same attraction and repulsion, action and reaction being equal, would cause the magnet to turn on its axis, or, if

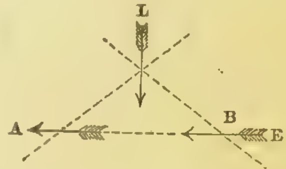


Fig. 37.

proper arrangements were made, to revolve round the wire. Exact calculation by means of the general expression for the action of magnets on each other, on the supposition that they are solenoids, has led to results which are in perfect conformity with the known magnetic laws. For instance, Ampere has proved that the total action of two solenoids upon each other may be represented by a force emanating from each end of the one towards each end of the other, viz. an attraction and repulsion, as is shown in fig. 38 —the attractions, by the continuous, and the repulsions by the interrupted lines, and that each of these forces acts according to the inverse square of the distance. This is exactly the case of magnetic bars, so the magnets act at least as if they were solenoids.—

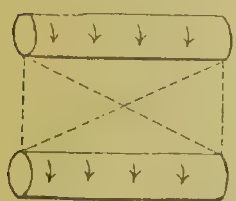


Fig. 38.

According to Ampere's view, terrestrial magnetism is nothing more than the effect of electric currents circulating continually from east to west round the earth, which is by no means impossible. Faraday's discovery of the induction of electric currents by magnets moving in the neighbourhood of other bodies, is thus also simplified, and referred to the induction of electricity by the electric currents in the magnet. Recently, different attempts have been made to modify the hypothesis of Ampere, in so far as the nature and modes of circulation of the electric currents round the elementary molecules is concerned. Weber, De la Rive, and others, have been engaged in such speculations in connection with the subject of diamagnetism, and Tyndal's researches have thrown doubts not only on this but on all existing theories of magnetism.

Electrolysis = *Electro-chemical decomposition:* a term of Faraday's.

Electrolyte. A body that is directly decomposed by the electric current, such as *water*—is named by Mr. Faraday, an electrolyte.

Electro-Magnet. A current of electricity itself possesses magnetic properties, as evinced by the attraction of iron filings toward the conducting wire of a voltaic battery. If the wire be coiled up into a flat spiral, and suspended so as to allow of its freely turning on a vertical axis while it carries the current, it will arrange itself like a magnetic needle, and exhibit the two opposite magnetic polarities on its two faces, being attracted or repelled by the ordinary magnetic poles, and may therefore be called a true electro-magnet. If the conducting wire be coiled into a long spiral or cylindrical form, as in fig. 1, a still closer approximation to the characters of a magnetic bar will be produced with the opposite poles at the two ends, the intensity of the magnetic power becoming greater as the strength of the voltaic current increases, and also as the number of coils is augmented. Such coils or helices turn themselves, when free to move, into the direction of the magnetic meridian; and a small floating battery of a single pair of copper and zinc plates placed in a glass tube containing diluted acid, and supported by a cork float with a coil of wire soldered to the plates, as in fig. 2, may be substituted when set to float in a vessel of water, for the magnetic needle, answering all the purposes of a compass without any of the ordinary magnetism, and might therefore be called an electro-magnetic compass. If into the interior of a coil of wire carrying a current there be introduced a bar of hardened steel, it will instantly be converted into a magnet, and if the current be intense enough, it will be magnetized to saturation, and thus excellent permanent magnets can be most expeditiously made; but it is upon soft iron that the magnetizing powers of the voltaic current are most powerfully exercised. Thus, if a copper wire be coiled round a piece of soft iron, and a current sent through it, the iron will instantly acquire exceedingly energetic magnetic properties, and indeed this is the mode in which by far the most powerful magnets can be constructed. A bar of well annealed soft iron, 6 inches long by 1 inch in diameter, having about 30 or 40 feet of copper bell-wire covered by silk or cotton thread, wound round it in close coils from end to end, becomes, when carrying the current from a battery of three or four pairs of 4-inch-square plates, an exceedingly powerful magnet capable of magnetizing compass needles to saturation, and exhibiting in a striking way most of the effects of magnetism. When very great power is required, the form of a horse-shoe, or of the letter U, is generally given to the iron bar. Care must be given to the selection of the best and softest iron. The copper wire should be thickish, say about No. 12, and should not be strained or twisted, lest its conducting powers be injured. It is generally wound on in several lengths, the separate ends being soldered to two different thick wires, for connection with the battery, taking care to wind on all the coils in


Fig. 1.

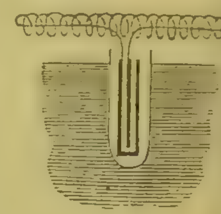


Fig. 2.

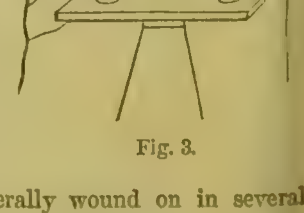


Fig. 3.

the same direction, and to solder the beginnings of all of them to one wire, while the ends are soldered to another, as in fig. 3. The electro-magnet belonging to the Faculty of Sciences of Paris is capable of lifting a weight of three tons when an energetic current is sent through its 20,000 feet of copper wire. Electro-magnets are now generally used for magnetizing compass needles and also the long and powerful correcting magnets for iron vessels. For experiments on the magnetic field and researches on diamagnetism and magne-crystallization, electro-magnets are nearly exclusively used, not only because of their great power, but also because of the facility with which, by discontinuing the current, or reversing its direction, the magnetism can be produced, or the poles reversed. For convenience, the annexed form is sometimes given to them, and sliding pieces of soft iron, called pointed armatures, for approximating the poles, are used where great concentration is required, and a field of very variable force. The electro-magnet is also extensively used in telegraphy. Electro-magnets have likewise been applied as prime movers of machinery, but not hitherto economically; a circumstance arising from two causes: 1st, while coal, the material used in the production of steam, is found in a state of nature fit for use, the metals and acids from which electric currents are evoked, require expensive and troublesome processes for their preparation; 2d, while it is true that a vast statical pressure is exerted by the magnetic force in keeping the armature in contact with the face of the magnet; yet, on removing the armature, even for a short distance, on account of the rapid decrease of the attractive force, a very great diminution in the effort which pulls the armature back again is experienced. Two or three different modes have been adopted to convert the attraction of these powerful magnets into mechanical action. One of the earliest, is, that of the Rev. Mr. M'Gauley, a modification of which is presented in the annexed figure.

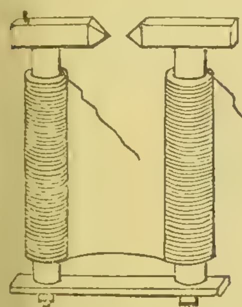


Fig. 4.

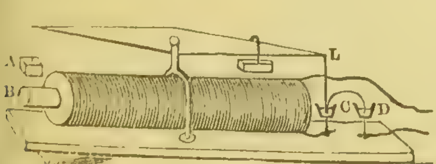


Fig. 5.

The bar of iron B, in the interior of the coil of wire, is rendered magnetic by the passage of the current, and attracts the piece of iron A, and pulls it down to contact with B, thereby elevating the opposite end of the lever at L, and lifting the bent wire which connects the cups C and D out of the mercury, thereby interrupting the circuit, and thus, destroying the magnetism, and allowing the iron, A, to separate from B, and the lever to resume its former position, which restores the connection of the coil with the battery, and renews the magnetism. The apparatus is thus automatic, and continues to work the lever with a reciprocating motion similar to the motion of the working beam of a steam engine. M. Froment, the philosophical instrument maker of Paris, has several machines of a kind similar to the one just described, at work in his machine shops. In other cases, a bar of soft iron is supported on a vertical axle between the poles of an electro-magnet. The axle carries a

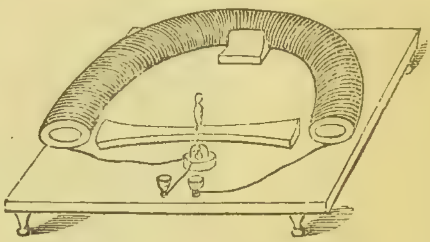


Fig. 6.

piece of copper wire of the form of a fork, which dips into a small box, divided into four compartments, two of which, opposite each other, are filled with mercury, into one of which one end of the wire of the coil is dipped, and into the other, the wire from the battery, the other end of the coil being put into permanent connection with the remaining wire of the battery. The mercurial cistern is so fixed that when the iron armature is in the line joining the poles, as is represented in the figure, the forked copper wire on the axle is over the two empty divisions, and continues over them till the armature is turned to a position at right angles to its present one. Then the copper fork comes in contact with the mercury in the opposite compartments, thus transmitting the current from one to the other, and allowing it to pass through the coil and magnetize the iron, which that instant attracts the armature round by both the extremities, closely approximating it to the poles, when the battery connection ceases, and the momentum acquired carries on the armature in its wheeling motion, when again the battery connection is completed, and another impulse is given, till at last a very high velocity is attained, frequently rising to a speed of many thousand revolutions in a minute if no resistance is put on the machine in the way of work to be done. In some cases, many magnets are arranged in a circle, and the iron armatures made to revolve only for a small

part of the revolution under the influence of each when in its immediate neighbourhood where the force of attraction is strongest. This last was the arrangement in the experiment for propelling railway trains, which was, some years ago, made on the Edinburgh and Glasgow line. The inconvenience there experienced was, that, though the force was so great when the iron armature was opposite the magnets as to drag the axles from their places, and destroy the working arrangements, yet, at a short distance from those positions, it was insignificant. Professor Page has recently employed, for producing motion, a modification of an experiment many years ago made by Mrs. Sommerville. If an iron or steel wire be placed with one of its ends slightly entered into the interior of a coil of insulated wire, as in fig. 7, it will instantly be dragged in when a voltaic current is sent through the coil, and, it is said, that, if the current be sufficiently energetic, it will remain suspended in the air in the axis of the coil without visible support. Be this as it may, it is certain that it is dragged in with great force, if the current is of considerable energy. Page, in his machine, employs a steel magnet instead of the wire, and by using large coils and magnets excited by powerful batteries, he is able to lift heavy weights through many feet at each stroke of the machine. An apparatus, on this principle, is shown in fig. 8.

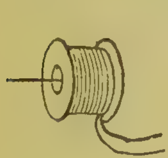


Fig. 7.

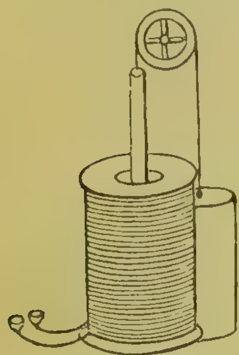


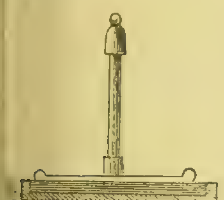
Fig. 8.

Electro-Magnetic Machines: *Mechanical Power developed by.* An *Electro-Magnet* being easily constructed, capable of sustaining a weight of enormous amount, considering the weight of the sustaining Magnet, and the expense of the battery; and it being easy to shift the attractive to a corresponding repulsive power of equal intensity (see Electro-Dynamics),—it is not wonderful that sanguine persons early imagined that a motive power had become attainable, uniting the qualities of extreme cheapness, of disposability, freedom from fracture or accident, and almost illimitable in intensity. The form or mode of application too, seemed palpable. Suppose an ordinary armature or set of armatures connected like the spokes of a carriage wheel, and made capable of revolving horizontally, around a point half way between the poles of a horse-shoe Electro-Magnet,—it is clear that the opposite ends of each armature would be attracted by the pole nearest it; and if that pole were suddenly changed into the reverse pole, the same armature would be as suddenly repelled. By attraction then, the spoke of the wheel would be drawn under the poles of the Electro-Magnet, and then as suddenly, and with equal force driven away: but as the *momentum* given by its approach would cause it to tend beyond the pole that attracted it, the substituted repulsion would drive it onwards in the same direction; so that a *rotatory motion* of the wheel would inevitably result. In this case there is no difficulty whatever in constructing the mechanism: a rotatory motion can be produced at once. See Electro-Magnet. And if this motion had any true or permanent momentum —if it represented in the strict sense a quantity of motion, we should have the element essential to any form of machine. Nor did interesting experiments fail to indicate an apparent possibility that effective mechanisms of this kind might be constructed:—wheels of such sort moving on pivots carefully freed from friction, were made to revolve rapidly: unfortunately a fact was overlooked, which quite incapacitated these rotations to overcome any resistance or do any work;— the power of a magnet to support a weight in contact with it, was confounded in all such expectations with its power to draw towards it a weight at a little distance. But these two powers are utterly different. The efficiency of magnetic attraction diminishes according to the law of the inverse square of the distances of the surface of the magnet and the armature; so that whatever the power to prevent separation when the two are in contact, their mechanical tendency to approach each other deserves little consideration. It is the latter, however, alone, that can induce *rotation*, or become a source of mechanical effect; so that there is in the very source of this new power, a cause of feebleness, over which no mechanical ingenuity can ever prevail. There are other obstacles in principle— such as the action of induced Electricity during the process; but as the foregoing is fatal, we shall not specify further. The leading Experimenter, or rather *Inventor*, on this subject was the *Petersburg* (not the *Berlin*) Jacobi; and certainly his experiments were conducted on a sufficiently boastful scale. But his submarine boat, before it could be persuaded to move required the propulsion of eight rowers; the full energy of one of these being consumed on the load or inertia of the Galvanic Battery. Luckily for the "Inventor," it is the policy of the Russian *Chancery*, that nothing undertaken by the Government shall fail; so that how foolish soever may have been the spectacle of his boat, Jacobi himself reaped consolation in the reward.

Electrometer; generally used indiscriminately with Electroscope, (*q.v.*) Accurately an *Electroscope*, *shows* the existence of a development of the electric attractions and repulsions an electrometer *measures* the intensity of the development. The best form of the electrometer in this latter and strict form, is Coulomb's torsion balance, (see art. in page 61;) in which if the

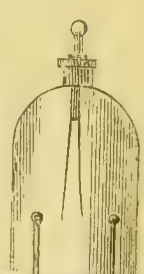
fixed ball be electrified the moveable ball diverges, and the angle of divergence can be measured. This instrument is so delicate, that a force equivalent to the *dead pull* of $\frac{1}{60000}$ of a grain is sufficient to make the needle traverse the whole circumference; consequently, an arc of one degree measured by its needle, indicates a force not greater than $\frac{1}{21600000}$ of a grain. The slightest trace of electricity may thus be discovered.—See ELECTROSCOPE, MULTIPLIER, and VOLTAMETER.

Electrophorus. An instrument that is often used instead of the electric machine. It consists essentially of two parts, a nonconducting cake and a conducting disc. The cake is of resin, and is contained in an envelope of wood or metal, into which it has been poured in a melted state so that it has a plane and smooth surface. The disc is formed of metal, or of wood covered with tin or metallic foil. Its diameter is less than that of the cake, and its edge is carefully rounded. To the disc is attached an insulating handle, as represented in the figure. To obtain charges from



the instrument we first excite the resin; and this is usually done by beating it with cats' fur, a highly positive electric. The disc is then placed upon the excited cake, and is touched with the finger. It is finally withdrawn by the insulating handle, and is found to be positively electrified. If the disc be now discharged, the experiment may be repeated without a new excitation of the resin; and this may be done hundreds of times in succession, and at intervals of hours or even days apart. When the charges obtained in the disc become too feeble, the efficacy of the instrument may be at once restored by a new excitation of the resin. The theory of this interesting apparatus may be very briefly stated. The development of electricity in the disc is evidently due to inductive action, for the charge obtained in the disc is positive while the originating charge in the resin is negative, and the former charge is obtained further by the uninsulation of the disc when in the immediate neighbourhood of the resin. We find accordingly, that if the disc is not touched by the finger or other conductor when it is placed on the cake, it possesses no charge when it is withdrawn, except a barely sensible charge of negative electricity derived by communication. The efficiency of the instrument depends greatly upon the fact, that when the disc is in position it is in circumstances highly favourable to intense inductive action. The whole of the lower surface of the disc is then in closest possible proximity to a well excited body, and the disc is uninsulated. It might be thought that in these circumstances the inductive action would necessarily terminate in discharge; but this result is prevented by the extraordinary power of the resin in retaining a charge once bestowed upon it. The retentive power of resin is otherwise proved by several striking experiments, but it is nowhere more clearly evidenced than in the action of the Electrophorus itself, especially in the permanence of its inductive power without new excitation.

Electroscope. In electrical experiment it is so frequently necessary to detect the presence of charges and to determine their species, that permanent instrumental arrangements of different kinds have been made for the purpose, and have found their way into every physical laboratory. Such instruments are called Electroscopes. Under all varieties of form they are constructed upon the common principle, that two charged bodies attract or repel one another according to definite laws. The simplest form of electroscope is the *electric pendulum*, a small and light conducting ball suspended say by a light nonconducting thread. When the ball is charged with a known electricity, it is in a condition to indicate by its movement the presence and species, and to some extent the intensity also, of the charge upon a body brought into its neighbourhood. Another simple form is the *electric needle*, a light nonconducting lever balanced in the manner of the mariner's compass, and bearing at one end a small body charged with a known electricity. Haüy's Electroscope is a simple and elegant instrument of this form, in which the charged body at the end of the lever is a small mass of a particular crystal that may be definitely electrified by pressure for an instant between the fingers, and that retains a charge so bestowed upon it for hours in succession. The instruments that are now generally formed for electroscopic purposes consist essentially of two small and very light conducting bodies that are placed in contact and are free to move to a distance from each other. When the two bodies are electrified, either by communication with the charge examined or by its inductive action, they give immediate signs of this effect by their mutual repulsion. The adjacent cut represents Bennet's gold leaf electroscope, one of the most useful apparatus of this kind, and one that is very generally employed.



In this instrument two thin gold leaves of sufficient length are fastened at their extremities between a pair of metallic pincers. The pincers are prolonged into a metallic stem which passes through an opening in the top of a glass case that envelops the whole apparatus; and the stem is terminated without by a knob of metal. When the leaves are electrified, they diverge to a greater or less extent according to the quantity of the charge. To prevent the contact of the leaves with the sides of the case, a result that would produce great inconvenience, in the working of the apparatus, two uninsulated

conducting balls are placed as in the figure, so that the leaves when greatly divergent come into contact with the balls and are discharged. To examine a body electrically by means of this instrument, we either place the body in contact with the external knob, or merely bring it close to the knob. In the former case the body shares its charge with the knob, and the leaves diverge in virtue of the electricity thus communicated; while in the latter case the given charge acts inductively upon the metallic mass of the knob, stem, and leaves; repels the electricity similar to itself into the leaves, and attracts the other electricity into the knob, so that the leaves will diverge in this case as well as in the former. We can easily determine the species of the charge that produces any sensible effect on the leaves of the electroscope. If the leaves have been electrified by discharge from the given body, let the body be withdrawn; and let a definitely excited body, say a positively excited rod of glass, be brought down from a distance towards the knob. As the rod approaches it will repel into the leaves a continually increasing positive charge; so that if the divergence of the leaves increases under this inductive action, the charge upon the leaves is evidently increasing and was therefore originally positive. If on the other hand the divergence diminishes, the charge upon the leaves must have been originally negative. When the body that we examine is not put in contact with the knob, but only brought near to it so as to electrify the leaves by induction, the simplest way of testing the species of the charge is, first to charge the knob and leaves with a known electricity, and then to bring the body down gradually from a distance towards the knob. The increase or diminution of the divergence will prove here as in the former case, that the charges on the body and in the electroscope are similar or dissimilar. The sensibility of the gold leaf electroscope in its simplest form is very great; and it may be greatly augmented by the use of a condenser instead of the external knob. The instrument in this form is sometimes called the gold leaf condenser. When an Electroscope is so constructed as to measure the intensities of charges, it is called an electrometer. The several forms of apparatus referred to are sometimes employed as electrometers. For this purpose indices are attached to the instruments which mark the extent of the movements of the small charged bodies. None of these instruments, however, can compete as accurate electrometers with the Torsion Balance of Coulomb. For an account of this invaluable instrument, see the article on the BALANCE OF TORSION; see also the article on ELECTRICITY, especially the remarks upon the ELECTRIC FORCES and upon DISTRIBUTION.

Electrotype is a new art which has not yet been so extensively cultivated, as it will one day assuredly be. The delicacy of some of its operations with which workmen are not yet familiar, as well as their powerlessness to manage processes which they do not more than half comprehend, sufficiently explain its present state. Yet in the seventeen years which have passed since its simultaneous invention by Spencer (an Englishman), and Jacobi (a Russian), very much has already been accomplished. The art of Electrotyping has been employed for the reproduction of coins and medals; the copying of stamps and seals, and plaster casts; for obtaining hollow copies of surfaces in relief, and the reverse; for imitating fruits and vegetables; for making moulds for the foundry, reproducing printed characters, copperplates, woodcuts, and daguerreotype pictures, and for copperplate engraving. The mere enumeration of these varied forms of appliance, will at once show the reader how important an instrument in art, a process which has accomplished this in seventeen years, must ultimately become. The process rests upon general rules, which should be followed with great care, and which we shall first indicate, before describing those special precautions that must be adopted. The purpose of it, is the precipitation, by means of a galvanic current, of a metal, from a chemical solution of it, upon a given object in a continuous layer (not, however, adhering to the object), so that this layer may accurately represent the object in all its minuteness and detail. Sometimes it is not meant that what is deposited in this way should come off—that is, the process is employed to deposit a permanent metallic coating upon an object. At other times, it is meant to be removed. The processes for both, with slight variations as to the length of time and the conditions of operation, are nearly identical. Either a simple or a compound pole may be used in the generators of the galvanic current which is employed. In the first, the mould itself forms an essential part of the galvanic current, in the other, it is outside the decomposing bath; the advantage being, that in the latter case there may be attached to the copper pole, a soluble electrode, that is, a plate of the same material as the metal being decomposed in the bath, which itself disintegrates, and nearly makes up for the loss of metal precipitated upon the mould. The first form of the electrotype art, as discovered by Spencer, was this. A square copper plate was put in communication with a zinc plate of the same form and size, by means of a copper wire. The plate was covered with a hot coating of varnish made of unbleached wax, resin and red ochre; and, with a metal point letters had been traced on the varnish, laying bare the copper. Then a vessel was half-filled with a saturated solution of sulphate of copper, and the copper plate immersed in it; also the glass of another smaller glass vessel (one mouth of which is closed by a plaster of Paris diaphragm) filled with a dilute solution of sulphate of soda to two-thirds of its contents. The zinc element of the galvanic couple was plunged into the

latter solution, and placed parallel to the plaster stopper, the wire being so bent that the copper plate should also be parallel to it on the other side. The moment that the current was completed, the copper from the solution of the sulphate, became deposited in the furrows made in the varnish, so as to produce the characters in relief. The idea immediately occurred to Mr. Spencer, that this method might be made useful in printing, and he actually employed it for this. It is needless to indicate more minutely—what indeed the whole of this article besides will do—how variously the ideas suggested by this root-phenomenon became subsequently developed. Certain difficulties were met with at the first stage. In overcoming these, new discoveries were made. The first was, that the layer deposited in a moderate time was so thin that it was with difficulty it could be taken off entire. To cure this, Mr. Spencer allowed the plate to stand for a very long time, and he found that instead of the deposit hardening and becoming non-adherent to the plate, it adhered firmly. Here then a new difficulty. If a mould was to be taken in this way, either the layer must be taken off, when too thin to be safely taken off, or if allowed to remain, it must become so adherent, as to injure the original in the separation. The suggested idea of employing moulds taken in plaster of Paris, wax, &c., of the object to be copied, was very natural. In copying a medal—the layer would have been indented where the medal was in relief, and *vice versa*. In copying the mould from the medal—the electrotypic reproduction would be an exact fac-simile of the original.—We proceed to enter on a few details. We shall not assign names to all the inventions enumerated. The most eminent in the cultivation of the art are perhaps, Messrs. Smee, Elkington, Grove, Mason, and MM. Becquerel, Boquillon, Elsner, Solly, Sorel, and Chevalier. We have said that the galvanic apparatus employed may be simple or compound. We shall describe both. First, the *Simple*. That most commonly used is figured below. In a glass, porcelain, or stoneware vessel, the solution of the metal to be decomposed, is put *e.g.* for copper, the sulphate of copper. In the middle of that vase, another one, P, is set, much smaller in diameter, and of porous material. Into this latter, sulphuric acid diluted with twelve or fifteen times its weight of water, is poured, and a plate or cylinder of zinc, Z, inserted. The moulds, *n*, are put in communication with this zinc by a brass wire. As the deposition of copper from the sulphate constantly weakens it, while the process goes on, it is best to keep continually supplying the loss by the addition of fresh crystals, or a bag containing them, as K in the figure, may be placed in the solution.—An apparatus of M. Becquerel's, manifestly on the same principle, is made of a square wooden triangle, glazed inside with gutta percha, and divided into two compartments by a porous diaphragm, on one side of which is the dilute acid, and on the other, the solution of sulphate. The porous vase P, is thus got rid of, and the apparatus is much more easily used. It will be well to keep the temperature between 100° and 160° Fahr.; and as the degree of saturation becomes always unequal in such a process, to turn the box pretty frequently. Another inconvenience comes from the unequal thickness of the metallic deposit, which is always more copious opposite to the point where the mould is attached to the wire. Several conductors symmetrically placed behind, are made use of to remedy this. The origin of another inequality is, that the distance of the corresponding points of the zinc and the mould are not all the same. This can only be remedied, either by giving the zinc the same relief as the mould itself, or making the plaster diaphragm take a suitable shape. The latter method is quite practicable.—The *Compound cell* process as it is called, is, as we have already said, when the current is produced in a vessel different from that in which the decomposing solution is placed. In the annexed figure, A is the battery, and B the vessel into which the decomposing liquor is poured. The mould with zinc plate corresponding—or, as may be in the compound cell—*copper plates*, which supply the loss of the copper ore, being as in the figure; and the connection established. A yet simpler form is, where there are no cells—but B is a mere trough, along which, a metallic rod is laid parallel to its edge, from which rod the moulds are suspended, while parallel to it again, a copper plate, the whole length of the vessel, is dipt into the sulphate; the plate and the rod are then connected with the wires S and Z, respectively. In those batteries, as generally in galvanic batteries, the zinc ought to be amalgamated. The advantages obtained are numerous. There are especially these three—*first*, in that state the metal is not affected by the solution in which it is placed, until the connection is made—*secondly*, the process is very much more regular, in the unamalgamated zinc,

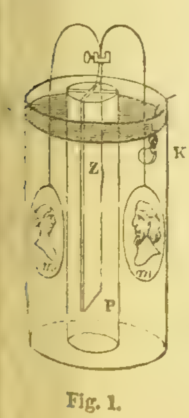


Fig. 1.

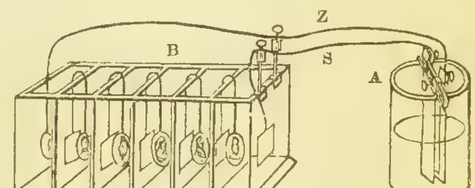


Fig 2.

indeed there seem to be certain slight subsidiary electric currents between parts unequally electric, making the process irregular,—currents which besides, (this indicating the *third* advantage,) waste the power, so that not nearly the same equivalent of work (effective or valuable), is obtained from unamalgamated, as from amalgamated zinc. Recent experiments by M. Millen, appear to indicate that some foreign matter will be ultimately discovered, which when present in the solution in very slight quantity, will render amalgamation unnecessary. The simplest process of amalgamation is this. Pour water, pure sulphuric acid and mercury, together into a phial, and then brush with this mixture the surface of the zinc, until it acquires a very bright surface.—For the various kinds of Batteries that are in use in the generators of the voltaic current, reference is made to the article BATTERY, in this Cyclopædia. There is only one, not mentioned there, which is yet little known, but seems to promise very remarkable results. This is the pile of Prince Bagration. According to M. Jacobi's description, it will excel all others in the constancy and regularity of its effect—in the little care needed for its management—and in its own extreme simplicity. It can work for example, for more than six weeks without being touched, preserving perfect regularity of action. Then again, it is so simple in construction, that it may be used and set up anywhere, and its cost is very slight. It consists of a flower-pot, or any other vase impermeable to water: this is filled with earth saturated with a solution of sal-ammoniac, then a copper plate C, and a zinc plate Z, are placed, and put in the earth; and a simple voltaic couple is thus obtained, the action of which may be almost indefinitely kept up by moistening the earth occasionally with more of the solution, and by renewing the zinc plate as it becomes eaten away. It is good before immersing the copper plate to plunge it into a solution of sal-ammoniac for a few minutes, and to leave it to dry, till decided oxidation becomes visible on the surface. The plates must not be too near, and they must be of such size as to overcome the resistance to transmission which the earth interposes. Several elements may, of course, be used instead of one. M. Jacobi recommends that the different vases should be carefully isolated. The theory upon which the process rests is somewhat obscure;—but, assuming that the apparatus does act as described, there is one very manifest advantage which it has over all the other batteries,—viz.: it gives off no acid exhalation. Those who have wrought at experiments with the ordinary Daniell's or Maynooth batteries,

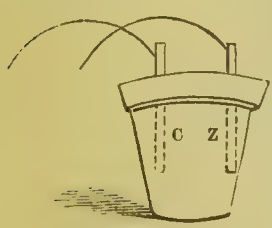


Fig. 3.

can appreciate the advantage.—The Baths employed in Electrotyping have now to be described—*i.e.* the nature of the solution containing the substance to be deposited. We give only a short list of the best. For gold, the following solution may be used: 100 parts of distilled water, 10 parts of cyanide of potassium, and 1 of cyanide of gold. To obtain different shades of gold, we must attach to the picture pole, plates of gold alloyed with silver or copper. The same solution, substituting silver for gold only, serves for silvering substances. We may also have 100 parts of distilled water, 10 of cyanide of potassium, and 1 of carbonate or of ferro-cyanide of silver. Also 100 of distilled water, 10 of hyposulphate of soda, and 1 of chloride or of phosphate of silver. The solutions for platinum are analogous. For nickel, the nitrate or the ammoniac sulphate may be used For copper, the sulphate chloride, nitrate and acetate (especially for its cheapness, the sulphate). In the case of the sulphate, there is considerable resistance to the current, and the power of the bath is increased by addition of a little nitric or sulphuric acid. When this is the case, the soluble electrode is attacked, and the strength of the solution is increased. If the matter of the mould should be more oxidizable than the copper, i would plainly be unwise to add the acid, so much so, that both Jacobi and Spencer discourage the use of all acid solutions in consequence. Th nitrate of copper needs for its decomposition weaker current than the sulphate, but it i much dearer. The soluble electrode is, in bath of copper, always of copper. For zinc, th sulphate—for lead, the acetate, very dilute, an acidulated with acetic acid, or a little nitri acid—for tin, a solution in aqua regia acidulate by nitric acid is perhaps the best.—In givin these details, we have spoken very generally But, four circumstances, which have been noted distinctly bearing on the success of any propose experiment; though no clear laws of their actio have as yet been arrived at. 1st. The intensity of the pile. 2d. The degree of concentration, and th conducting power of the solution. 3d. Its temperature. 4th. The relative position and magnitude of the two electrodes. M. Boquillon ha shown that, as these conditions vary, the deposi of metal may become as hard and brittle as stee as soft and flexible as lead, or even appear as powder, or in a crystalline form. As examples, M Boquillon says that, all other things being equa if the positive electrode be greater than the nega tive, or mould, a crystalline deposit will be pro duced, which may become a powder, if the diffe ence be considerable. The contrary effect occurs the positive electrode be the smaller. Like effec are produced by a rise of temperature. If we ha three cases—the first where the solution is con pletely saturated, the second where it is less so, an the third where very slightly so, in the first t deposit will probably be hard and brittle, in t

second more flexible, in the third formed of a spongy mass of unaggregated crystals, ultimating a black nonadherent powder.—For the *moulding*, or *types*, any body capable of conducting electricity may be made use of, provided only it be not of such a nature as to be attacked by the solution, and to react on the precipitated metal. A nonconductor may also be used, if we take the precaution to give its surface the faculty of conduction by a very thin layer of a conducting body in a state of powder. The conducting bodies fitted to give moulds are the metals, well powdered charcoal and plumbago. The sulphate of copper, which is the commonest solution made use of, may be affected by zinc, tin, and iron. Hence these metals cannot be used. Platinum and gold would answer perfectly, but they are too dear. There are only left to us silver, copper, and lead, and the alloys of the latter. As silver is precipitated by gold and platinum, it should be used to reduce the metals when the principal deposit is to be very fine. Very fine copper moulds may be obtained by depositing an electro-chemical deposit of the metal on the original piece or plate. Sheet lead suits admirably, being first carefully cleared of oxide on its surface, then smoothed by placing it on a sheet of iron under a press. If the object to be copied be laid with its face to the lead, above the iron, and the press applied, the most delicate copy of the object will be obtained. Where the object cannot be submitted to such pressure, other methods require, of course, to be employed. These are the chief metallic moulds. The non-metallic consist chiefly of sealing-wax, soft wax, compound wax, stearine, paper, plaster of Paris, and sulphur. The latter gives very admirable copies of objects, being extremely delicate, but the moment that it touches the precipitated metal, it is apt to combine with it and form a sulphuret. When the plaster mould is obtained, its surface must be covered with something metallic, in order that it may become a conductor. This layer of conducting matter requires to be excessively thin, that it may not alter the relief of the object. This is done either by solutions or powders. In the first case, the surface is washed with a solution from which the metal is deposited either by the agency of light or of some vapour or gas, which takes away the other constituents on being passed over the surface. Sometimes, however, this leaves fissures on the moulds, which destroy the electric conductibility, so that the whole lower half may not be represented. Hence, it is usual to powder the surface rather, and the powders generally used are those of copper and of silver. When the surface of the mould is to be of glass, it is necessary to cover it first with a light varnish, that will lay hold of the conducting powders.—We shall conclude the article by a brief description of various applications of the electrotype process to the arts. *First*. The reproduction of coins or metals. This is done in three ways. 1. The piece is directly used as the negative pole, after necessary precautions are taken to prevent cohesion. These consist in passing over the original a very thin coating of some greasy substance, as oil, wax, stearine, suet, or the like, and then taking off as much of this as is possible by sucking it up with fine linen cloth. There is obtained by this process an image in hollow, which is again used to give the desired fac-simile of the original object. 2. The impression of the piece is taken on a fusible alloy, so that the first electrotype operation gives the desired relief. 3. The impression is taken with one of those plastic substances which have been before specified. In all the three cases, great care must be employed to prevent the adherence of air-bubbles to the moulds; without this they could not be reproduced in all their delicacy. This is especially troublesome when they consist of glass. To get rid of this, the piece must be examined after it has been for some little time in the solution, and lightly heated, to dispel and disperse them. When only the one face of the medal is to be copied the other must be covered. The *second* application, to the copying of seals and plaster-casts, depends on principles perfectly identical with those last. *Thirdly*, the process is applied to statuettes and bas-reliefs. We regret exceedingly that our space will not allow us to describe the method adopted by Dr. Fau for this purpose. The *fourth* application is to the reproduction of fruits, plants, vegetables, &c. In fact, the actual fruit is just covered with the finest possible layer of metal—in which all the delicacies of nature will manifestly be retained, and then its moisture is extracted first, and ultimately the whole fruit. We shall not do more than allude to the applications of this beautiful art to the purposes of the founder, the electro-metallurgist, the looking-glass manufacturer, the engraver. It has been applied also to the reproduction of photographs and daguerreotypes. Assuredly, in the triumphs which it has already accomplished, there is the highest possible promise for its future.

Elements. The name *Elements*, is given in Astronomy to those numerical quantities that enable us to compute the place of any planet or satellite in the Heavens,—in other words, to compute the size of its elliptic orbit, the position of that orbit in reference to the Ecliptic, and the position of the planet or satellite in its orbit at any given time. Tables of these *elements* as they exist for different epochs, are given in all good treatises on Astronomy; and the mode of using them is familiar to every instructed Student. We subjoin a general Table of the Elements of the chief Planets and Satellites of our Solar System:—for those of the Asteroids, see ASTEROIDS. Further reference to details of the subject will be found under the names of the several Planets. The application of such numerical data, is, of course, a deduction from the theory of Gravitation.

		Distance from Sun. Mean.	Greatest.	Least.	Eccentricity.	Sidereal Revolution in Days.	Synodical Revolution in Days.
Mercury...	☿	·3870984	·4666927	·3075041	·2056178	87·9692824	115·877
Venus......	♀	·7233317	·7282636	·7183998	·0068183	224·7007754	583·920
Earth	♁	1·000000	1·0167751	·9832249	·0167751	365·2563744	
Mars	♂	1·523691	1·6657795	1·3816025	·0932528	686·9794561	779·836
Asteroids...							
Jupiter.....	♃	5·202767	5·453663	4·951871	·0482235	4332·5848032	398·867
Saturn......	♄	9·538850	10·073278	9·004422	·0560265	10759·2197106	378·090
Uranus.....	♅	19·18239	20·07630	18·28848	·0466006	30686·8205556	369·656
Neptune...	♆	30·03627	30·29816	29·77438	·0087193	60126·722	367·488

	Long. of Perihelion.	Annual Variation.	Longitude of Ascending Node.	Annual Variation.	Inclination of Orbit.	Annual Variation.	Mean Daily Motion.	Compression.
	° ′ ″	″	° ′ ″	″	° ′ ″	″	′ ″	
Mercury...	74 57 27·0	+ 5·81	46 23 55·0	—10·07	7 0 13·3	+0·18	245 32·6	1/150
Venus......	124 14 25·6	— 3·24	75 11 29·8	—20·50	3 23 31·4	+0·07	96 7·8	
Earth	100 11 27·0	+11·24					59 8·3	1/299
Mars	333 6 38·4	+15·46	48 16 18·0	—25·22	1 51 5·7	—0·01	31 26·7	1/50
Asteroids...								
Jupiter.....	11 45 32·8	+ 6·65	98 48 37·8	—15·90	1 18 42·4	—0·23	4 59·3	1/17
Saturn......	89 54 41·2	+19·31	112 16 34·2	—19·54	2 29 29·9	—0·15	2 0·6	1/10
Uranus.....	168 5 24	+ 2·28	73 8 47·8	—36·05	0 46 29·2	+0·03	42·4	1/9
Neptune...	47 17 58		130 10 12·3		1 46 59·0		21·6	

	Time of Rotation.	Diameter. Apparent.	In Miles.	Volume.	Mass.	Density.	Light at Perihelion.	Aphelion.	Gravity.	Bodies fall in one second.
	h. m. s.	″								
Sun.........	607 48 0	1923·64	888,646	1415225	354936	·250			28·36	456·6
Mercury...	24 5 28	6·69	3,089	·0595	·0729	1.225	10·58	4·59	0·48	7·7
Venus......	23 21 21	17·10	7,896	·9960	·9101	·908	1·94	1·91	0·90	14·5
Earth	23 56 4		7,926	1·0	1·0	1·0	1·034	0·967	1·0	16·1
Mars	24 37 22	5·8	4,070	·1364	·1324	·972	·524	·360	0·49	7·9
Asteroids...										
Jupiter.....	9 55 26	38·4	92,164	1491·	338·718	·227	·0408	·0336	2·45	39·4
Saturn......	10 29 17	17·1	75,070	772·0	101·364	·131	·0123	·0099	1·09	17·6
Uranus.....		4·1	36,216	86·5	14·251	·167	·0027	·0025	0·76	12·3
Neptune...		2·4	33,610	76·6	18·900	·321	·0011	·0011	1·36	21·8

The preceding elements are for the beginning of 1840, except in the case of Neptune, for 1854.

ELEMENTS OF THE MOON.

Mean distance from the Earth	59·96435 Earth's radii.
— Sidereal Revolution..	27·321661418 days.
— Synodical —	29·530588715 — .
— Longitude, January 1st, 1801	118° 17′ 8″·3
— — of perigee at do..	266° 10′ 7″·5
— — — ascending node at do.......................................	13° 53′ 17″·7
— inclination of orbit	5° 8′ 47″·9
— revolution of nodes...	6798·279 days.
— — — perigee	3232·575343 days.
Eccentricity of orbit	·0548442
Diameter of the moon	2153 miles.
Density—that of Earth being 1	·5657
Mass	·011399

ELEMENTS OF JUPITER'S SATELLITES.

No.	Sidereal Revolution.	Distance in Radii of Jupiter.	Orbit Inclined to Jupiter's Equator.	Diameter.		Mass, that of Jupiter being 1.
				Apparent.	In Miles.	
	d. h. m. s.		° ′ ″	″		
1	1 18 27 23·505	6·04853	0 0 7	1·015	2436	·000017328
2	3 13 13 42·040	9·62347	0 1 6	0·911	2187	·000023235
3	7 3 42 33·360	15·35024	0 5 3	1·488	3573	·000088497
4	16 16 32 11·271	26·99835	0 0 24	1·273	3057	·000042659

ELEMENTS OF SATURN'S SATELLITES.

No.	Sidereal Revolution.	Distance in Radii of Saturn.	Eccentricity.	Longitude of Peri-Saturnium.	Mean Longitude.	Epoch.
	d. h. m. s.			° ′ ″	° ′ ″	
1	0 22 36 17·7	3·1408	·06889	104 42 0	264 16 36	1789·705
2	0 8 53 2·7	4·0319	uncertain		67 56 25	1789·705
3	1 21 18 33·0	4·9926	·0051	184 36	158 31 0	1836·308
4	2 17 44 51·2	6·399	·02	42 30	327 40 48	1836·0
5	4 12 25 11·1	8·932	·02269	95	353 44 0	1836·0
6	15 22 41 24·9	20·706	·029223	244 35 30	137 21 24	1830·0
7	21 4 20	25·029	·115	295	32	1849·0
8	79 7 54 40·8	64·359			269 37 48	1790·0

ELEMENTS OF URANUS' SATELLITES.

No.	Sidereal Revolution.	Daily Motion.	Mean Apparent Distance.	Mean Distance in Miles.
	Days.	°	″	
1	2·520315	142·8373	13·54	119,994
2	4·14397	86·8732	19·28	170,863
3	8·705866	41·35133	31·44	278,627
4	13·463263	26·73943	42·87	379,921

ELEMENTS OF NEPTUNE'S SATELLITE.

Sidereal Revolution	5d. 21h. 0m. 17s.
Apparent Mean Distance	16″·75
True Mean Distance	232,000 miles.
Orbit Inclined to Plane of Ecliptic	29°

Elevation. It is now recognized among geologists that the inequalities of the earth's surface are mainly owing to the agency of some Grand Elevating Force seated at a greater or less depth within its mass. What that Force is, what its origin, and in what manner it is connected with the general Physics of the Globe, are problems whose solution lies beyond the sphere of this Dictionary: but as with Earthquakes, the phenomena of Elevation are being reduced within the sphere of Dynamical Laws; and in so far as these are concerned, it is necessary that we glance in this place, however briefly, at the remarkable and most interesting subject. The true dynamics of geology owe their foundation to Mr. Hopkins of Cambridge. Avoiding all ultimate theories, he rests simply on the fact, that upheavals do take place not of chains of mountains merely, but, as investigation demonstrates, of vast zones, of which these ranges are the mere accidents; and the question is, whether by aid of the common laws of Force, we may not deduce some general features necessarily characterizing all such dislocations of the earth's crust? Mr. Hopkins' inquiry—now extended over several highly instructive memoirs—has three special divisions. I. The simplest case of elevation is that in which the upheaving force is limited to a small area—say to a point on the earth's surface—its intensity diminishing according to the distance from that point. The upheaval under such circumstances will manifestly be confined to a limited circular region, and must result in what is now well known as a *crater of elevation.* The splits or fissures of the upraised strata will probably be confined to a single system—a system, viz., of long radii from the central point; or there may exist separately or conjoined with the former, another system

dividing the distended soil into concentric rings. The phenomena attending such upheavals had been fully discussed previous to the investigations of Mr. Hopkins, by Elie de Beaumont and Dufrenoy—perhaps with an aim at too high numerical precision—in their interesting memoirs on the volcanoes of central France; where every attainable elucidation is thrown on the phenomena of circular elevations. It ought to be remarked that these species of dislocations seem to have prevailed among the older epochs of the history of the earth: and certainly it has affected most abundantly the existing surface of our satellite. II. The upheaving force, however, instead of being confined within a small district, may be diffused below a large tract or zone: and the simplest mode of considering its operation in this case, is to neglect the shape of that zone, unless in regard to its two dimensions, length and breadth. Now, if such a zone be elevated, it must, during the whole course of its upheaval, be *stretched*, or subjected to *tensions*, which incline to tear or split it; and a little consideration will show, that, (as results from Mr. Hopkins' Geometry) these tensions must reach their *maxima* in two directions; one set inducing the zone to split in the direction of its length, and another acting at right angles to these, and tending to produce fissures along the zone's breadth. Without going farther then, we obtain an insight into the phenomena of *transverse valleys* uniformly characterizing every mountain chain; and the reason is seen why mineral veins are generally found associated in two systems at right angles to each other. A very effective attempt is made by Sir R. Murchison, in his recent Memoir on Scandinavia, to explain the broken-out lines of these northern regions by this prolific principle. But if, in either of these directions, the zone yields in more than one place, it must do so *along parallel lines*. The direction of the *length* of the zone being that of the *maximum* of one set of tensions, it is plain, that whether the zone splits, in obedience to these tensions, in one place or many, the fissures must assume *that direction*, and no other; and as the zone must here be supposed of uniform constitution, it is much more likely that several cracks will take place, than a single one. Nor can there be a doubt that these parallel fissures must occur *at the same moment of time;* for, on the occurrence of one crack by itself, the tension would clearly be relieved; and the only result of a further upheaval would be the mere widening of the first crack, which is now a *line of weakness.* Hence, then, the frequency of the phenomenon of parallel veins, and hence their necessary contemporaneity. There is no difficulty in verifying these deductions, in all the aspects and relations of mineral veins, faults, &c.; and the same physical principles doubtless, overrule that grand series of dislocations treated by Elie de Beaumont. It appears, then, that the contemporaneity of parallel chains crowning the same zone of upheaval must be taken as absolute and undeniable; but, unfortunately, there are two considerations which prevent an absolute *application* of the principle; and in which those difficulties originate that are so emphatically indicated in De Beaumont's scheme of systems, by the existence of separate groups, of widely different ages, but with corresponding directions. The first source of ambiguity is in the effect of successive shocks of elevation undergone by the same zone; for though these can create no new fissure, but would simply farther elevate the mountain chains resting over the old ones; they may make, even at a later period, fissures belonging to the oldest shock *visible for the first time.* Mr. Hopkins clearly demonstrates that th. cracking of every zone begins on its *lowest surface*, and proceeds upwards; and as it is perfectly conceivable that the first shock may not have completed the split in many cases, or caused it to reach the surface, it is quite consistent with theory, that subsequent shocks, while farther elevating the masses protruded through many splits, may suffice to *complete others*, and rear over them a mountain range, not referable, in so far as dislocations of the neighbouring strata intimate, to the epoch of the first disturbance. Mr. Darwin also inquires what might be the probable effect in strengthening an old fissure, of long intervals of rest, during which the injected rock had intertwined itself with the other beds around it and formed with them one consolidated mass? "Would not," he asks, "the crust in such case yield more readily on either flank, as I believe it must have done in the Cordillera, than along an axis composed of solidified rocks, such as granite and porphyry?" But, besides, the law only holds in the case of mountain ranges *belonging to the same zone;* and there seems no reason to believe, considering the extent of the earth's surface, and the frequency of these oscillations, that *different zones*, with major axes corresponding in direction, may not have been acted on at different epochs. For instance, with regard to that immense area now undergoing subsidence in the coralline regions of the Pacific, is it not probable that its major axis corresponds in direction with the eastern Alps and Himalaya; and, therefore, that such also will be the direction of its system of dislocations? It is far from improbable that the groups in De Beaumont's scheme, which correspond in direction, but differ in age, demand such a solution; and that this is the information they give us regarding the History of the Earth. Like all apparent exceptions to simple and enlarged truths, such ambiguities, while checking the universality of the application of general laws, do not invalidate the laws themselves, but only inform us better of the circumstances within which they have operated. III. Mr. Hopkins next surveyed a case in which the resulting phenomena might be supposed modified by the shape of the upraised surface; and probably his application of

general principles to the specialties of the Wealden, is as complete a triumph as any inquiry of this kind can be expected to accomplish in theoretical geology. The deductions having quite outrun observation, Mr. Hopkins re-surveyed the Wealden, with a view to the detection of its dislocations; and his theoretical chart might almost be taken as an accurate map of the country. Now, in this application of the theory, there is an achievement quite beyond any mere probable explanation of the *law of parallelism*. This law, although still the prevailing one, is interfered with and subjected to great modifications by the *shape* of the district—modifications, however, of a fixed and determinate nature, flowing from the same actions which caused the law of parallelism itself; and every *predicted* deviation of the lines of fissure or dislocation from rectilinearity and accurate parallelism, as well as every predicted new relation of the transverse lines to the longitudinal ones, agrees—to an exactness altogether remarkable—with the facts elicited by a careful scrutiny of the region.

Elevation (*Angle of*). See DEPRESSION.

Elimination, a process so important and comprehensive, that it may be said to constitute the whole of analysis. Every research in analysis may, indeed, be put under the form of a problem to be resolved. Equations between certain quantities are presented—portions of which are *known*, the others *unknown*,—the values of these last being the *quæsita*. Now the object of analysis is to extract the required value from these equations; in other words, to *eliminate* the quantities that at first rested unknown. Until comparatively recently, the methods of elimination were simple, elementary, and often only tentative: the subject has now grown into a large and profound theory, containing within it some of the subtlest speculations of the *modern Algebra*. In our article EQUATIONS, certain of the more interesting portions of this inquiry have been exposed; but space would not permit a full appreciation either of its extent or remarkable generality. Under POLYNOME, we shall give as detailed and satisfactory an account as the nature of this work will allow, of several researches of the modern Algebra, such as DETERMINANTS, with which the theory of elimination is now intimately connected; and very fainly would we have entered in this place on a special and systematic glance over the whole subject. This, however, is rendered impossible, in the meantime, by other exigencies of this work; and, although most unwillingly, we must simply refer the student to such works as the *Algebre* of *Lebefure de Fourcy*, of *Bourdon* (latest edition), of *Serret*, &c., and more particularly to the distinct and very able treatise of *Faà de Bruno*. The only substitute we can at present offer is a mere enumeration of the contents of Bruno's volume; these contents will at least indicate the sphere, and show the boundaries of the doctrine of elimination as it is now understood. Bruno arranges his treatise into three great chapters, whose subdivisions are mainly as follows:—I. THEORY OF ELIMINATION FROM TWO EQUATIONS. (1.) *On Symmetrical Functions of the* ROOTS *and their Properties.* (2.) *Elimination of the Variable from two Equations with one Variable.*—Definition of the resultant; different modes of elimination by aid of symmetrical functions of the *roots;* methods by which the resultant is sought for as that of a system of linear equations; method of the greatest common divisor; method of Bezout; method of finding the resultant by considering it as the *discriminant* (see POLYNOME) of a function of the second degree; formation of polynomial multipliers. (3.) *Properties and Uses of the Resultant.*—II. THEORY OF ELIMINATION IN THE CASE OF THREE EQUATIONS WITH TWO VARIABLES. (1.) *Properties of the Solutions common to two Equations with two Variables.*—Number of common solutions; degree of the final equation when the two equations are not canonical; method of *Liouville* for developing an implicit function into series; calculation of symmetrical functions; theorem of *Jacobi.* (2.) *Elimination of the Variables from three Equations with two Variables.*—The resultant formed by means of symmetrical functions; method of *Bezout;* method of *Sylvester.*—III. GENERAL THEORY OF ELIMINATION. (1.) *Properties relative to Common Solutions.*—Symmetrical functions of common solutions; theorem of *Betti; Bezout* on the degree of the final equation; method of *Liouville;* theorem of *Jacobi;* number of independent solutions. (2.) *Research and Formation of the Resultant.*—Formation of the resultant by aid of symmetrical functions; method of *Bezout;* method of *Sylvester.* (3.) *Properties of the Resultant.*—If the English student can learn nothing else from the foregoing bald list of contents, he may at least learn this—how wrong it were for him, in the present condition of science, to rest satisfied with a knowledge of what once passed current as an account of the *Theory of Elimination.*

Ellipse. One of the well known conic sections. Unlike the parabola and the hyperbola, it is a re-entering curve: on one side its limit is the circle, on the other the parabola. The equation of the ellipse referred to its centre is—

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=1,$$

where a and b are the major and minor semi-axes. The student should consult *Salmon's Conic Sections.*—Of course it is universally known, that the ellipse is the curve in which our planets move.

Ellipsoid. If we imagine an ellipse to revolve round either its major or minor axis, and suppose that each successive ellipse so formed in space be marked down, the resulting surface will be what is called a *spheroid.* As we have already

seen that every circle is a sort of ellipse, the spheroid is a kind of ellipsoid. As the earth is of a spheroidal form (FIGURE OF EARTH), the properties of the spheroid, and of its generic surface the ellipsoid, are of the greatest physical importance. The technical expression for the ellipsoid, referred to its centre is $\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$.

Ellipticity. The proportion which the excess of the larger semi-axis of an ellipse over the smaller, bears to this smaller axis. It is not the same, therefore, as the *eccentricity*. Its value as technically given is $\frac{a-b}{b}$.

Elongation (*Angle of*). The angle measuring the distance between two stars as seen from the earth. It is the angle made by the lines drawn from the eye to each star respectively. Custom applies it only to the case of bodies in the solar system; and still further confines it, in almost every case, to a planet and the sun. We thus speak of the elongation of Mercury,—*i.e.*, its distance from the sun, and instead of the elongation of two fixed stars or planets, we use the word "*distance*."

Emersion. The reappearance of one heavenly body from behind another after an *eclipse* or *occultation*.

Empirical Law. If, on the contemplation of a number of separate facts connected with the same subject, the inquirer discerns some external relationship expressible by a simple term or proposition, that term, or theorem, is called the Empirical Law of these facts. For instance, Bode's Law of the Distances of the Planets from the Sun, would, were it correct, be an Empirical Law: in the same way in Physics, multitudes of facts are connected and expressed by general terms, or *Empirical Laws*. These Laws may be termed the rudest and rudimentary steps in the process of generalization; and they often lead to the discovery of Higher Laws, or Laws that connect the whole range of subordinate phenomena with some wider scheme of sequences. These latter laws appear *causative:* the former never are so.—Empirical Laws may frequently be most easily detected by GRAPHIC METHODS, to which article the student is referred.

Energy.—*Energetics.*—The term *Energy*, in its larger and only true sense, is the capacity to effect changes; and if any general laws can be predicated of Energy as such, these laws must be applicable to, or dominate over, every branch of Physical Science, and express the order and mode in which changes are effected. Hence the idea of a pure and abstract *Science of Energetics*—a science which, if constituted, would be the highest *abstraction* or *generalization* attainable in *physical research*. For the first attempt to lay the basis and sketch the probable course of such a generalization, science is indebted to Mr. Macquorn Rankine,—a sketch which must be ranked among the most important of our numerous and emphatic indications, that Physical Science is about to enter on a fresh and vigorous career. The reader is earnestly referred to a paper on the *Science of Energetics* in the *Edinburgh Philosophical Journal* for July, 1855. See also HEAT and POTENTIAL.

Engine. A name given to a mechanical contrivance, by which any physical power is applied to produce any given physical effect. The difference between the words *engine* and *machine*, is not at all a definite one: oftenest the words are used indiscriminately. For a notion of certain general theoretical principles applicable to the construction and play of Engines, see MACHINES.

Engine, Air. See AIR ENGINE.

Engine, Calculating. We shall speak briefly in this place of that class of machines which have been proposed for the execution of calculations and other mathematical operations.—It is, of course, wholly impossible to endow any mechanism, however ingenious, with the power of *thought*, or the ability to originate: but without intrenching on this inaccessible sphere, there are duties no less extensive than onerous, which it quite falls within the scope of mechanical contrivance to perform. Let an operation be *definite*, or have a fixed order, then—no matter for its complexity—we have seen enough to know, that some combination of motions and processes may be imagined, that might undertake and determine it: the results of existing manufacturing genius should establish that there is at least nothing wonderful in the conception, that an engine can be contrived capable of relieving the scientific inquirer of much, if not of all that tedious but pure manipulation with figures, in which his time and energies are at present so largely consumed. Recently, for instance, we have had a very successful *Arithmometer*, by M. Thomas, of Colmar, by which all ordinary arithmetical calculations are executed without fatigue to the operator: and again, the machine just exhibited before the Royal Society, by M. M. Scheutz, which—resting on the principle of *Differences*—is able, on the turning of a wheel, to give the successive terms of any *series*, whose law may be confided to it. The machine of these very excellent Swedes likewise *prints* a large part of its results, and thus still further provides for the accuracy of its tables. But while giving all honour to these and other gentlemen, it were equally unjust and unbecoming to refrain from naming as the instigating and guiding genius in this remarkable career—our countryman, Mr. Babbage. Mr. Babbage's achievements—not *projected* merely, although not yet *realized* achievements—are twofold. I. In the first place he perfected a *Difference Engine* of very comprehensive powers. On referring to our short article on DIFFERENCES, the reader may have it re-

called to him, that any *series*—be the relation uniting its terms as complex as it may—will in the end yield a certain order of Differences that shall be 0. The complicacy of the relationship merely affects the *order* of those differences which becomes 0—the more complex the relationship, the *higher* that *order*. Now, Mr. Babbage's enterprise was this,—he undertook to construct an engine capable of managing series so complex, that the differences of its terms do not reach *zero* until we ascend to the seventh order: or in analytical language, he undertook to manage the *integral*, defined by the equation

$$\Delta^7 \phi z = 0.$$

and this holds where $\phi . z$ contains no power of a variable higher than the *sixth;* or when

$$\phi . z = a + b x + c x^2 + d x^3 + e x^4 + f x^5 + g x^6.$$

An immense range of nautical and astronomical tables lies within the limits now defined; but, still further, while an engine with such capabilities, commanded everything within its grasp accurately and completely, it also tabulated approximatively, or, between intervals of greater or less extent, any series whatsoever, that could be treated by the *Method of Differences*. Referring, again, to our short article on DIFFERENCES, the student will see that the hope to succeed in such an enterprise, how novel soever it appeared, was not chimerical: it rested on this only, that an engine could be made capable of performing at command all operations of *addition*. The chasm between the idea and the realization of it, is in this case vast indeed; but we believe that it has been universally conceded, that all practical difficulties had yielded to the genius of Mr. Babbage.—II. During the process of the construction of the *Difference Engine*, Mr. Babbage's views enlarged—probably in so far through his growing familiarity with the capabilities of machinery; and a new, and much more gigantic conception arose before him in perfect definiteness. If an engine could be constructed to perform, at command, the process of addition, no reason seemed to exist why one might not perform the whole of the elementary changes to which quantity can be subject, viz. *addition*, *subtraction*, *multiplication*, and *division*. But all changes that can be produced on quantity, in other words, any development to which quantity can be subject, are mere combinations of these: so that an engine capable of performing these at command, might become an instrument to execute any development whatsoever. And such an instrument is the proposed *Analytical Engine*. Without stopping to describe the machinery, we shall take it, as a fact accepted everywhere, that Mr. Babbage devised the means of executing *directly* all elementary operations. And the next requisite was, that he should be able to cause his engine perform all these, according to *any special order*,—or what is the same thing, to develop any function whatsoever, whose law of development is ascertained and fixed. To obtain a clear conception of the mode in which he realized this object, it is necessary that the reader have in his mind a distinction, already of vast value in Analytic Science, and exemplified everywhere in our industrial mechanisms —the distinction, viz. between the operations to be performed, and the quantities or substances operated on. An *operation*, is the method, or the *law* according to which some object or material is to be changed; and is perfectly distinct from consideration of the material or object itself. Can an engine be made, then, so that it be adjusted to the performance of any order of operations, however complex,—so that, whatever its abstract capacities, it may, at any time, be constrained to work according to *some fixed law or order*, to the exclusion of every other? Suppose the *zero*, or neutral state of the *Analytical Engine*, to be a mere expression or possession of capability to execute all the elementary and essential changes on quantity, can it be adjusted to perform these according to a fixed law, or what is the same thing, to *develop any function?* The answer has been practically given by the JACQUARD LOOM. In this case, the *cards*, oblige a machine, in which there really is a latent power to work any pattern— to work out one particular pattern: and Mr. Babbage saw, that, in the same way, a peculiar and appropriate set of *cards of operation* might compel the calculating machine, to act for the time, according to one certain fixed law and no other. The wonderful results of the *Jacquard*, illustrate the amazing comprehensiveness of this principle; and it may further assist our conceptions if we liken the *numerical* or other quantities, which form the subject-matter of the functions, to the *material* on which the Jacquard mechanism works. These *numbers*, or *subjects*, are introduced into the Analytical Engine by arrangements quite independent of those which regulate the *operations* to which they are to be subjected: the two, in fact, work independently, although harmonizing throughout; and the result of the two is the reproduction of the matter,—introduced in a *raw* state—in the shape of cloth with the pattern woven. It is clear, too, that the matter or things acted on need not be *numbers:* such an engine could overtake any problem concerning *objects* whose natural fundamental relations can be expressed by the relations $+$, $-$, $\times$, and $\div$:—for instance, if the fundamental relations of pitched sounds were susceptible of any similar expression, the Engine would be capable of *weaving* elaborate and scientific pieces of music of any degree of complexity or extent.—On the details of the mechanism of the *Difference Engine*, the reader will find a very instructive paper in the *Edinburgh Review* for July, 1834; and a much more comprehensive and generalized description of *both*, in an exceedingly precise memoir by M.

Menabrea, of Turin. A translation of this memoir, accompanied with very remarkable notes, from the pen of the late Lady Lovelace—Lord Byron's accomplished Ada—appeared in the third volume of *Taylor's Scientific Memoirs.*— It were unjustifiable to conclude even this brief notice, without an expression of profound regret that circumstances of any kind should have induced Government to suspend the realization of great works like the foregoing,—works which a Government alone could realize, and of which any Government might well be proud. It is gratifying to notice, that in a spirit of fullest justice to Mr. Babbage, and under deep sense of the public importance of what is at issue, Lord Rosse took occasion, on his vacating the chair of the Royal Society, to make a reclamation on the subject, which every scientific man will join in hoping may have its deserved success.

Engine, Locomotive. See LOCOMOTIVE ENGINE.

Engine, Steam. See STEAM ENGINE.

Engine, Thermo-dynamic. See HEAT, § 22, &c.

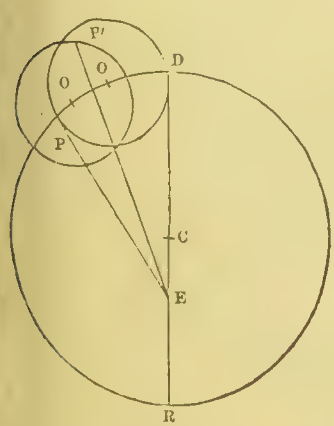
Engineering. That department of science which may be termed the Science of Engineering, is defined most accurately as follows. Every one conversant with mechanics, knows that the abstract truths of Statics, Dynamics, &c. cannot be represented in any actual case of equilibrium or in the working of any machine: in no case in nature, for instance, do we find a pure mathematical line or circle; so, likewise, in no case do we find the perfect expression of pure or rational mechanical truth. And the cause is this:—instead of working, in nature, with *ideas*, or *hypotheses*, we work with materials having certain properties or qualities of their own; and it is only through these instruments, with their peculiar and essential qualities or characteristics, that Static or Dynamic theorems can be presented in practice. It becomes, therefore, a question of the last importance, in what manner must these qualities of our instruments, modify the action of our pure theorems; and how must we take account of such inevitable modification, before applying these theorems? The reply to which question is the end and aim of the true *Science of Engineering.* Unfortunately, it cannot be said that any such Science has hitherto been methodized; although, in the constructions and writings of our famous engineers and architects, there are abundance of valuable contributions towards its various departments. The object of the present *Cyclopædia* does not expressly include this wide sphere of inquiry; and it is to be hoped that the publisher may be induced to undertake as a separate and express work—the *Cyclopædia of Practical Mechanics and Engineering.* Nevertheless, it is not possible to draw an absolute line of demarcation; and certain considerations more suitable to the other work, are touched, under such articles as ELASTICITY, FORCE, FRICTION, MACHINE, ROTATION, STRENGTH OF MATERIALS, &c. &c.—There are now, among most civilized nations, regular and organized corps of scientific men pursuing, in various ways, the calling of engineers,—such as *Architects; Civil Engineers; Military Engineers; Engineers of Mines, of Roads and Bridges, Railways,* &c. &c.

Epact. The number of days after the beginning of the year, when the new moon takes place. It is used in all our almanacs for this. Suppose, for example, 12 be the epact of this year, then the new moon will come on the 12th of January, and by adding 29 and 30 alternately, we get the dates of all the new moons throughout the year thus. Then, for next year, we add 11 to 12, because 11 is the difference of the days in the year 365, and the days of the 12 lunar months 354. We obtain thus 23 as the epact for next year. Add another 11 and we get 34. Now, if the first lunar month of that year be to be taken as 30 days, then 4 (34—30) will be the epact, and so on. For the year immediately succeeding leap year, we must add 12 (366—354).—The system of epacts has many curious and somewhat puzzling details which we do not require to describe. It is chiefly in use for determining the date of Easter and of the church festivals depending on it. It is of Greek origin. A very interesting account of it will be found in Delambre (*Astronomie Moderne*, i. 4-32).

Ephemeris. A species of almanac of extensive use to navigators and astronomers. To the former it is a matter of the utmost importance to determine their exact position on the globe. He can do this, as we shall see (LONGITUDE and LATITUDE), by sextants, chronometers, and other methods of direct observation. But all of these are liable to very many errors, and require to be corrected one by the other, leaving no complete certainty on the mind after all. His Ephemeris, or as in this country it is more commonly termed *Nautical Almanac*, tells him, however, the exact position for every hour of every day of most of the heavenly bodies, gives him the exact moments of eclipses of satellites, the amounts of lunar distances, &c. &c. for the standard position—that of the Greenwich observatory in this country. He observes them from his own position, and calculates it from the observation. For the astronomer it is equally important. Many phenomena can now be so calculated that we shall thus know them better, than we can by observation. They depend upon general laws admitting of mathematical statement, and therefore we can infer the exact date of the phenomenon, the true position in which it will place some visible object, and such like. The knowledge of this, as directing the astronomer when to observe, and how, would be of very great importance, but it becomes of still greater importance in another light. He does actually

›bserve the phenomenon, under, for example, he disturbing influence of *refraction.* He gets he exact position calculated mathematically rom his almanac, and he finds the apparent ›osition from his observation. The difference of he two is due to this influence of refraction, and ie tabulates that. From a series of such obser-ations, he can find the laws which that influence ollows, and can correct those observations which annot be predicted, in which this same disturb-ng influence operates, so as to arrive at the true ›henomenon which he wishes to observe. If, is is often the case, in any given observation on he phenomena calculated by the almanac, several auses of error act, the resultant difference will e due to them all, and he will have, after tabu-ating these differences, to adopt the method of ELIMINATION (*q.v.*) to arrive at the values of ach by itself. The French nautical almanac, r *Connaissance des Temps*, is the best known of he foreign ones.

Epicycle. The notion that uniform and ircular motion inheres in all bodies so grand as he celestial ones, was a source of considerable rouble to the ancient astronomers. They re-olved to cling to it whatever came of it; ind they had to make some artificial physical ypothesis to support its credit. One dis-repancy has been already accounted for (see ECCENTRIC), but a greater remained. It was ound by very ordinary observation, that the lanets appear—instead of moving in a circle—n fact to move in a very extraordinary line vithout any apparent regularity. The in-entive Greek mind surmounted the difficulty n this way. The planet moves in a regular ircle, the centre of which regular circle moves iniformly and circularly itself round the ec-entric (*q.v.*) in which we are. Thus (see fig.) the planet P may move in the small circles indicated, the centres of which circles move along the larger circle DR. Here, though the general motion of the planet be unquestionably forward, we can still understand how at ne time it should take a turn backwards and gain progress, then go forward more rapidly, then ppear to stand still, and may change, at once he direction and velocity of its motion, in all maginable ways, simply by a proper adjust-nent of the rates of these two motions, viz., f the planet in the circumference of the small circle, and of the centre of that circle, along the circumference of the large one. The figure will show a case, where a general forward motion, from O to O′ will appear to the spectator at E, to be in reality a backward motion from P, where the planet really is at the time that the centre is at O, to P′ where it is when the centre is at O′. All varieties of motion might be similarly produced.



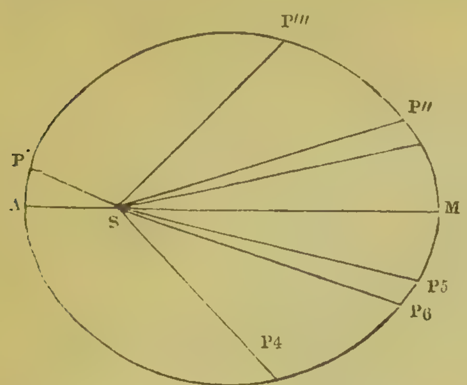
Epoch. The date of some memorable event in the history of the world, from which nations reckon their time;—the commencement of an Era. The Christian era commences at the epoch of Christ's birth; and the Mohammedan, at the date of the Hegira.

Equation, Differential (*Technical*). The theory of differential equations, is an extension of the integral calculus, in which it is required to find the function whose differential, with regard to a variable, is known. In some cases we have given, the differentials of higher orders, the second, third, fourth, &c., and we are required to find the function corresponding. The subject has not been yet so fully considered as it ought from its importance. A differential equation is expressed in such a form as this, $\frac{d^2 y}{d x^2}+\frac{d y}{d x}=x$, where y is a function of x, which it is required to discover. In $\frac{d y}{d x}=x$ we have a problem of the integral calculus—a mere case of this. See CALCULUS.

Equation, Lunar. A perturbation of the moon. See LUNAR THEORY.

Equation of Centre. The motion of the earth round the sun is seen by us exactly as if it were an equivalent motion of the sun round the earth. If the earth moved round the sun with a uniform circular motion, we would suppose the sun to do the same round the earth. It does move, however, in an ellipse, describing equal areas, in equal times, and so does the sun appear to move. This ellipse, however, varies very little from a circle. In uniform circular motion we could readily tell the direction of the visual ray and the change of direction—in fact, determine the longitude of the sun and its changes. In any number of days the same fraction of 360° of angular space would be described, as this number of days is of the complete year. The closeness of the actual ellipse to a circle makes it convenient for us to calculate what would be the position of the sun if this were the true state of the case, and to tabulate the differences from this result, which actually exist. How these differences arise, we can understand. Suppose P P to be the sun's apparent or the earth's real orbit, and S the central orb in the focus of the ellipse. Let A M be the axis major and M S P′, A S P be two equal angles. Then, according to the hypothesis of uniform angular motion the spaces M S P′, A S P should be described in

equal times. But, according to the facts of the case, they will not be. M S P′ has a much larger area than A S P, and will therefore take longer time to be described. The sun will there-



fore appear to move slower at P′ M than at S P, and in consequence of this inequality, it will happen that the mean longitude would make the visual ray perhaps take the direction S P″ while the actual direction is S P′. The angle P′ S P″ is called the *equation of the centre.* The motion at A P is thus more rapid than the average motion. This excess above the average increases from A onwards by new additions, until the body P reaches such a position as P‴. There the motion is just about the average motion, but still the *true place* is much in advance of the *mean place*, though that advance now ceases to increase and begins to diminish, lessening the excess gradually until it vanishes, and the *true* and *mean* place coincide. Where there is the greatest difference between the *mean* and *true* place of the earth, or apparent place of the sun, this difference (*e.g.* the angle P′ S P″) never exceeds $1° 55′ 33″ \cdot 3$. In case of the motion from A to M, its amount is to be added to that of mean longitude—in the other, from M to A, to be subtracted; at M and A, it will be nothing.

Equation of Differences (*Technical*). An Equation of Differences is such as this;—required a function of X, such that the difference between it and the same function of a number larger than it, whatever it may be, say by 1, shall differ by 1. This equation would be so expressed,

$\phi(x+1) - \phi(x) = \phi(x) + 1$. Required $\phi(x)$. These equations of differences have been of immense importance in the history of recent physical science. All our more valuable theories are based on them; and from solutions of them, a great many of our most beautiful recent discoveries take their origin. See DIFFERENCES.

Equation of Equinoxes. The motion of the equator along the ecliptic, which is distinguished by the name of *precession*, is like those of which we have been just speaking, an alternating one—sometimes faster, sometimes slower, and returning in a cycle constantly to the same degrees of velocity. Taking the motion then as uniform, we calculate the position of the equinoxes on the ecliptic, and tabulate the amount which must be added to or subtracted from that calculated result, to give the *true* position of the equinoctial point. This difference is set down in tables as the *equation of the equinoxes.*. A table of these differences will be found at page 242 of the *Nautical Almanac* for 1854, which may give the reader a notion of the amount of these variations. The position of the equinox is given by it for every ten days, during which interval, the alteration of rate is not of importance, and can be calculated.

Equation of Time. It has been already frequently noted that there is a difference between the lengths of the mean day and the apparent day. If the actual apparent day—the interval between the sun's successive transits of the meridian—were of uniform length, the apparent and mean time would be the same. But the simple mechanisms which we can construct as measures of time give measures merely of successive *equal* intervals, and cannot be brought except by most complex machinery to coincide entirely with the solar motions. Yet it is convenient that the two methods of measuring should be coincident as much as possible; and for that purpose the interval which clocks and watches are set to measure, is the mean of the solar days for a year or a century or an epoch. The causes of this inequality in the length of the solar day are due to all the "equations" which affect the regularity of the earth's motion round the sun. Those have been already referred to. They express different velocities and different spaces during the same intervals of time, of the sun's proper motion (as it appears to us) among the stars. In one day, for instance, he may move say $1\frac{1}{2}$ degree backward, and in another, say only $\frac{3}{4}$ degree. These irregularities have considerable effect in causing the difference of mean and solar time.—Another cause is the obliquity of the ecliptic. If the equator and ecliptic coincided, the exact amount of motion in longitude of the sun would be, each day, represented full in the apparent day. But as they do not, but form two separate circles having a definite inclination and a changeable one also, the reader acquainted with trigonometry will see that there cannot be the same amount entering, at different times, into the valuation of the mean day. At one time, the motion is parallel nearly to the equator—at another, differently inclined to it. At one time, the sun in his motion is going towards the equator—at another, from it, and these differences will be readily conceived to occasion our connecting different parts of the real amount of change of longitude (or apparent solar motion), with the apparent solar day. To remedy this a fictitious sun is supposed to move in a circle parallel to the equator (cutting the meridian at right angles always, instead of with a constantly varying obliquity), and the di

erence between its hypothetical motion and the eal motion of the sun in the ecliptic, is tabulated. The whole of these equations are put together, in he *equation* of *time*. Its object thus is, to enable is to adjust the mean noon, measured by our locks and watches, with the true noon regulated by the sun. Tables of it are found in he nautical almanacs; its amount is sometimes pretty considerable. Sometimes the apparent noon is $16\frac{1}{2}$ minutes before mean noon, and ometimes $14\frac{1}{2}$ minutes behind it.

Equations, Algebraic.—It will be assumed hroughout this article that the reader knows he elements of algebra, at least up to quadratic quations and the binomial theorem. All equaions involving one unknown quantity x, may be omprehended under the general form—

$$f(x) = \varphi(x);$$

where $f(x)$ and $\varphi(x)$ denote *functions of* x, that is, *quantities* or *expressions derived*, in a more or ess complex manner, *from* x *and given numbers.* t should be observed at the outset that this echnical form of statement, $f(x) = \varphi(x)$, adnits of two meanings, perfectly distinct from each ther, and so far indicated by the terms *identity* nd *equivalence*. *First*, The expressions $f(x)$ and (x) may be one and the same; or they may be educible to one and the same, by the performnce of operations merely indicated. To assert uch a relation of $f(x)$ to $\varphi(x)$, mathematiians employ the form of statement $f(x) = \varphi(x)$, which is therefore called, in such cases, n *identical equation*. The following are exmples:—

$$x + 5 = x + 5,$$
$$2x + 7 - (x + 4) = x + 3,$$
$$(x + 3)^2 = x^2 + 6x + 9.$$

Evidently the quantity represented in any idencal equation by the general symbol x may be uite indeterminate: the equation holds for all alues of x. With equalities of this kind we hall have nothing to do in what follows, except s means of investigation. *Secondly*, If the unctions $f(x)$ and $\varphi(x)$ are not identical, they ill be generally unequal when a certain value assigned to (x); but for some particular value r values of x, the functions may be equal to ach other. To assert this relation of equality etween $f(x)$ and $\varphi(x)$ for a particular value or alues of x, mathematicians employ still the form f statement $f(x) = \varphi(x)$, which, in such a ase, is called simply an *equation:* and by a *root* f the equation is meant *any quantity*, positive or egative, arithmetical or literal, real or imaginry, *which, when substituted for* x, *satisfies the quation*, or verifies the statement of equality. y the *resolution of an equation* is meant the etermination of its roots: this is the main inquiry the theory of equations, and one to which all her questions upon this subject are subordinate. y transposition of terms, every equation involvg one unknown may be reduced to the form $F(x) = 0$, where $F(x)$ is a function of x. The equation is called *algebraic* when $F(x)$ is an *algebraic function* of x, that is, an expression derived from x, in combination with given numbers, by *algebraic* operations alone. In the study of algebraic equations, it is generally found convenient to reduce them, or to suppose them reduced to the following form:—

$$\left.\begin{array}{r} x^n + p_1 x^{n-1} + p_2 x^{n-2} + \ldots \\ + p_{n-1} x + p_n = 0 \end{array}\right\} \quad (1.)$$

As distinctive of which form let these facts be remembered: the second side of the equation is 0: on the first side the powers of x are arranged in descending order, the indices diminishing by unity from term to term, beginning with the whole number n, which measures the degree of the equation: the coefficient of x^n is $+1$: the following coefficients $p_1, p_2 \ldots p_n$, are all *given* numbers, any one or more of which may $= 0$ in particular cases. We shall often refer to the above equation as the eq. (1): we shall sometimes represent its first member, for brevity, by $f(x)$: sometimes also we may have to speak of $f(x)$ as a *rational integer function* of x; meaning by this that $f(x)$ is the sum of a series of multiples of powers of x, including generally a term, (such as p_n) independent of x, but excluding all powers with negative or fractional indices. Whatever be the form in which an equation is proposed, or in which it presents itself as the algebraic translation of some actual question, it may always be reduced to the form (1) by such operations as the transposing and collecting of terms, clearing of fractions, clearing of radicals, dividing by the coefficient of x^n; and as these operations are generally of the most elementary nature, we shall refer to them no further. A brief exposition of this very wide subject will now be attempted, under the three following heads:—*First*, the general theory of algebraic equations. *Second*, the resolution of numerical equations, or of equations whose coefficients are given arithmetically. *Third*, the resolution of algebraic equations, that is, of equations whose coefficients are represented by letters, and therefore left indeterminate.

I. The General Theory of Algebraic Equations.

1. For a moment, let us consider $f(x)$, or the first member of eq. (1), apart from the condition of its equality to 0. If we assign any value to x, it is evident that $f(x)$ will assume one definite value; if we assign a second value to x, $f(x)$ will assume a second definite value, and so on. It follows that $f(x)$ *varies in value along with* x, *according to a perfectly definite law;* and we may observe in passing, that it is this and this alone which is implied in the name *function of* x. The study of the law of simultaneous variation of $f(x)$ and x belongs properly to the differential calculus; but there are a few notions upon

the subject that are indispensable in the discussion of the elementary properties of equations. Suppose then that x is changed into $x + h$; the successive terms of $f(x)$ are changed into $(x + h)^n$, $p_1(x + h)^{n-1}$ . ... $p_{n-1}(x + h)$, p_n: and these terms may be developed in series of ascending powers of h, by the binomial theorem, as follows:—

$$\begin{array}{l} x^n \quad + \quad n x^{n-1} h + \ldots\ldots + h^n \\ p_1 x^{n-1} + (n-1) p_1 x^{n-2} h + \ldots + p_1 h^{n-1}, \\ p_2 x^{n-2} + (n-2) p_2 x^{n-3} h + \ldots + p_2 h^{n-2}, \\ \ldots\ldots\ldots\ldots\ldots\ldots \\ p_{n-1} x + p^{n-1} h, \\ p_n. \end{array}$$

We obtain the value of $f(x + h)$, or the new value of $f(x)$, by adding all these quantities; adding by columns we find a result, of the form

$$f(x) + \mathrm{P}h + \mathrm{Q}h^2 + \mathrm{R}h^3 + \ldots\ldots + h^n:$$

where the coefficients P, Q, &c., are independent of h. The full expression for P is—

$$n\, x^{n-1} + (n - 1)\, p_1\, x^{n-2} + (n - 2)\, p_2\, x^{n-3} + \ldots + p^{n-1},$$

which the more advanced reader will recognize as the *first differential coefficient* of $f(x)$, by which name we shall refer to it hereafter. But observe that the first diff. coeff. of $f(x)$ is defined here to be *the coefficient of the first power of* h *in the development of* $f(x + h)$ *in a series of ascending powers of* h. Observe also, from the above expression for P, that to find the diff. coeff. of the first member of eq. (1), *we multiply each term* of that member *by the index of* x *in the term, and then diminish the index of* x *by unity*. These elementary notions will be of use hereafter. Returning to the development of $f(x) + h$), we observe that if the increment of x be h, that of $f(x)$ is—

$$\mathrm{P}h + \mathrm{Q}h^2 + \mathrm{R}h^3 + \ldots + h^n, \text{ or}$$

$$h \left\{ \mathrm{P} + \mathrm{Q}h + \mathrm{R}h^2 + \ldots + h^{n-1} \right\}$$

Now the successive coefficients P, Q, R, &c., are finite when x is finite; this is evident from their composition in terms of x and n: and therefore the quantity within the vinculum in the last expression (while it may be in some cases positive, in others negative, in others nothing) *cannot be infinite* when x and h are finite. It follows that the increment of $f(x)$ is, at most, a finite multiple of the increment of x: and therefore, when the increment of x is very small, that of $f(x)$ is also very small; and the two increments vanish together. Hence, when x passes from one finite value a to another finite value b, *continuously* (that is, by an infinite succession of increments, each infinitely small), then $f(x)$, or the first member of eq. (1), passes *also continuously* from the value $f(a)$ to the value $f(b)$. This conclusion, that $f(x)$ is a *continuous function* of x, would require more space for its proper illustration than may be here given to it; but the only deduction from it that we shall have occasion to apply is perhaps evident enough from what precedes. It is this, that if $f(x)$ be positive when $x = a$, and negative when $x = b$, or *vice versa*, then there is at least one value of x between a and b for which $f(x)$ equals nothing. In proof of one other property of $f(x)$, consider a series of multiples of descending powers of x, such as—

$$a x^n + b x^{n-1} + \ldots \quad + r x + s \ldots \quad . \text{ (2.)}$$

in which the coefficients a, b, .. r, s, are given numbers, positive or negative. Dividing by x^n, we obtain the series—

$$a + \frac{b}{x} + \ldots + \frac{r}{x^{n-1}} + \frac{s}{x^n} \ldots \text{ (3.)}$$

All the terms of the series (3), after the first, evidently diminish in absolute value as x increases. Suppose the number of those terms to be m, and equate each of them in succession to the quantity $a : m$. Of the resulting values of x, such as

$$\frac{m\,b}{a} \text{ and } \sqrt[n]{\frac{m\,s}{a}}$$

let the greatest be denoted by e. Then, if x be made equal to e, the greatest of the terms of (3) after a will be equal to $\frac{a}{m}$; and since the number of those terms is m, their aggregate cannot exceed a numerically, and will be generally less than a. It is evident therefore that if the finite number e be determined as above, the series (3) shall have the same sign as its first term a for all values of x greater than e. And hence it is evident, since

$$\text{series (2)} = \text{series (3)} \times x^n,$$

that for all values of x greater than e, the series (2) will assume the sign of the product $a\,x^n$ which is its own first term. The reasoning applies, and therefore the result holds, whether e be positive or negative; only, in the latter case by values of x greater than e, we must understand those *numerically greater*. Finally, $f(x)$ or the first number of eq. (1), is of the same form as the series (2); and therefore *we can alway discover a finite number* (e), *such, that when tha or any greater number, positive or negative, i substituted for* (x), *the function* f (x) *will assume a value having the same sign as its first term* x^n.

2. Let us apply the preceding results in illustration of the statement, that *every algebrai equation has at least one root.* We shall suppos in every case, that the given equation has bee reduced to the form (1), and that the finite num ber e, which we have just had under considera tion, has been properly determined. When th last term p_n of the equation is equal to 0, ther is evidently a root equal to 0; for when 0 is sut stituted for x, all the terms disappear. Suppos

ing then that p_n has some finite value, we shall listinguish four cases, depending on the degree of the equation and the sign of p_n. First case: n odd, p_n negative. When $x = 0$, $f(x)$ is negaive, because it is reduced then to p_n: when $x = + e$, $f(x)$ assumes the sign of its first term x^n, which is +. Now $f(x)$ being a continuous unction of x, and passing from negative to posiive when x passes from 0 to $+ e$, there must be at least one value of x between 0 and $+ e$, for which $f(x)$ equals nothing. In this case, therefore, the equation *has at least one real positive root.* Second case: n odd, p_n positive. When $x = 0$, $f(x)$ is positive: when $x = - e$, $f(x)$ assumes the sign of $(- e)^n$, which is —, because n is odd: so that $f(x)$ passes from positive to negative when x passes from 0 to $- e$. And therefore, in this case, the equation *has at least one real negative root.* Third case: n even, p_n negaive. Here $f(x)$ is negative when $x = 0$; posiive when $x = + e$; positive also when $x = - e$, because n is even. So that in this case the equation *has at least two real roots*, one positive, another negative. Fourth and last case: n even, p_n positive. Here $f(x)$ has the same sign + when $x = 0$, when $x = + e$, and when $x = - e$; so that our test fails. And it may be shown, by a simple example or two, that an equation of the form specified in this fourth case may or may not have a real root. The equation

$$x^2 - 7x + 12 = 0$$

has two real roots, 4 and 3, as may be seen by solving the equation, or by substituting 4 and 3 successively for x. The equation

$$x^2 + 12x + 35 = 0$$

has two real roots, — 7 and — 5. The equation

$$x^2 + 4 = 0$$

has no real root; for the least value of the first member, for real values of x positive or negative, is positive and equal to 4. It has two imaginary roots, $+ \sqrt{-4}$ and $- \sqrt{-4}$, or, as they are usually written, $\pm 2 \sqrt{-1}$. The equation

$$x^2 - 8x + 25 = 0$$

has no real root. Solving by the ordinary method, we find the roots to be $4 + \sqrt{-9}$ and $- \sqrt{-9}$, or $4 \pm 3 \sqrt{-1}$, both imaginary. And accordingly, each of the expressions $4 + 3\sqrt{-1}$ and $4 - 3 \sqrt{-1}$, when substituted for x, satisfies the equation. Finally, the equation—

$$x^8 + 2x^6 + 3x^4 + 4x^2 + 6 = 0$$

has no real root; because the least value of the first member, for real values of x positive or negative, is evidently positive and equal to 6. It would be easy to obtain any number of equations, of any even degree, having no real roots. The question then arises: Must every algebraic equation have *some root?* Or if the affirmative to this be considered self-evident, another question presents itself, which indeed includes the former: Can we assume any general algebraic expression, or form of magnitude, and assert, that every algebraic equation must have *at least one root of that form?* To illustrate the question, let us look back to the preceding examples, and assume a and b as general symbols of real number, positive or negative. The first two equations have roots (4, 3, and — 7, — 5) of the form a: the third has no root of this form, but two $(\pm 2 \sqrt{-1})$ of the form $b \sqrt{-1}$; the fourth has no root of the form a, none of the form $b \sqrt{-1}$, but two $(4 \pm 3 \sqrt{-1})$ of the form $a + b \sqrt{-1}$. Of these forms, the first two, a and $b \sqrt{-1}$, are included in the third, $a + b \sqrt{-1}$; for the latter is reduced to a and to $b \sqrt{-1}$ respectively, when $b = 0$, and when $a = 0$; so that each of these four equations has a root, of the form $a + b \sqrt{-1}$. With regard to the fifth equation, which is of the 8th degree, we have seen that it has no root of the form a; but as yet we can say nothing more. And if an equation, say of the 40th degree, were proposed, having certainly no real root, possibly, as far as we have yet seen, the simplest of its roots would be as complex in relation to $a + b \sqrt{-1}$ as the equation of the 40th degree is itself complex in relation to a quadratic. It is therefore an interesting theorem, as well as a fundamental one, in the Theory of Equations, *that every algebraic equation has at least one root of the form $a + b \sqrt{-1}$.* It is a very difficult proposition, and remained a long time unproved. Demonstrations of it have been given by Gauss, Cauchy, Ivory, and other eminent mathematicians; but they are all very tedious, and far from elementary. I can only refer the reader to Cauchy's *Cours d'Analyse*, or to Lefebure de Fourcy's *Algebre*, where he will find Cauchy's proof, which is perhaps the best upon the whole that has been published. In what follows, this theorem will be assumed.

3. In the study of the properties of equations, the following algebraical theorem is found to be of the greatest consequence. If the first member of equation (1), or the polynomial

$$x^n + p_1 x^{n-1} + p_2 x^{n-2} + \ldots + p_{n-1} x + p_n$$

be divided by $(x - a)$, where (a) is any quantity; and if the division be carried on till the remainder is independent of x; then, 1st, the remainder is

$$a^n + p_1 a^{n-1} + p_2 a^{n-2} + \ldots + p_{n-1} a + p_n;$$

and, 2dly, the quotient is a polynomial

$$x^{n-1} + q_1 x^{n-2} + q_2 x^{n-3} + \ldots + q_{n-2} x + q_{n-1};$$

where each of the coefficients (q) is obtained by

adding the coefficient (p) of the same order (or suffix) to the coefficient (q) of the preceding order, multiplied by (a), that is,

$$q_1 = a + p_1, q_2 = aq_1 + p_2 = a^2 + ap_1 + p_2, \&c.$$

To prove the first part, represent the dividend by $f(x)$, the quotient by $\phi(x)$, the remainder by R: then, by the nature of division,

$$f(x) = (x - a)\,\phi(x) + \mathrm{R} \ldots\ldots (3)$$

This being an identical equation, holds when $x = a$; but in that case,

$$f(x) = f(a);\; (x - a)\,\phi(x) = (a - a)\,\phi(a) = 0;$$

and R is unchanged, because independent of x. So that, when $x = a$, the equality (3) becomes

$$f(a) = \mathrm{R};$$

and therefore, the *remainder* may be obtained from the *dividend* by the substitution of a for x throughout: which was to be proved. The second part of the theorem, or the law of the coefficients q_1, q_2, &c., is discovered, perhaps most easily, by a little reflection upon the actual process of the division by $x - a$. If the reader is not already familiar with the proposition, he may find an advantage in verifying it upon particular cases, as in dividing

$$x^2 + px + q \text{ or } x^3 + px^2 + qx + r \text{ by } x - a.$$

As an immediate deduction from the first part, we obtain the following fundamental theorem:—*If* (a) *be a root of the eq.* (1), *the first member of the eq. is divisible without remainder by* ($x - a$); *and conversely, if the first member be exactly divisible by* ($x - a$), *the quantity* (a) *is a root of the equation.* For, if a be a root, $f(a)$ is equal to nothing, so that the division by $x - a$ leaves no remainder; and conversely, if there be no remainder, $f(a)$ is equal to nothing, or a is a root of the equation.

Observing now that the eq. (1) of the nth degree has at least one root, which may be represented by a, we infer from the theorem now proved that

$$f(x) = (x - a) f_1(x),$$

where $f_1(x)$ is a polynomial of the form

$$x^{n-1} + q_1 x^{n-2} + \ldots + q_{n-2}x + q_{n-1},$$

a rational integer function of x, of the degree $n - 1$. Again, the equation $f_1(x) = 0$ has at least one root, which may be represented by b. Reasoning then as in the former case, we find

$$f_1(x) = (x - b) f_2(x).$$

Where $f_2(x)$ is a rational integer function of x, its first term equal to x^{n-2}. Substituting this value of $f_1(x)$ in that of $f(x)$ given above, we find

$$f(x) = (x - a)(x - b) f_2(x).$$

In the same manner, assuming c as a root of the equation $f_2(x) = 0$, we find

$$f(x) = (x - a)(x - b)(x - c) f_3(x).$$

The process may be continued till we arrive at $f_{n-1}(x)$, which will be evidently of the first degree in x, and its first term equal to x: so that $f_{n-1}(x)$ may be put equal to $x - l$. And hence,

$$f(x) = (x - a)(x - b) \ldots (x - k)(x - l):$$

that is: $f(x)$, or *the first member of eq.* (1), *is the continued product of* (n) *factors of the first degree in* x, *and each of the form* ($x - a$). This result gives a beautiful view of the structure of algebraic equations, when reduced to the standard form (1). One of the simplest conclusions deducible from it is this: that every equation has as many roots as it has dimensions, and no more. It appears thus: Each of the n quantities, a, b, c, ... k, l, is a root of the equation $f(x) = 0$; for the product $f(x)$ vanishes when any one of the factors $x - a$, $x - b$, ... $x - l$, vanishes, or when $x = a$, when $x = b$, ... when $x = l$. No other quantity than these can be a root of the equation; for the product $f(x)$ cannot vanish while each of its factors $x - a$, $x - b$, ... $x - l$, has a value different from zero. The number of the roots of eq. (1) is therefore n. But to prevent wrong notions, observe that in particular cases, any number of the quantities a, b ... l may be equal to each other. Suppose that

$$a = b, \text{ and } k = l = 0;$$

then the product $f(x)$, still of the nth degree in x, becomes

$$(x - a)^2 (x - c) \ldots (x - h)\, x^2;$$

and the number of different values of x that satisfy the equation is now not n, but $n - 2$. Cases of this kind are not allowed to stand as exceptions to the general statement: we consider, in the instance now supposed, that the equation $f(x) = 0$ has still n roots, two of them equal to a, and two equal to zero. And in this way it holds universally, that an equation of the nth degree has neither less nor more than n roots.

4. An important property of Imaginary Roots may now be established. The most general form of such a root is $h + k\sqrt{-1}$, where h and k are real, and k different from zero. If this expression be substituted for x in the first member of eq. (1), the result may be represented by $H + K\sqrt{-1}$; where H and K are rational integer functions of h and k independent of $\sqrt{-1}$, H involving only even powers of k, and K involving an odd power of k in each of its terms. K may therefore be divided by k, and if the quotient be represented by L, the above expression becomes

$$\mathrm{H} + k\,\mathrm{L}\sqrt{-1} \ldots (3)$$

If we had commenced with the substitution

$h - k\sqrt{-1}$ for x, the resulting value of $f(x)$ would be

$$H - k\,L\sqrt{-1}\,; \ldots (4)$$

this being the value of $H + k\,L\sqrt{-1}$ when k is changed into $-k$, because H and L involve only even powers of k. Now, if $h + k\sqrt{-1}$ be a root of the equation $f(x) = 0$, the expression (3) must vanish: and by a first principle of Algebra in relation to imaginary quantities, this condition is equivalent to the two equations

$$H = 0 \text{ and } k\,L = 0, \text{ or}$$
$$H = 0 \text{ and } L = 0:$$

and if these equations be satisfied, the expression (4) also will vanish. But we have seen that the expressions (3) and (4) are the values of $f(x)$ when the quantities $h + k\sqrt{-1}$ and $h - k\sqrt{-1}$ respectively are substituted for x: and we infer, that the conditions necessary to constitute $h + k\sqrt{-1}$ a root of (1) are sufficient to constitute $h - k\sqrt{-1}$ also a root. In other words: *Imaginary roots occur always in pairs, of the form* $(h \pm k\sqrt{-1})$, differing only in the sign of the imaginary part. Such a pair are called Conjugate Roots.

Hence we deduce an interesting result on the composition of $f(x)$. The factors $x - a$, $x - b$, . . $x - l$, of which we have shown that $f(x)$ is composed, are not real unless the roots $a, b \ldots l$, are real. If the equation have a pair of imaginary roots $h \pm k\sqrt{-1}$, the two factors $x - a$) of $f(x)$ corresponding to them are

$$(x - h) - k\sqrt{-1}, \text{ and}$$
$$(x - h) + k\sqrt{-1}\,;$$

and their product is $(x - h)^2 + k^2$, or

$$x^2 - 2\,x\,k + (h^2 + k^2).$$

and hence: the first member of eq. (1) is composed of as many *real factors* of the 1st degree of the form $x - a$) as the equation contains real roots, with as many *real factors* of the 2d degree (of the form $x^2 + bx + c$) as the equation contains pairs of imaginary roots.

5. Given the roots of an equation, required the equation itself: more definitely; Given the roots of the eq. (1), required the coefficients $p_1, p_2 \ldots p_n$. To solve the question in any case; subtract each root from x, multiply the results together, arrange the product in a series of multiples of powers of x, and equate it to 0: this equation is that required; for (by section 3) it admits of all the assigned roots, and of those only. What we wish to obtain, however, is a general view of the results of this process. To begin with a simple case, let the assigned roots be *two*, a and b. The equation is

$$f(x) = (x - a)(x - b) = 0, \text{ or}$$
$$x^2 - (a + b)\,x + a\,b = 0\,;$$

and therefore, $-p_1 = a + b$, $p_2 = a\,b$.

Again, let the assigned roots be a, b, c. The equation is of the form

$$f(x) = x^3 + p_1\,x^2 + p_2\,x + p_3 = 0:$$

and if we multiply the trinomial $f(x)$ of the last example by $x - c$, we find

$$-p_1 = a + b + c,$$
$$p_2 = ab + ac + bc,\ -p_3 = abc.$$

Several cases thus taken are sufficient to suggest the following theorem: In the eq. (1), $-p_1$ = sum of the roots, p_2 = sum of the products of every two, $-p_3$ = sum of the products of every three; and generally, $(-1)^m p_m$ = sum of the products of every m roots. To prove the theorem; *assume it to be true* for an equation

$$x^{n-1} + q_1\,x^{n-2} + \ldots + q_{n-1} = 0 \ldots (2.)$$

Multiplying the first member by $x - l$, and equating to 0, we obtain an equation

$$x^n + (q_1 - l)\,x^{n-1} + (q_2 - lq_1)\,x^{n-2} + \ldots = 0,$$

or more briefly,

$$x^n + p_1\,x^{n-1} + p_2\,x^{n-2} + \ldots p_n = 0 \ldots (1.)$$

The roots of eq. (1) are the same as those of eq. (2), with the additional one, l; and the coefficients of (1) are obtained from l and the coefficients of (2) as follows:—

$$-p_1 = -q_1 + l, \quad (3)$$
$$p_2 = \quad q_2 - lq_1, \quad (4)$$
$$-p_3 = -q_3 + lq_2,$$

and so on, the law being evident, to p_n, which $= -lq_{n-1}$. Now, by the assumption, $-q_1$ is the sum of all the roots of eq. (1) except l; and therefore, the value of p_1 as determined by the equality (3), is according to the theorem. Again, the sum of the products of every two roots of eq. (1) may be divided into two parts, one containing all the products which do not involve l, the other containing those that do involve l; and it is evident that by the assumption, q_2 is equal to the first of those parts, and $-lq^1$ equal to the second; so that the value of p_2 determined by the equality (4) is also according to the theorem. Precisely similar reasoning applies to the succeeding coefficients p_3, &c.; therefore, *if the theorem be true for every equation of the* (n — 1)*th degree, it is also true for every equation of the* n*th.* But the theorem is verified, as we have seen above, for an equation of the 3d degree; it is therefore true for one of the 4th degree, and so on, from one degree to the next above it; so that it holds for all equations. The proof now given is a good example of *demonstrative induction*, a kind of reasoning often employed in algebraic inquiries.

Examples.—The equation whose roots are 1 and 2 is

$$x^2 - 3\,x + 2 = 0;$$

that whose roots are $-1, 1, 2$, is

$$x^3 - 2x^2 - x + 2 = 0;$$

that whose roots are $1, -1, \sqrt{-1}, -\sqrt{-1}$, is

$$x^4 - 1 = 0.$$

There is a corollary to this theorem that will be useful to us in what follows. Keeping the same notation as before, we have (by section 3)

$$f(x) = (x-a)(x-b)\ldots(x-l).$$

Changing x into $x+h$, we obtain

$$f(x+h) = \{h+(x-a)\}\{h+(x-b)\}\ldots \{h+(x-l)\};$$

and if the multiplications indicated in the second member were performed, the result would be a polynomial

$$h^n + Ah^{n-1} + \ldots. + Qh + R,$$

where the coefficients A ... Q, R, are independent of h. From the definition given in section (1), it is evident that Q is the differential coefficient of $f(x)$; and by the theorem just proved, Q is the sum of all the products of the n quantities $x-a$, $x-b$, ... $x-l$, taken $n-1$ together, products that may be found by the division of $f(x)$ by each of the quantities $x-a, \ldots x-l$, separately. Hence, the differential coefficient of $f(x)$ is equal to

$$\frac{f(x)}{x-a} + \frac{f(x)}{x-b} + \ldots. + \frac{f(x)}{x-l},$$

and also, by section (1), equal to

$$nx^{n-1} + (n-1)p_1 x^{n-2} + \ldots. + p_{n-1};$$

so that *these two expressions are equal;* a very useful algebraic theorem.

6. Let us inquire, for a moment, how far we are yet advanced towards the solution of our principal question: To find the roots of an equation. To discover the roots in the most elementary manner, we proceed by trial, substituting particular numbers for x, and retaining those as roots that satisfy the equation. The theorem

$$f(x) = (x-a)(x-b)\ldots(x-l),$$

as extended in the end of section (4), suggests another method of proceeding; as it reduces the determination of the roots of $f(x) = 0$ to the decomposition of $f(x)$ into its real factors, simple or quadratic. Obtaining the factors we obtain the roots. But the latter inquiry is of such a nature, that this result must be considered, not as in any proper sense a *solution*, but as a mere *transformation* of the original question into a definite algebraic form. In the same connection, observe, that if we have discovered one root a, or two roots a and b, of $f(x) = 0$, we may divide $f(x)$ by $x-a$, or by the product $(x-a)(x-b)$: the quotient equated to 0, will admit of the remaining roots of the proposed equation, and no more. And generally, if $f(x)$ can be decomposed into the factors P, Q, R, involving x, the roots of $f(x) = 0$ are precisely those of the equations

$$P = 0, Q = 0, R = 0,$$

each of which is of less dimensions than the proposed equation, and on that account more easily solved. For example, the binomial $x^3 - 1$ is divisible by $x - 1$, and the quotient is $x^2 + x + 1$; therefore, the roots of

$$x^3 - 1 = 0 \quad . . (2)$$

are those of the two equations

$$x - 1 = 0, \; x^2 + x + 1 = 0.$$

Solving the last, we find the three roots of eq. (2), or the three cube roots of 1, to be

$$1, \frac{-1+\sqrt{-3}}{2}, \frac{-1-\sqrt{-3}}{2}.$$

Similarly, or by a change of signs, we find, that the equation $x^3 + 1 = 0$ has the three roots

$$-1, \quad \frac{1-\sqrt{-3}}{2}, \quad \frac{1+\sqrt{-3}}{2}$$

In like manner, the equation $y^6 - 1 = 0$, may be transformed into

$$(y^3 - 1)(y^3 + 1) = 0;$$

so that the sixth roots of unity are the roots of the two equations $y^3 - 1 = 0$ and $y^3 + 1 = 0$ already found. In the last section it has been shown, how, when the roots of an equation are given, the coefficients, or the equation itself is determined. It is natural to suppose, at first sight, that the relations there found to subsist between the coefficients and the roots might be inverted in some simple manner, so as to give the roots in terms of the coefficients; and if this were done, it would furnish a complete resolution of all algebraic equations. But it appears, upon examination, that those relations between the coefficients and the roots are not generally capable of being transformed, by any means, in such manner as to simplify the question of resolution in the least degree. For example: given the coefficients p, q, r, of the cubic equation

$$x^3 + px^2 + qx + r = 0;$$

required the roots, a, b, c. The relations from which we have to proceed are

$$-p = a + b + c$$
$$q = ab + ac + bc, \; -r = abc:$$

and we have to find a root in terms of $p, q,$ Multiplying the first of these equalities by a^2, the second by $-a$, and then adding the three, we find

$$-pa^2 - qa - r = a^3, \text{ or}$$
$$a^3 + pa^2 + qa + r = 0;$$

·hich is just the given equation; so that the uestion of resolution has not advanced a step. ı a similar manner, and with a similar result, e might insulate the root b, or the root c. The ıme thing happens generally, by whatever ıeans we insulate one of the roots; and we ight be sure that it *must* happen, for the simple ason that a, b, c, are involved in the same anner, in the equations from which we set ıt, so that the equations are not changed when b, c, are interchanged in any manner. For om this it follows, that whatever process ads to a final equation involving only a, should ad to one involving only b, or to one involv- g only c, by a simple interchange of the let- rs a and b, or of a and c, throughout; so that e three final equations, being in fact identical ith each other, must suffice separately for the termination of the three roots, and must there- re be identical with the proposed equation. ill, the theorem proved in the last section is a ry important one; it leads to some valuable operties of roots, besides giving a perfectly clear ıtement of the conditions that must be fulfilled the roots of any proposed equation. It was scovered by Harriott, with whose labours and ose of Descartes the systematic theory of equa- ıns may be said to have originated.

7. We proceed to the Transformation of Equa- ıns. As an illustration of the kind of questions at belong to this part of the theory: *Given an uation* $f(x) = 0$; *required a second equation* $y) = 0$, *such, that between the roots* (y) *of the and the roots* (x) *of the* 1*st there may be assigned relation*, $y = F(x)$. If possible, solve the equation $y = F(x)$, considering x as e unknown. Having found thus $x = \varphi(y)$, bstitute $\varphi(y)$ for x in the equation $f(x) = 0$; d the question is solved. For, by the process, e final equation is $f\{\varphi(y)\} = 0$; and, there- e, if $a, b, \ldots l$, be the roots of $f(x) = 0$, the ots (y) of the final equation must be such that $y)$ shall have the values $a, b, \ldots l$. But the o equations

$$\varphi(y) = x, \text{ and } y = F(x),$$

press the same relation between x and y, so ıt if two particular values of x and y satisfy e of the equations, they will satisfy also the ıer. But

$$\varphi(y) = a, = b, = \ldots, = l; \text{ and therefore}$$
$$y = F(a), = F(b) \ldots = F(l):$$

required. When the equation $y = F(x)$ can- t be resolved in relation to x, we must have ourse to higher methods. The following are ıong the most useful elementary problems in ınsformation. 1st, *To form an equation whose ots shall be the reciprocals of those of a given uation* (1). Two numbers are called reciprocals their product = 1. Therefore, in this case the ıations $y = F(x)$ and $x = \varphi(y)$ become $y = \frac{1}{x}$ and $x = \frac{1}{y}$. Substitute $\frac{1}{y}$ for x in eq. (1); and to reduce the resulting equation to the stand- ard form without changing the roots, multiply it by y^n and divide by p_n. 2d, *To change the signs of all the roots of a given equation.* Substitute $-y$ for x; and if the first term of the resulting equation be negative, as it will be when the degree of the equation is odd, change the sign of every term. 3d, *To multiply all the roots of a given equation* (1) *by any number* h. By the question, $y = hx$, or $x = y : h$. Substi- tute $y : h$ for x in (1), and multiply all the terms by h^n. The required equation is

$$y^n + hp_1 y^{n-1} + h^2 p_2 y^{n-2} + \ldots + h^n p_n = 0.$$

4th, *Given an equation in the form* (1), *with nume- rical coefficients, some of them irreducible fractions: it is required to transform it into another equation, with whole coefficients, but still of the form* (1). Find the least common multiple k of the denomi- nators of all the coefficients, and, by the last pro- blem, transform the proposed equation into an- other whose roots are k times greater. It is evi- dent that all the denominators will disappear in the transformation, while the coefficient of the highest power of the unknown is still unity: as required. And it is evident that the multiplier k may be taken less, in most cases, than the least common multiple of the denominators. Contrast the present transformation with the elementary reductions to which equations are often submit- ted, such as clearing of fractions. The object of those reductions is to simplify the equation with a view to resolution; and this end is gained with- out any change in the value of the roots. In the present case the object is similar: the equation is simplified by the transformation; and although the roots are changed in value, they are changed in a *known and simple* manner. The next pro- blem is of the same kind. 5th, *To transform a given equation into another which wants a certain term.* In the given eq. (1) change x into $y + h$: develop the terms $(y + h)^n$, $p_1(y + h)^{n-1}$, &c., by the binomial theorem; and find the equation in the form

$$y^n + q_1 y^{n-1} + \ldots + q_n = 0 \ldots (2)$$

the coefficients $q_1 \ldots q_n$, are rational integer functions of h, independent of y. By equating one of them to zero, we can obtain one or more values of h that will give eq. (2) in the assigned form: and we see thus, that the required trans- formation can be always effected by diminishing each root of the given equation by a certain quantity h. It is found that the removal of the 2d, 3d, .. nth terms of eq. (2) require the reso- lution of equations in h, of the 1st, 2d, .. $(n-1)$th degrees. To remove the last term q_n, we have to solve the given equation, as might have been expected. The second term (and this is the most

important case), is easily seen to be removable by the diminution of each root of the proposed equation by a certain number h. For

$$-p_1 = a + b + \ldots + l$$

and $-q_1 = (a - h) + (b - h) + \ldots (l - h) = -p_1 - nh$; and to make $q_1 = 0$, we have

$$p_1 + nh = 0, \text{ or } h = -\frac{p_1}{n}.$$

Example.—The quadratic equation

$$x^2 + px + q = 0,$$

transformed thus, gives the equation

$$y^2 - \tfrac{1}{4}p^2 + q = 0,$$

which gives the two roots (x) equal to

$$-\tfrac{1}{2}p \pm \tfrac{1}{2}\sqrt{p^2 - 4q}.$$

6th, *To diminish each of the roots of a given equation by any number* h. Here, according to the general method, $y = x - h$, and $x = y + h$; so that the eq. (2) of last problem is that required. But with a view to the resolution of numerical equations, the reader ought to master the following solution: Divide the 1st member $f(x)$ of the given eq. by $x - h$, till the remainder is independent of x: denote the quotient by Q_1 and the remainder by R_1. Divide similarly Q_1 by $x - h$, and let the quotient be Q_2, and the remainder R_2: and proceed in this manner to Q_n and R_n. As the divisor is always $x - h$, and the first dividend is $x^n + \&c.$; the first terms of Q_1, Q_2, &c., must be x^{n-1}, x^{n-2}, &c.; so that $Q_n = 1$. Hence, putting $x - h = y$, we have, by the nature of the process,

$$\begin{aligned} f(x) &= Q_1 y + R_1 \\ Q_1 &= Q_2 y + R_2 \\ Q_2 &= Q_3 y + R_3 \\ &\ldots\ldots\ldots \\ Q_{n-2} &= Q_{n-1} y + R_{n-1} \\ Q_{n-1} &= \quad\;\; y + R_n. \end{aligned}$$

Multiply these equations in order, beginning with the second, by y, y^2, $y^3 \ldots y^{n-1}$; take their sum, and reject the terms $Q_1 y$, $Q_2 y^2$, &c., common to both members: then

$$f(x) = y^n + R_n y^{n-1} + R_{n-1} y^{n-2} + \ldots + R_2 y + R_1.$$

This is an identical equation, y standing for $x - h$; and therefore, if $a, b, \ldots l$, be the roots of the given equation, the second member of the last equality will vanish when $x = a, = b, \ldots = l$; that is when $y\,(= x - h) = a - h, = b - h, \ldots = l - h$. And therefore the equation

$$y^n + R_n y^{n-1} + R_{n-1} y^{n-2} + \ldots + R_1 = 0$$

is that required, its roots (y) being severally less by h than those of the given equation $f(x) = 0$.

8. We proceed now to another kind of transformation, the *depression of an equation*, or the diminution of its degree, *when some of its roots have particular relations to each other.* Upon this subject there are two partial theories of special importance, that of equal roots and that of reciprocal equations: and these we shall take up in order. Let $f(x) = 0$ be any given equation; let $a_1, a_2, a_3 \ldots a_n$, be its roots; let $f_1(x)$ be the diff. coeff. of $f(x)$; then, by sections (3) and (5), we have

$$f(x) = (x - a_1)(x - a_2) \ldots (x - a_n) \;.\;. \;(2)$$

$$f_1(x) = \frac{f(x)}{x - a_1} + \frac{f(x)}{x - a_2} + \ldots + \frac{f(x)}{x - a_n}.$$

$$\text{Let } \frac{f(x)}{x - a_1} = Q_1, \ldots \; \frac{f(x)}{x - a_n} = Q_n:$$

$$\therefore f_1(x) = Q_1 + Q_2 + \ldots + Q_n \;.\;. \;(3)$$

It should be carefully remembered that each of the terms (Q) of equation (3) is composed of all the factors $(x - a)$ of equation (2), one excepted. Supposing now that the equation $f(x) = 0$ has m roots each equal to a_1; it is evident that Q_1 contains $(x - a_1)^{m-1}$ as a factor, and that each of $Q_2, Q_3, \ldots Q_n$, contains either $(x - a_1)^{m-1}$ or $(x - a_1)^m$ as a factor; whence, by equation (3), $f_1(x)$ contains $(x - a_1)^{m-1}$ as a factor. We infer, that if the eq. $f(x) = 0$ has m roots equal to a_1, the two quantities $f(x)$ and $f_1(x)$ have a common factor $(x - a_1)^{m-1}$. Suppose conversely that $f(x)$ and $f_1(x)$ have the common factor $(x - a_1)^{m-1}$. Conceive the terms (Q) of equation (3) distributed into two sets, those of the first set being derived from $f(x)$ by the removal of the factor $(x - a_1)$, and those of the second by the removal of any facto[r] different from $(x - a_1)$: and represent the sum of the first set by B, and that of the second by C. Then

$$f_1(x) = B + C, \text{ or, } B = f_1(x) - C.$$

Now, by hypothesis, $(x - a_1)^{m-1}$ is a factor o[f] $f_1(x)$; it is also, by our assumption, a factor o[f] C, and therefore a factor of the difference $f_1(x)$ — C, and therefore, by the last equation, a factor of B: in other words, $(x - a_1)^{m-1}$ is a facto[r] of the aggregate of those terms (Q) which w[e] derive from $f(x)$ by throwing out the factor $(x - a_1)$. It follows evidently, that $f(x)$ contain[s] the factor $(x - a_1)^m$. And hence, if $f(x)$ an[d] $f_1(x)$ have a common factor $(x - a_1)^{m-1}$, th[e] equation $f(x) = 0$ has m roots each equal to a_1. By a simple extension of this reasoning: if th[e] equation $f(x) = 0$ has m roots equal to a, [p] roots equal to b, q roots equal to c, then, $f(x)$ and $f_1(x)$ have a common divisor—

$$(x - a)^{m-1}(x - b)^{p-1}(x - c)^{q-1};$$

and conversely. Hence, $f(x) = 0$ being th[e] given equation, and (d) being the greatest com[mon] measure of $f(x)$ and $f_1(x)$: *if* (d) *be nume[rical], or independent of* (x) *the equation has n*

equal roots: if d *contain any number of simple factors* (x — a), *the equation has the same number of double roots* (a)*: if* d *contain any number of double factors* $(x - b)^2$, *the equation has the same number of triple roots*, &c. To simplify now the resolution of equations which have equal roots. Denote by u the product of those factors $(x - a)$ of $f(x)$, which correspond to the simple roots; by v the product of the factors, taken in the first degree, which correspond to the double roots; by w the product of the factors, also in the first degree, corresponding to the triple roots: and suppose that there are no roots of a higher degree of multiplicity. Then

$$f(x) = u v^2 w^3;$$

and d being, as above, the G. C. M. of $f(x)$ and its diff. coeff. $f_1(x)$; we have, by the theorem,

$$d = v w^2.$$

In like manner, if e be the G. C. M. of d and the diff. coeff. of d, we find, by the theorem,

$$e = w.$$

Dividing each of these equations by the succeeding;

$$f(x) \cdot d = u v w,\ d : e = v w,\ e = w.$$

Dividing each of these by the succeeding;

$$\frac{f(x) \cdot e}{d^2} = u,\ \frac{d}{e^2} = v,\ e = w.$$

Now, when $f(x)$ is given in the form (1), with numerical coefficients, we can find $f_1(x)$ by the rule in section (1), and then d by the process of G. C. M., and then e in like manner; so that we can find u, v, w, in terms of x. In this way we reduce the resolution of the proposed equation to that of the three equations $u = 0$, $v = 0$, $w = 0$, none of which has equal roots, and of which the degrees, taken altogether, are less than that of the proposed. And a similar method applies to more complex cases.

For example, given the equation

$$x^7 - x^6 - 4x^5 + 4x^4 + 5x^3 - 5x^2 - 2x + 2 = 0;$$

we find successively,

$$f_1(x) = 7x^6 - 6x^5 - 20x^4 + 16x^3 + 15x^2 - 10x - 2;$$

$$d = x^3 - x^2 - x + 1;\ e = x - 1.$$

By divisions, as indicated above, we find

$$f(x) : d = u v w = x^4 - 3x^2 + 2;$$
$$d : e = v w = x^2 - 1;\ e = w = x - 1.$$

And by division again,

$$u = x^2 - 2,\ v = x + 1,\ w = x - 1.$$

The question is reduced to the resolution of the three equations

$$x^2 - 2 = 0,\ x + 1 = 0,\ x - 1 = 0.$$

The given equation has two simple roots $\pm\sqrt{2}$, a double root -1, and a triple root $+1$: or

$$f(x) = (x^2 - 2)(x + 1)^2 (x - 1)^3.$$

9. An equation is called reciprocal when it is such, that if a be any one of its roots, $\frac{1}{a}$ is also a root; so that all the roots will be reproduced, though in a different order, by the division of unity by each of them. *Given an equation in the form* (1); *by what characters may we know that it is a reciprocal equation?* Substituting $\frac{1}{x}$ for x in eq. (1), and reducing, we obtain the equation

$$x^n + \frac{p_{n-1}}{p^n} x^{n-1} + \frac{p_{n-2}}{p^n} x^{n-2} + \ldots$$
$$+ \frac{p_1}{p_n} x + \frac{1}{p_n} = 0 \ldots \quad (2.)$$

The roots of eq. (2) are the reciprocals of those of those of (1). It follows, that if (1) be a reciprocal equation, (1) and (2) have the same roots, and are therefore identical with each other: and conversely. But we can identify (1) and (2) only by equating their coefficients in order. Therefore

$$\frac{p_{n-1}}{p_n} = p_1,\ \frac{p_{n-2}}{p} = p_2 \ldots \frac{p_1}{p_n} = p_{n-1},$$
$$\frac{1}{p_n} = p_n.$$

From the last equality we find $p_n^2 - 1$: therefore

$$p_n = \pm 1,\ p_{n-1} = \pm p_1,\ p_{n-2} = \pm p_2, \text{\&c.}$$

One or other of these two sets of relations among the coefficients is necessary and sufficient to constitute (1) a reciprocal equation. Observe, however, that if n be even, and p be the coefficient of the middle term, we find as above, $p = \pm p$; and therefore, in the case of the upper sign, p may have any value, but in the case of the lower, p must vanish. Hence, *in a reciprocal equation, the coefficients of terms equidistant from the extremes must be equal and of like signs, or equal and of unlike signs: also, when the equation is of even dimensions, and the corresponding terms have unlike signs, there must be no middle term.* On the resolution of such equations, observe, first, that all cases may be reduced to that of a reciprocal equation of an even degree, with its last term positive. Consider the equations

$$x^5 + a x^4 + b x^3 + b x^2 + a x + 1 = 0,$$
$$x^5 + a x^4 + b x^3 - b x^2 - a x - 1 = 0.$$

It appears by substitution, that -1 is a root of the first, and $+1$ of the second: therefore the first members are divisible respectively by $x + 1$ and $x - 1$. Now it is evident, that in the first

of those quotients, we should have obtained the same coefficients in the same order if we had taken the terms of divisor and dividend in the reverse order ($1 + x$, $1 + ax +$ &c.); and likewise in the second quotient, if we had reversed the order and changed all the signs. Hence, the resulting equations of the fourth degree are still reciprocal, and their last terms are $+1$. The same reasoning applies to a reciprocal equation of whatever odd degree: and it appears, in like manner, that when the equation is of even dimensions, and its last term is -1, it may be depressed, by the removal of the factor $x^2 - 1$, into a reciprocal equation whose last term is $+1$. And therefore, by changing n into $2n$ in eq. (1), and introducing the proper conditions, $p_{2n} = 1$, $p_{2n-1} = p_1$, &c., we shall obtain a general form of reciprocal equation, whose solution will suffice for all cases. Consider then the equation

$$x^{2n} + p_1 x^{2n-1} + p_2 x^{2n-2} + \ldots + p_2 x^2 + p_1 x + 1 = 0.$$

Connect by pairs the terms equidistant from the extremes, and divide the equation by x^n: it then becomes

$$\left(x^n + \frac{1}{x^n}\right) + p_1\left(x^{n-1} + \frac{1}{x^{n-1}}\right) + \ldots + p_{n-1}\left(x + \frac{1}{x}\right) + p_n = 0 \quad .. \ (2.)$$

Let $x + \frac{1}{x} = z$, and $x^n + \frac{1}{x^n} = R_n$

Multiplying together the two equations

$$R_{n-1} = x^{n-1} + \frac{1}{x^{n-1}}, \text{ and}$$

$$z = x + \frac{1}{x}\text{; we find}$$

$$z R_{n-1} = \left(x^n + \frac{1}{x^n}\right) + \left(x^{n-2} + \frac{1}{x^{n-2}}\right)$$

$$= R_n + R_{n-2}.$$

$$\therefore R_n = z R_{n-1} - R_{n-2}.$$

This relation gives the value of each quantity R_n or $x^n + \frac{1}{x^n}$, in terms of z, when the values of the preceding two are known. Thus, $R_1 = z$,

$$R_2 = x^2 + \frac{1}{x_2} = \left(x + \frac{1}{x}\right)^2 - 2 = z^2 - 2.$$

$$R_3 = z R_2 - R_1 = z(z^2 - 2) - z = z^3 - 3z.$$
$$R_4 = z^4 - 4z^2 + 2.$$
$$R_5 = z^5 - 5z^3 + 5z, \text{ \&c.}$$

Substituting in eq. (2) the values of R_1, R_2, .. R_n, in terms of z, we reduce the proposed equation of the $2n$th degree in x to one of the nth degree in z. Finally,

$$x + \frac{1}{x} = z, \text{ or } x^2 - zx + 1 = 0$$

$$\therefore x = \frac{z}{2} \pm \frac{1}{2}\sqrt{z^2 - 4};$$

and we have only to substitute in this expression the n values of z, in order to find the $2n$ values of x. As an example, consider the equation

$$x^5 - 1 = 0.$$

When the factor $(x - 1)$, corresponding to the root 1 is removed, the equation becomes

$$x^4 + x^3 + x^2 + x + 1 = 0\text{; or}$$

$$\left(x^2 + \frac{1}{x^2}\right) + \left(x + \frac{1}{x}\right) + 1 = 0\text{: or}$$

$$(z^2 - 2) + z + 1 = z^2 + z - 1 = 0.$$

$$\therefore z = \frac{-1 \pm \sqrt{5}}{2}$$

$$\therefore z^2 = \frac{6 \mp 2\sqrt{5}}{4}\text{; and } z^2 - 4 = \frac{-10 \mp 2\sqrt{5}}{4}$$

$$\therefore \sqrt{z^2 - 4} = \frac{1}{2}\sqrt{10 \pm 2\sqrt{5}} \cdot \sqrt{-1}.$$

Substituting successively these two values of $\sqrt{z^2 - 4}$, with the corresponding values of z in the expression

$$\frac{z}{2} \pm \frac{1}{2}\sqrt{z^2 - 4},$$

which is the value of x in terms of z, we find that the five roots of the proposed equation, or the five 5th roots of unity are, 1,

$$\frac{-1 + \sqrt{5}}{4} \pm \frac{\sqrt{10 + 2\sqrt{5}}}{4}\sqrt{-1},$$

$$\frac{-1 - \sqrt{5}}{4} \pm \frac{\sqrt{10 - 2\sqrt{5}}}{4}\sqrt{-1}$$

10. The theory of Elimination, or of the resolution of equations which involve more than one unknown, must be noticed here, however, briefly, as it is one of the most important subjects in algebra. The reader, we suppose, has met with examples of simultaneous equations, and with the methods of elimination by addition, by comparison, and by substitution, as applied to equations of the first degree. There is another method, that of Indeterminate Multipliers, which is not usually consigned to the elements, although simple in principle, and very extensively used. Required x and y from the two simultaneous equations

$$\left.\begin{array}{l} a_1 x + b_1 y = c_1 \\ a_2 x + b_2 y = c_2 \end{array}\right\} \ (2.)$$

Multiply the second equation by the quantity m, and then add to the first:

$\therefore (a_1 + ma_2)\,x + (b_1 + mb_2)\,y = c_1 + mc_2$; (3)

ınd to eliminate y, assume in eq. (3)

$$b_1 + m\,b_2 = 0, \text{ or } m = -\frac{b_1}{b_2}:$$

$$\therefore \left(a_1 - \frac{a_2\,b_1}{b_2}\right)x = c_1 - \frac{b_1\,c_2}{b_2};$$

$$\therefore (a_1\,b_2 - a_2\,b_1)\,x = b_2\,c_1 - b_1\,c_2.$$

'roceeding in the same manner, or by substitu-ion, to determine y, we find

$$x = \frac{b_2\,c_1 - b_1\,c_2}{a_1\,b_2 - a_2\,b_1};\ y = \frac{a_2\,c_1 - a_1\,c_2}{a_1\,b_2 - a_2\,b_1}. \quad (4.)$$

'he process depends upon this, that the eq. (3) nust admit of the solution common to the given quations, whatever be the value of m. As a econd question: Required the values of x and y hat satisfy the three equations

$$\left.\begin{array}{l} a_1\,x + b_1\,y = c_1 \\ a_2\,x + b_2\,y = c_2 \\ a_3\,x + b_3\,y = c_3 \end{array}\right\} \quad (5.)$$

'rom the first and second equations find, as in he last question, the values of x and y, and rom the first and third find two other values: hen, if the latter values of x and y are equal o the former, the third equation is superfluous, nd if unequal, the three given equations are ıconsistent. More simply, to make the given quations consistent with each other, substitute he values (4) of x and y in the 3d of equations 5). This gives the *equation of condition*

$$_3\,(b_2\,c_1 - b_1\,c_2) + b_3\,(a_2\,c_1 - a_1c_2) = c_3\,(a_1\,b_2 - a_2\,b_1).$$

imilar principles apply to higher questions, vhenever the number of equations, independent f each other, exceeds the number of unknowns. ls a third question: Required the values of , y, z, that satisfy the three equations

$$\begin{array}{l} a_1\,x + b_1\,y + c_1\,z = k_1, \\ a_2\,x + b_2\,y + c_2\,z = k_2, \\ a_3\,x + b_3\,y + c_3\,z = k_3. \end{array}$$

Iultiply the second equation by m and the third y n, and add the products to the first. In the esulting equation equate the coefficients of y nd of z to 0. This process gives the three quations

$$\left.\begin{array}{l} b_1 + mb_2 + nb_3 = 0 \\ c_1 + mc_2 + nc_3 = 0 \end{array}\right\} \quad (6.)$$

$$a_1 + ma_2 + na_3)\,x = k_1 + mk_2 + nk_3. \quad (7.)$$

'he equations (6), solved as in the first question, ive

$$m = \frac{b_3\,c_1 - b_1\,c_3}{b_2\,c_3 - b_3\,c_2} = \frac{\text{M}}{\text{D}};$$

$$n = \frac{b_1\,c_2 - b_2\,c_1}{b_2\,c_3 - b_3\,c_2} = \frac{\text{N}}{\text{D}}.$$

Substituting these values of m and n in eq. (7) we find

$$x = \frac{k_1\,\text{D} + k_2\,\text{M} + k_3\,\text{N}}{a_1\,\text{D} + a_2\,\text{M} + a_3\,\text{N}}.$$

From this expression we obtain x in terms of the given coefficients, by substituting for D, M, N, their values $b_2\,c_3 - b_3\,c_2$, &c.: and from the value of x written at length, that of y may be found by an interchange of the letters a and b without change of suffixes, and that of z from that of x by an interchange of a and c.

11. A complete equation of the nth degree, in x and y, after suitable reductions, may always be written thus:

$$\text{A}x^n + \text{B}x^{n-1} + \text{C}x^{n-2} + \ldots + \text{K}x + \text{L} = 0;$$

where the coefficients A, B, C . . . L, are of the forms,

$$a,\ b + cy,\ d + ey + fy^2 \ldots$$
$$g + hy + ky^2 + \ldots + py^n.$$

Given two such equations, one of the nth degree, another of the mth: required all the systems of values which, when substituted for x and y shall satisfy the two equations. The equations may be represented, for brevity, as

$$f(x, y) = 0,\ \varphi(x, y) = 0.$$

If we restrict ourselves to questions having a finite number of solutions, it is evident that $f(x, y)$ and $\varphi(x, y)$ can have no common factor involving x or y, or both x and y. Excluding these cases, there is a simple test by which we can discover suitable values of either unknown. Suppose b to be a suitable value of y. Then, by the question the two equations

$$f(x, b) = 0 \text{ and } \varphi(x, b) = 0,$$

must have one or more roots in common: and therefore, the polynomials $f(x, b)$ and $\varphi(x, b)$ must have a common divisor, F (x), involving x; and the roots of the equation F $(x) = 0$ are the values of x corresponding to the value b of y. Suppose conversely that $f(x, b)$ and $\varphi(x, b)$ have a common divisor F (x): then any root a of the eq. F $(x) = 0$, when substituted for x, will satisfy the two equations $f(x, b) = 0$ and $\varphi(x, b) = 0$; and therefore the values a and b of x and y form a solution of the given equations. Upon these principles is founded the method of elimination by the process of greatest common measure. Arrange the polynomials $f(x, y)$ and $\varphi(x, y)$ with respect to x, and apply to them the process of G. C. M. till a remainder is obtained involving only y. Let F (x, y) be the last divisor, and $\psi(y)$ the remainder: then if

$$\psi(y) = 0, \quad (8.)$$

the given polynomials have a common measure F (x, y). Therefore the roots of (8) are, according to what precedes, suitable values of y; and

if any one of them, b, be substituted in the eq. $F(x, y) = 0$ for y, it will give corresponding value of x. Such is the principle of the method; but in practice, this theoretical simplicity disappears, except in rare cases; the principal difficulty arising from the introduction of factors involving y, which give values foreign to the question. The necessary details are given in the most of the good elementary works, such as Bourdon's *Algebre*, *Lefebure de Fourcy's*. There are other elementary points connected with this subject (such as the extension of the method of indeterminate multipliers to equations of the 2d and higher degrees), which are of great interest, but must be passed for want of space. There is an interesting and precise discussion of the elements of this subject in Peacock's *Algebra*. The object of every method of elimination is, as in the above process, to obtain a *final equation* $\psi(y) = 0$ whose roots are the values of one of the unknowns proper to the question. And a method of elimination is considered *perfect* when it leads to a final equation that admits of all the values of y proper to the question, and of no others. The most general method of this kind is that depending on the properties of *symmetrical functions*.

12. A function of the roots of an equation is called symmetrical, when the roots are all involved in it in precisely the same manner, so that the function is not changed when the roots are interchanged for each other in whatever order we please. A function called symmetrical is usually understood to be *rational*, or such, that none of the roots is involved in it under a radical sign. It will be remembered that each coefficient of equation (1) is a rational symmetrical function of the roots. According to Newton's method, the first question upon this subject is, to find the sum (s_1) of the first powers of the roots, the sum (s_2) of their second powers, the sum (s_3) of their third powers, &c., in terms of the coefficients of the equation. Newton effected this in a simple and elegant manner by means of the theorem which has been proved in the end of section (5). By actual division, or by the statement made in section (3), we find

$$\frac{f(x)}{x-a} = x^{n-1} + (a+p_1)\,x^{n-2} + (a^2+p_1 a+p_2)\,x^{n-3} + \ldots$$

Substituting successively $b, c, \ldots l$, for a, in this expression, and adding all the equations together, we find

$$\frac{f(x)}{x-a} + \frac{f(x)}{x-b} + \ldots + \frac{f(x)}{x-l} =$$

$$nx^{n-1} + (s_1 + np_1)\,x^{n-2} + (s_2 + p_1 s_1 + np_2)\,x^{n-3} + \ldots$$

To identify the second member of this equation with the expression

$$nx^{n-1} + (n-1)\,p_1 x^{n-2} + (n-2)\,p_2\,x^{n-3} + \ldots$$

which we know, by section (5), to be identical with it, we equate the coefficients of like powers of x. Therefore

$$s_1 + np_1 = (n-1)\,p_1,$$
$$s_2 + p_1 s_1 + np_2 = (n-2)\,p_2,$$
$$\ldots\ldots\ldots\ldots, \text{ or}$$

$$(1.)\left\{\begin{array}{l} s_1 + p_1 = 0, \\ s_2 + p_1 s_1 + 2p_2 = 0, \\ s_3 + p_1 s_2 + p_2 s_1 + 3p_3 = 0, \\ \ldots\ldots\ldots\ldots \\ s_{m-1} + p_1 s_{m-2} + \ldots + p_{m-2}\, s_1 + (m-1) \\ \qquad p_{m-1} = 0. \end{array}\right.$$

The first of these equations gives s_1, the second s_2, the third s_3, &c.; the highest sum determined thus far being s_{n-1}. Thus,

$$s_1 = -p_1,\; s_2 = p_1^2 - 2\,p_2,$$
$$s_3 = -p_1^3 + 3p_1 p_2 - 3p_3,$$
$$s_4 = p_1^4 - 4p_1^2 p_2 + 4p_1 p_3 + 2p_2^2 - 4p_4;\ \&c.$$

To find the sums of similar powers of the roots whose indices exceed $n-1$, we proceed thus. The given equation, multiplied by x^m, is

$$x^{n+m} + p_1\,x^{n+m-1} + \ldots\ldots + p_{n-1}\,x^{m+1} + p_n\,x^m = 0.$$

Substituting successively for x each of the roots $a, b, \ldots l$, and adding the resulting equations together, we find,

$$s_{n+m} + p_1\,s_{n+m-1} + \ldots\ldots + p_{n-1}\,s_{m+1} + p_n\,s_m = 0.$$

Giving here to m the values 0, 1, 2, 3, &c., and observing that $s_0 = n$, we find the following relations:

$$(2.)\left\{\begin{array}{l} s_n + p_1\,s_{n-1} + \ldots + p_{n-1}\,s_1 + np^n = 0, \\ s_{n+1} + p_1\,s_n + \ldots + p_{n-1}\,s_2 + p_n\,s_1 = 0, \\ s_{n+2} + p_1\,s_{n+1} \ldots + p_{n-1}\,s_3 + p_n\,s_2 = 0; \\ \qquad \&c. \end{array}\right.$$

The sums $s_1, s_2, \ldots s_{n-1}$, are already known by equations (1); the sum s_n is therefore known by the first of equations (2), the sum s_{n+1} by the second; &c. We can find in the same manner the sums of similar powers of the roots, with negative exponents, by making m equal to -1, -2, &c.: but we find these values more easily by changing x into $\frac{1}{x}$ in the proposed equation, and then calculating, as above, the sums of similar powers, with positive exponents, of the roots of the transformed equation. The following inferences are obvious:—The expressions of the sums $s_1, s_2, \ldots$ in terms of $p_1, p_2, \ldots$ will not contain any denominator. If the coefficients (p) are integers, the sums (s) will be also integers. If n sums of similar powers are given, such as s_1,

$_2, \ldots s_n$, we can determine all the coefficients (p) of the equation by means of (1) and (2).

The next question is to find the value of any *double function*, or of the sum of all possible terms ($a^\mu b^\nu$), where a is any one root and b any other root; the numbers μ, ν, being integers. Such a function is represented by $\Sigma\ (a^\mu b^\nu)$, Σ standing for "sum of;" thus, $p_2 = \Sigma\ (a\ b)$. According to the previous notation, we have

$$s_\mu = a^\mu + b^\mu + \ldots + l^\mu;$$
$$s_\nu = a^\nu + b^\nu + \ldots + l^\nu.$$

The product of the second members consists of two parts; first, the sum of all possible terms of the form $a^{\mu+\nu}$; second, the sum of all possible terms of the form $a^\mu b^\nu$. Therefore

$$s_\mu \cdot s_\nu = s_{\mu+\nu} + \Sigma\ (a^\mu b^\nu); \text{ or}$$
$$\Sigma\ (a^\mu b^\nu) = s_\mu s_\nu - s_{\mu+\nu}.$$

And hence, any double function whatever may be expressed, under a rational and integer form, by the coefficients (p) of the given equation. The last formula is altered if $\mu = \nu$; for then $a^\mu b^\nu = a^\nu b^\mu$, so that the terms of the double function become equal, by pairs, and the function is reduced to $2\Sigma\ (a^\mu b^\mu)$. Therefore

$$\Sigma\ (a^\mu b^\mu) = \frac{1}{2}\left\{(s_\mu)^2 - s_{2\mu}\right\}$$

The denominator 2 disappears when we substitute for s_μ and $s_{2\mu}$. Having found the values of all simple and double functions of the roots in terms of the coefficients (p), we have to find next the value of any triple function $\Sigma\ (a^\lambda b^\mu c^\nu)$; and this we do by multiplying together the double function $\Sigma\ (a^\lambda b^\mu)$ and the simple function s^ν, and then proceeding in the same manner as above. Without having the work placed before him, the reader will easily find the value of the triple function, in terms of simple functions, and in its simplest form, to be

$$s_\lambda s_\mu s_\nu - s_\lambda s_{\mu+\nu} - s_\mu s_{\lambda+\nu} - s_\nu s_{\lambda+\mu} + 2\, s_{\lambda+\mu+\nu}.$$

And the same method applies to the determination of quadruple functions, and of the successive symmetrical functions of higher orders. Each of the symmetrical functions yet considered is homogeneous, or such, that the exponents of the letters are the same in all its terms. But it is easily seen that a rational symmetrical function which is not homogeneous must be the sum of two or more such functions which are homogeneous; and further, that every non-integer symmetrical function may be reduced to a quotient of one integer symmetrical function by another. Hence, the expression of any rational symmetrical function whatever, in terms of the coefficients, may be derived by mere addition and division from the expressions of the simple, double, triple . . . functions above investigated. From all which it follows that, by Newton's method, *every rational symmetrical function of the roots of an equation is expressible rationally by the coefficients of the equation.* The properties of symmetrical functions are of the greatest importance in the theory of equations. They have therefore been studied with great diligence by several eminent algebraists, particularly Waring, Lagrange, and Cauchy; whose inquiries have led to some very remarkable methods and results. But to these matters want of space prevents further reference.

13. We have already discussed a few simple problems in the transformation of equations. Upon the same subject there are several important questions, of a higher kind, which are cases of the following:—Given an equation; required any assigned rational function of two or more of its roots. In the investigation of such questions it is generally found, that symmetrical functions of the roots of the given equation present themselves naturally, and as the principal elements of the solution. One example will be given here, which is of great interest historically and upon other accounts. *To find an equation whose roots shall be the squares of the differences of the roots of a given eq.* (1.) The n roots of eq. (1) being $a, b, \ldots l$; those of the required equation are $(a-b)^2$, $(a-c)^2$, $(b-c)^2$, &c.; and their number is the number of combinations (not permutations) of n letters taken two together, and therefore equal to $\frac{n\,(n-1)}{2} = \mu$. The required equation is therefore of degree μ, and may be represented by

$$z^\mu + q_1 z^{\mu-1} + q_2 z^{\mu-2} + \ldots + q_\mu = 0. \ (2.)$$

The question is to find the coefficients (q), which are evidently symmetrical functions of the roots of eq. (1). The following process is due to Lagrange. As in the last section, let $s_1, s_2, \ldots$ be the sums of similar powers of the roots of eq. (1); let $S_1, S_2, \ldots$ be the like functions of the roots of eq. (2); and let

$$\phi\,(x) = (x-a)^{2m} + (x-b)^{2m} + \ldots + (x-l)^{2m}.$$

By substitution of $a, b, \ldots l$, successively for x in the last equation, and by addition of the results, it is easily found that

$$\phi\,(a) + \phi\,(b) + \phi\,(c) + \ldots + \phi\,(l) = 2\,S_m.$$

Developing now the different terms $(x-a)^{2m}$, &c., of $\phi\,(x)$, by the binomial theorem, we find that $\phi\,(x) =$

$$\begin{aligned} &x^{2m} - 2\,m\,a\,x^{2m-1} + \ldots + a^{2m} \\ &+ \ldots\ldots\ldots\ldots\ldots\ldots \\ &+ x^{2m} - 2\,m\,l\,x^{2m-1} + \ldots + l^{2m}; \end{aligned}$$

where the reader, if he finds it necessary, may supply a few additional terms. Adding the n polynomials contained in the second member of the last equation, we find

$\phi(x) = n x^{2m} - 2 m s_1 x^{2m-1} + \ldots + s_{2m}.$

Substituting successively $a, b, \ldots l$ for x in this equation, and adding the resulting equations, we obtain the following value of $\phi(a) + \phi(b) + \ldots + \phi(l)$, or of $2 S_m$;

$$n s_{2m} - 2 m s_1 s_{2m-1} + \frac{2m(2m-1)}{1 \cdot 2} s_2 s_{2m-2} - \ldots + n s_{2m}.$$

It is easily seen that in this expression the terms equidistant from the extremes are equal. Connecting equal terms, and dividing by 2, we find

$$S_m = n s_{2m} - 2 m s_1 s_{2m-1} + \frac{2m(2m-1)}{1 \cdot 2} s_2 s_{2m-2} - \ldots \pm \frac{1}{2} \frac{2m(2m-1)\ldots(m+1)}{1 \cdot 2 \cdot 3 \ldots m} s_m s_m.$$

To find now the coefficients (q) in terms of the coefficients (p) of eq. (1):

1*st.* Calculate the sums $s_1, s_2, \ldots s_n \ldots s_{2\mu}$, in terms of the coefficients (p), by Newton's formulas.

2*d.* Calculate the sums $S_1, S_2, \ldots S_\mu$, in terms of (p), by the formula last obtained. Thus, making $m = 1, = 2$; $S_1 = n s_2 - s_1 s_1$, $S_2 = n s_4 - 4 s_1 s_3 + 3 s_2 s_2$, &c.

3*d.* Calculate the coefficients (q) in terms of (p) by Newton's formulas, $q_1 + S_1 = 0$, &c. These operations are extremely prolix, when n is greater than 2 or 3. There is another very interesting question, of the same class with that now solved: Required an equation whose roots shall be equal to $a + b + k a b$, where a is any root of a given eq. (1), b any other root of (1), and k any given number. By a simple course of reasoning, in connection with this question, Lagrange has proved, that when the equation $f(x) = 0$ is of even dimensions, $f(x)$ is decomposable into real quadratic factors; seeming to prove thus the fundamental theorem which we have assumed, that every equation has at least one root real or imaginary. Lagrange's investigation, considered as establishing this last result, is certainly open to objection: it is not perfectly clear that the thing to be proved is not virtually assumed in the proof, the laws of the composition and transformation of equations being assumed throughout. Dr. Peacock has obviated this objection by presenting the proof in another form, which, though not in all respects quite satisfactory, is certainly rigorous, and well worthy of attention. Lagrange's proof may be found in the appendix to Bourdon's *Algebre;* the other in a paper by Dr. Peacock, contained in the *Reports of the British Association* for 1833, where there is much valuable information to be had upon the theory of equations and other branches of analysis.

14. The properties of symmetrical functions find their most useful application in the following method of elimination. Let

$$x^m + p_1 x^{m-1} + \ldots + p_{m-1} x + p_m = 0 \quad (1.)$$
$$x^n + q_1 x^{n-1} + \ldots + q_{n-1} x + q_n = 0 \quad (2.)$$

be two equations in x and y, of degrees m and n. The coefficients p and q are all rational integer functions of y; and their dimensions are equal to their suffixes, if the equations are complete. Let $a, b, c, \ldots l$, be the m roots (x) of eq. (1.) Substituting these in eq. (2), we find the m results

$$(4.)\begin{cases} a^n + q_1 a^{n-1} + \ldots + q_{n-1} a + q_n, \\ b^n + q_1 b^{n-1} + \ldots + q_{n-1} b + q_n, \\ \ldots\ldots\ldots\ldots\ldots\ldots \\ l^n + q_1 l^{n-1} + \ldots + q_{n-1} l + q_n. \end{cases}$$

Let V be the product of these polynomials; then

$$V = 0 \quad (5.)$$

is the final equation resulting from the elimination of x between the equations (1) and (2); for it is satisfied by every value of y that makes any of the quantities (4) vanish, and by no others. Now, V is evidently a symmetrical function of the roots of (1); for when $a, b, c, \ldots l$, are interchanged in any manner for each other, the only effect upon V is a change in the order of its factors. And hence, without obtaining the expressions (4) by the resolution of eq. (1), which is often impossible, we can express the product V in terms of the coefficients (p) and (q) by processes of which the elements have been already indicated (sect. 11); and the question is then reduced to the resolution of the final equation, which involves only y. The method is open to one objection, that the operations required by it are in most cases very prolix. On the other hand, it gives a final equation, which admits of all the roots, and those only that are proper to the question. It leads also, in a simple manner, to the theorem of Bezout, which is, *that the degree of the final equation is at most equal to the product of the degrees of the given equations.* The proof may be indicated briefly, for the case of two equations. Retaining the above notation, each term of the polynomial V is the product of m terms taken separately from the m polynomials (4), and may therefore be represented by

$$q_{n-\alpha}\, q_{n-\beta} \ldots q_{n-\lambda} \cdot a^\alpha b^\beta \ldots l^\lambda.$$

As V is symmetrical in relation to $a, b, \ldots l$, it must contain all the terms of this form obtainable by interchange of the *constant* indices $\alpha, \beta, \ldots \lambda$, for each other; the sum of which terms is

$$q_{n-\alpha}\, q_{n-\beta} \ldots q_{n-\lambda}\, \Sigma a^\alpha b^\beta \ldots l^\lambda.$$

V being the sum of a set of expressions derived from this by the assignation of particular values to $\alpha, \beta, \ldots \lambda$; we have only to find the degree of this expression in relation to y. Now, by hyp. the dimensions (in y) of the coefficients (q) are equal to their suffixes; therefore, the dimensions of the product $q_{n-\alpha}\, q_{n-\beta} \ldots q_{n-\lambda}$ are

ι — α) + (n — β) . . . . + (n — λ), or $m n$ — α + β + . . . + λ). Next, with regard to Σa^{α} ³ . . . l^{λ}; it is expressible in terms of the sums , s_2, . . . in an integer formula, which is of the egree $\alpha + \beta + \ldots + \lambda$ in relation to the oots a, b, &c., of eq. (1), as we have seen particularly in the end of sect. 11, with regard to the ouble and triple functions; and this expression reducible to another, in terms of the coefficients) of eq. (1), which, by Newton's formula, is of e same degree, $\alpha + \beta + \ldots + \lambda$, in relation y. Adding $\alpha + \beta + \ldots + \lambda$ to the quan-y $m n - (\alpha + \beta + \ldots + \lambda)$ previously und, we obtain $m n$ as the degree of the final uation. In particular cases (when the equa-ons are incomplete), the degree may be less an $m n$. Simple rules have been obtained for ing the degree of the final equation precisely all cases, as may be seen in a memoir by inding in the *Journ. de Mathem.*, tome vi. e method of elimination by symmetrical func-ons has been extended, in a very elaborate form, rticularly by Poisson, to any number of equa-ns. But returning to the simplest case, that two equations, it is evident that the method es not enable us to find directly the values of second unknown which correspond to the ots of the final equation. To supply this want, ouville has originated a beautiful method: *urn. de Mathem.*, tome xii. He introduces a riable t, connected with x and y by the relation

$$t = x + \alpha y, \text{ or } x = t - \alpha y \qquad (2.)$$

eing an indeterminate parameter. Substitut-; $t - \alpha y$ for x in the given equations, and inating y, he obtains an equation

$$f(t, \alpha) = 0; \qquad (3.)$$

ence, putting $\alpha = 0$, and therefore, by (2), $= x$, he obtains the final equation

$$f(x, 0) = 0. \qquad (4.)$$

e values of y corresponding to the roots (x) of (4) are obtained simply, in virtue of the rela-ns (2) and (3), in terms of

$$\frac{d \cdot f(t, \alpha)}{d t} \text{ and } \frac{d \cdot f(t, \alpha)}{d \alpha}.$$

. Resolution of Numerical Equations.

To find the roots of an equation exactly, or proximately, when the coefficients are given mbers. This inquiry has engaged the atten-n of the ablest mathematicians of modern times, d it is now completely solved, at least in the st important branch of it, that concerning l roots. And what is of the greatest practical sequence, the solution has been reduced to nite arithmetical rules, which are general and ple, and in all cases easily applied. Only the re interesting investigations and results of an mentary kind can be noticed here, and these y briefly.

.5. *Given an equation with integer coefficients; required the integer roots.* We should naturally proceed here by trial, substituting particular integers for x, and retaining those as roots that satisfy the equation. To avoid the excessive labour often connected with such work, we may employ the method of divisors. Let

$$a x^4 + b x^3 + c x^2 + d x + e = 0,$$

be an equation with integer coefficients; and let r be a root, also an integer: then

$$\frac{e}{r} = - a r^3 - b r^2 - c r - d.$$

The second member of this equation is a whole number, by hypothesis: therefore r is a divisor of e. Again, transposing $-d$ to the first side, and dividing by r, we find a quotient

$$- a r^2 - b r - c,$$

which also is a whole number: therefore r is a divisor of $\frac{e}{r} + d$. Proceeding thus, we obtain the following theorem: The coefficients of an equation being integers, and r being an integer root; r is a divisor of the last term: and if the quotient be added to the coefficient of x, r is a divisor of the sum: and if this quotient be added to the coefficient of x^2, r is a divisor of this sum: and so on, till we reach the last quotient, which, with the coefficient of the highest power of x, gives a sum zero. The use of this theorem, in connection with our present question, is obvious; it excludes, first, all integers as not roots, except the divisors of the last term; it excludes of these all except those that fulfil the second condition; and so on, to the last condition, which excludes all integers not roots, that may have fulfilled all the previous conditions. There is another rule of exclusion, that was given by Newton, and that saves often a good deal of work. Upon the hypothesis of the former theorem; let A be the value of the first member of the equation when $x = 1$, and B its value when $x = -1$; then, A and B are divisible by $r - 1$ and by $r + 1$ respectively. The proof is left to the reader. Example:

$$x^4 - 11 x^2 + 14 x - 24 = 0.$$

Here A = — 20, and B = — 48. Of the divisors positive and negative of — 24, which are fifteen in number, the last theorem excludes all but the four, 2, 3, — 3, — 4. Observing then that the coefficient of x^3 is 0, the first theorem is applied in the following table of simple work, each second line containing the quotients:—

2	3	— 3	— 4
— 12	— 8	8	6
2	6	22	20
1	2		— 5
— 10	— 9		— 16
— 5	— 3		4
. . . .	. . . .	. . . .	. . . .
	— 1		— 1
	0		0

So that the only integer roots are 3 and -4. If the equation has two or more integer roots equal to each other, the above process does not determine them. But generally, when several integer roots a, b, &c., have been found, the given equation should be depressed by the removal of the factors $x-a$, $x-b$, &c., as the transformed equation is simpler and more manageable in all respects than the proposed. A very simple method of effecting this will be given in the following section. The equation being transformed in this manner, the mode of examination for equal roots is obvious: it is just a repetition upon the transformed equation, of the process formerly applied to the given one: only we may restrict ourselves to numbers which we have already found to be roots. In practice, we might proceed in an equally simple manner, by finding the successive differential coefficients $f_1(x), f_2(x)$, &c., of $f(x)$, and then finding the integer roots, if any, common to the equations

$$f(x)=0, f_1(x)=0, f_2(x)=0: \text{ \&c.}$$

Having found the integer roots, and having removed the corresponding factors: required the remaining commensurable roots. These must be irreducible fractions; and the following theorem shows when there are such roots, and how they are to be found. *The coefficients of the equation being whole numbers, and that of the highest power of* (x) *being* $+1$, *all the commensurable roots are integers.* For let

$$x^4 + ax^3 + bx^2 + cx + e = 0$$

be such an equation; and suppose the irreducible fraction $\frac{m}{n}$ to be a root. Substituting $\frac{m}{n}$ for x, and multiplying the equation by n^3, we find

$$\frac{m^4}{n} = -am^3 - bm^2n - cmn^2 - en^3.$$

The second member is an integer, and the first is not, because m is prime to n: so that the equality is impossible; no fractional number can satisfy the given equation. Given an equation then, in the form (1), with integer coefficients; there can be no question about commensurable roots other than integers. Given an equation in any other form, reduce it first to an equation whose coefficients are all whole numbers; and then, k being the coefficient of the highest power of x, transform this equation into another (section 7) whose roots are k times greater. The final equation is of the form (1); therefore, to find all the commensurable roots of the given equation, find all the integer roots of the latter, and divide each by k.

16. To divide the first member $x^n + p_1x^{n-1} + \ldots + p_n$ of a numerical equation by $x-r$, where r is a given number; we may proceed simply as follows, by "Detached Coefficients."

$$\begin{array}{llllll}
1 & p_1 & p_2 & \ldots\ldots & p_{n-1} & p_n \quad \underline{|r} \\
 & \underline{r} & \underline{rq_1} & & \underline{rq_{n-2}} & \underline{rq_{n-2}} \\
 & q_1 & q_2 & \ldots\ldots & q_{n-} & \mathrm{R}
\end{array}$$

Write the coefficients in a line, and $+r$ to the right: multiply r by the first coefficient 1, place the product under p_1, and add to p_1, getting q_1: multiply q_1 by r, place the product under p_2, and add, getting q_2, &c., to the last sum R. The quotient required is $x^{n-1} + q_1x^{n-2} + q_2x^{n-3} + \ldots + q_{n-1}$, and the remainder is R. The principle of the method has been stated already (section 1); but the process may be considered simply as the entire work of the division, all writing being omitted that we can safely dispense with. The utility of this short method will be better seen at a future stage: but observe here, that if r is a root, $\mathrm{R}=0$, and conversely; which gives a simple test for discovering integer roots, and for obtaining at the same time the depressed equations resulting from the removal of the corresponding factors. Example:

$$x^3 - 6x^2 + 11x - 6 = 0.$$

Trying the divisors $x-1$, $x-2$, $x-3$, we find

$$\begin{array}{rrrrl}
1 & -6 & 11 & -6 & \underline{|1} \\
 & \underline{1} & \underline{-5} & \underline{6} & \\
 & -5 & 6 & 0 & \\
 & & & & \\
1 & -6 & 11 & -6 & \underline{|2} \\
 & \underline{2} & \underline{-8} & \underline{6} & \\
 & -4 & 3 & 0 & \\
 & & & & \\
1 & -6 & 11 & -6 & \underline{|3} \\
 & \underline{3} & \underline{-9} & \underline{6} & \\
 & -3 & 2 & 0 &
\end{array}$$

so that 1, 2, 3, are the roots. Again, we have seen that the equation

$$x^4 - 11x^2 + 14x - 24 = 0$$

has two roots, 3 and -4: required the depressed equation.

$$\begin{array}{rrrrrl}
1 & 0 & -11 & 14 & -24 & \underline{|3.-4} \\
 & \underline{3} & \underline{9} & \underline{-6} & \underline{24} & \\
 & 3 & -2 & 8 & 0 & \\
 & \underline{-4} & \underline{4} & \underline{-8} & & \\
 & -1 & 2 & 0 & &
\end{array}$$

so that the depressed equation is

$$x^2 - x + 2 = 0,$$

which, when resolved, gives the remaining roots of the proposed equation. After these details, we may suppose, henceforth, that our given equation has been freed from commensurable roots, and also from equal roots by the method already explained (section 8). Upon this supposition there are two principal inquiries now before us: first, the *separation* of the roots into real and imaginary, positive and negative, with the deter-

mination of integer limits between which the real roots are severally situated: second, the discovery of the values of the real roots to any required degree of approximation.

17. Two numbers, the one greater and the other less than the root of an equation, are called Limits of that root, the first a superior limit, the second an inferior. Evidently, the less the difference between the limits, the more useful they are for defining the position of the root. The only question we shall consider here is, how to determine a superior and an inferior limit of all the real roots of an equation. Of the various rules given for this purpose, the following are among the simplest and most valuable:—*The numerically greatest negative coefficient of the equation, taken positively and increased by unity, is a superior limit of all the real roots.* Again: *If the first negative term that occurs in an equation* (1) *be* $(-p x^{n-m})$, *and the greatest negative coefficient be* $(-a)$; *the quantity* $(1+\sqrt[m]{a})$ *is a superior limit of all the roots.* Again: *If a number* (b), *substituted for* (x), *make* f (x) *and its successive differential coefficients all positive;* (b) *is a superior limit of the positive roots.* Again: *If we add to unity a series of fractions whose numerators are the successive negative coefficients, and denominators the sums of the positive coefficients, including that of the first term; the greatest of the resulting values is a superior limit of all the roots.* If we had space to enter into the proof of these rules, we should only have to show, that for the value of x assigned in each case as the limit, and for all higher values, the first member $f(x)$ has values different from zero. Thus, to prove the first rule, which is due to Maclaurin, let $(-a)$ be the greatest negative coefficient: then, the quantity

$$x^m - a\,(x^{n-1} + x^{n-2} + \ldots + 1)\left\{= x^n - a\,\frac{x^n-1}{x-1}\right\}$$

cannot be greater than $f(x)$ for any positive value of x; for the positive and negative parts of the expression are less and greater respectively than those of $f(x)$. Therefore, we find a limit by satisfying the inequality

$$x^n - a\,\frac{x^n-1}{x-1} > 0,$$

$$\text{or } \frac{x^n(x-1) - a(x^n-1)}{x-1} > 0,$$

$$\text{or } \frac{x^n(x-1-a)+a}{x-1} > 0;$$

and this is satisfied if $x - 1 = a$, or $x - 1 > a$; that is, if $x > 1 + a$. The second rule is proved in the same manner: only we satisfy the inequality

$$x^n - a\,(x^{n-m} + x^{n-m-1} + \ldots + x + 1) > 0.$$

The third rule is due to Newton. It often gives a closer limit than any other, and is easily applied if we begin at the highest differential coefficient, and proceed regularly towards $f(x)$, enlarging the limits as we find it necessary. To prove it: diminish each root of the given equation by a quantity (e), or substitute $y + e$ for x: the resulting equation is, by Taylor's theorem,

$$f(e) + y f_1(e) + \&c. = 0;$$

and evidently it has no *positive* root (y) if e be a limit according to the rule. The fourth rule is due to Bret: it is easily applied, and sometimes gives very close limits. Example:

$$x^5 + x^4 - 4x^3 - 6x^2 - 7000x + 800 = 0.$$

The superior limit found by the first rule is 7001, by the second $1 + \sqrt{7000}$ or 84, by Newton's 7, by Bret's 10: the greater labour in the third case is more than compensated by the smallness of the limit. To find now an *inferior limit* of all the real roots: change x into $-y$, and find a superior limit of the roots (y) of the transformed equation. To find *an inferior limit of the positive roots:* change x into $\frac{1}{y}$, and find a superior limit (a) to the roots (y) of the transformed equation; then $\frac{1}{a}$ is the limit required, for

$$a > \frac{1}{x} \therefore 1 < a x \therefore \frac{1}{a} < x.$$

It is obvious that in some cases there can be no question about the limits of real roots, as when the powers of x are all even, and the coefficients all positive; in other cases, no question about the limits of positive roots, as when all the coefficients have the same sign; in other cases, none about the limits of negative roots, as when all the terms of even dimensions have one sign, and those of odd dimensions the contrary.

18. Upon the separation of the roots, we may begin with this theorem: *If* f (r) *and* f (s) *have contrary signs* (r *and* s *being any real numbers*), *the equation* f (x) = 0 *has one root or some odd number of roots between* (r) *and* (s); *and if* f (r) *and* f (s) *have the same sign, the equation has no root or some even number of roots between* (r) *and* (s). It has been proved already upon simple grounds (sect. 1), that if $f(r)$ and $f(s)$ have contrary signs, the equation has at least one real root between r and s. To prove this more general theorem, let $a, b, \ldots k$, be the real roots of $f(x) = 0$: then,

$$f(x) = (x-a)(x-b)\ldots(x-k)\cdot\varphi(x);$$

$\varphi(x)$ being the product of the real quadratic factors corresponding to the imaginary roots, if there are such. We observe first that $\varphi(x)$ cannot undergo a change of sign for any successive real values of (x); for if it did, the equation $\varphi(x) = 0$ would have at least one real root (by the elementary theorem just mentioned), and $\varphi(x)$

would contain a real factor $(x - l)$ of the first degree. Hence, the signs of $f(x)$ for different values of x depend only upon the signs of its factors,

$$x - a,\ x - b, \ldots . (x - k).$$

And now, if $f(r)$ and $f(s)$ have contrary signs, it is evident that one of the factors $(x - a)$, or some odd number of them, have changed their signs in the passage from one substitution $(x = r)$ to the other $(x = s)$; and therefore one of the roots, a, or some odd number of them, lie between (r) and (s). In like manner, if $f(r)$ and $f(s)$ have the same sign, either none of the factors $(x - a)$ or an even number of them, have changed their signs in the passage from $(x = r)$ to $(x = s)$. The bearing of this theorem upon our question is obvious: but to obtain a perfect method from it, some other considerations are necessary, which will be supplied immediately. This question of separation of the roots is evidently most important; for there can be no resolution of a numerical equation till we know the number and positions of the real roots. Accordingly, the subject has been examined by able algebraists with the greatest diligence; and many interesting results have been obtained, such as the theorem of Descartes, those of De Gua, and those of Fourier contained in his posthumous work upon Equations. Referring, for a sufficient view of some of these, to Dr. Peacock's paper already mentioned, we shall present here, first, Lagrange's method, and then that of Sturm, which may be considered as superseding all others.

19. The first rigorous method of separating the roots was suggested by Waring, discovered independently by Lagrange, and expounded by the latter in his great work on the Resolution of Numerical Equations. Given a numerical equation

$$f(x) = 0; \qquad (1.)$$

find a second equation

$$\varphi(y) = 0, \qquad (2.)$$

whose roots (y) shall be the squares of the differences of the roots (x) of eq. (1); a problem that we have noticed at sufficient length (sect. 13). By any of the methods that have been indicated (sect. 17), find an inferior limit to the positive roots of eq. (2): let it be m: then $\sqrt{m}$ is less than the difference of any two roots of (eq. (1). Returning to the given equation, find a superior and an inferior limit (p and q) of all its roots. Substitute now successively for x in $f(x)$, a set of numbers

$$k\,a,\ (k - 1)\,a,\ (k - 2)\,a, \ldots . \ldots - (h - 1)\,a,\ -h\,a;$$

a being equal to $\sqrt{m}$, or to any less number more convenient; $k\,a$ being not less than p, and $-h\,a$ not greater than q. All the real roots of eq. (1) lie between $k\,a$ and $-h\,a$; and in the interval between any consecutive pair of values substituted for x, there must be either *one root* or *no root* of eq. (1). By the theorem just proved (last sect.), the signs of $f(x)$ for the two values of x are different in the case of one root, similar in the case of no root; so that the only thing to be observed is, the succession of signs of $f(x)$ for the values $k\ a$, $(k - 1)\ a$, &c., of x. This is a theoretically perfect method; it detects all the real roots inevitably; and it fixes their positions by means of a superior and an inferior limit for each. The work, however, is enormous, and for equations of high degrees, impracticable. And although Cauchy has simplified the method to this extent, that we require to find only the last term of eq. (2), yet even thus the calculations are excessively tedious, and the work is transferred in a measure from one part of the process to another, as the value of the interval (a) is generally by Cauchy's method much too small. We come now to Sturm's theorem.

20. Let $f(x) = 0$ be a given equation, which has no equal roots, or has been deprived of them. Find the differential coefficient $f_1(x)$ of $f(x)$. Apply to the polynomials $f(x)$ and $f_1(x)$ the ordinary process of greatest common measure, in all its details, except that each of the remainders, before being taken as a divisor, has the signs of all its terms changed. Denote the successive remainders, with their signs changed, by $f_2(x)$, $f_3(x), \ldots f_{r-1}(x), f_r$. The last remainder $-f_r$ is, by the nature of the process, independent of x; it is also different from 0, because the given equation has no equal roots. Sturm's theorem is as follows:—*In the series of functions*

$$f(x), f_1(x), f_2(x), \ldots . f_{r-1}(x), f_r,$$

substitute any number (a) *for* (x), *and write in order and in one line the signs assumed by the successive functions: in like manner, substitute a second number* (b) *for* (x), *and find a second series of signs: then, the difference between the number of variations* (+ — *or* — +) *in the first series of signs, and the number of variations in the second series, is precisely the number of the real roots of the given equation which lie between* (a) *and* (b). For simplicity of reference, the proof will be thrown into paragraphs.

(1.) It may be assumed here (see sect. 1) that each of the functions under consideration is a continuous function of x. And therefore *none of the functions* (f) *can undergo a change of sign* (in virtue of a continuous change in the value of x), *without passing through zero.*

(2.) Denoting the successive quotients by q_1, q_2, &c., we have, by hypothesis,

$$\begin{aligned} f(x) &= q_1 f_1(x) - f_2(x), \\ f_1(x) &= q_2 f_2(x) - f_3(x), \\ &\ldots\ldots\ldots \\ f_{r-2}(x) &= q_{r-1} f_{r-1}(x) - f_r. \end{aligned}$$

More generally, any three consecutive functions f_{n-1}, f_n, f_{n+1}, are connected by an equation, of the form

$$s \cdot f_{n-1}(x) = q_n \cdot t \; f_n(x) - f_{n+1}(x); \quad (2.)$$

where s and t are *positive* numerical factors, introduced for convenience, as in the process of G. C. M., to prevent the occurrence of fractions.

(3.) *No two consecutive functions* f_{n-1} *and* f_n *can vanish for the same value* (a) of (x). For if

$$f_{n-1}(a) = 0, \text{ and } f_n(a) = 0,$$

the equation (2) gives $f_{n+1}(a) = 0$; and since the three functions f_n, f_{n+1}, f_{n+2}, are connected by an equation of the same form as (2), we find in the same manner $f_{n+2}(a) = 0$, and so on, to the last result $f_r = 0$, which is impossible.

(4.) *If any intermediate function* (any one of the series except the first or the last) *vanishes for the value* (a) *of* (x), *the preceding function and the following have contrary signs for the value* (a); *and these signs are constant for all values of* (x) *included between an inferior limit which is less than* (a) *and a superior limit which is greater than* (a). For if $f_n(a) = 0$, equation (2) gives

$$s \cdot f_{n-1}(a) = -f_{n+1}(a);$$

so that the two functions f_{n-1} and f_{n+1} have contrary signs for the value (a), unless they both vanish, which they do not (par. 3): and further (par. 1), the two functions preserve their signs unchanged for all values of x included between those two of the roots of the equations

$$f_{n-1}(x) = 0 \text{ and } f_{n+1}(x) = 0,$$

which are next less than (a) and next greater than (a) respectively, roots necessarily different in value from (a).

(5). *If* (c) *be a root of the equation* f (x) = 0; *the signs of the first two functions* f (x) *and* f_1 (x) *change from contrary to similar, when* (x) *increases from below* (c) *to above* (c). It has been already proved (sect. 1), that

$$f(x+h) = f(x) + f_1(x) \cdot h + \mathrm{Q}\,h^2 + \mathrm{R}\,h^3 + \&c.;$$

and

$$f(x-h) = f(x) - f_1(x) \cdot h + \mathrm{Q}\,h^2 - \mathrm{R}\,h^3 + \&c.$$

whence, observing that $f(c) = 0$, we find

$$f(c+h) = h\left\{f_1(c) + (\mathrm{Q})\,h + (\mathrm{R})\,h^2 + \&c.\right\},$$

$$f(c-h) = h\left\{-f_1(c) + (\mathrm{Q})\,h - (\mathrm{R})\,h^2 + \&c.\right\};$$

where the coefficients (Q), (R), &c., cannot be infinite. Now let h be an indefinitely small quantity. It is evident (as in the end of sect. 1) that the signs of the second members of the last two equations are the same as those of their first terms; and therefore, $f(c+h)$ has the same sign as $f_1(c)$, and $f(c-h)$ the contrary sign. And since (as in last par.), $f_1(c-h)$, $f_1(c)$, $f_1(c+h)$ have all the same sign; we infer that $f(c-h)$ and $f_1(c-h)$ have contrary signs, while $f(c+h)$ and $f_1(c+h)$ have the same sign.

(6.) Consider now the changes that take place in the series of signs of the functions (f), while x increases continuously through any range of magnitude. There is no change at any point of the series till one of the functions passes through zero (par. 1): we shall suppose therefore, first, that x has reached a value a for which one of the intermediate functions, f_n, vanishes. It has been proved (par. 4), that when x passes through a, the signs of f_{n-1} and f_{n+1} are unchanged, and contrary to each other; and therefore, the signs of f_{n-1}, f_n, f_{n+1}, present one variation and one permanence for values of x immediately above a, as for values immediately below a; the change of sign of f_n having only the effect of making permanence and variation change places in three consecutive signs of the series. Hence, by the passage of x through values which cause any one or more of the intermediate functions to vanish, there is no change produced in the total number of variations. Suppose next, that x has attained a value b for which $f(x)$ vanishes. It has been proved (par. 5), that in this case, the signs of $f(x)$ and $f_1(x)$ form a variation for values of x immediately below b, and a permanence for values immediately above b; therefore, the series of signs loses a variation when x passes through the value b. As x increases from the root b of the given equation to the root c next greater, the variations in the series of signs are not altered in number; but their arrangement is necessarily altered in such a manner, that before x reaches c, the first two signs of the series are changed from permanence to variation; and this variation is lost when x passes c. It is therefore evident, that the number of variations in the series of signs of the functions (f) is diminished by unity every time that x increases through a root of the equation, and is neither diminished nor increased in any other case: which was to be proved. This admirable theorem was presented by Sturm to the Institute of France in 1829, and published in the *Memoires* for 1835: it took its place at once in the elements as an invaluable contribution to the Theory of Equations. For various important remarks upon the theorem, and for examples, the reader must be referred to regular works, such as Young's *Treatise on Equations*. For an instructive application of Sturm's *method* to a particular class of equations, see Serret's *Higher Algebra*, the 13th lesson.

21. To find the values of the incommensurable roots, to any assigned degreee of approximation. This is the only remaining inquiry that is of much practical interest in the theory of numerical equations, after we have seen how to detect all the real roots, and to assign special limits, a superior and an inferior, for each of them. The most natural mode of solution, and one that need not be dwelt upon, is to substitute for x in $f(x)$

a set of numbers intermediate between the two limits of the required root, till two numbers are obtained, which give results of contrary signs, and whose difference is less than the fraction that expresses the degree of approximation. Either of the last two numbers may be taken as the required approximate value of the root. To reach a result of this kind with certainty, in a regular and simple manner, and with as little work as possible; such are the objects of the higher methods of approximation. Of these methods there are three which have obtained a classical standing in the elements. The oldest is due to Newton. Let $f(x) = 0$ be the given equation: let a be an approximate value of the required root, found as above; let $a + y$ be the true value of the root. When x is changed into $a + y$, the equation becomes

$$f(a) + f_1(a) \cdot y + Q \cdot y^2 + \ldots + y^n = 0, \text{ or}$$

$$y = -\frac{f(a)}{f_1(a)} - \frac{Q}{f_1(a)} \cdot y^2 - \frac{R}{f_1(a)} \cdot y^3 - \&c. \qquad (3.)$$

Now if y be a small fraction, the powers y^2, y^3, &c., are much smaller, and the eq. (3) gives, nearly

$$y = -\frac{f(a)}{f_1(a)}. \qquad (4.)$$

Thus; let the approximate value a be true to one decimal figure, or let y be less than ·1; then $y^2 \angle$ ·01, $y^3 \angle$ ·001, &c.; and therefore we may suppose generally, that the part of eq. (3) omitted in eq. (4) is $\angle$ ·01, or, that the value of y given by eq. (4) is true to two places of decimals. Find therefore the value of the quotient $-f(a) : f_1(a)$ to two decimal figures, and add to a. The sum, b, is the value of the root, true to two places. For a second approximation; representing the true value of the root by $b + z$, and proceeding as before, we find

$$z = -\frac{f(b)}{f_1(b)}.$$

Calculate the value of the quotient $-f(b) : f_1(b)$ to four places of decimals, and add to b. The sum c, is the value of the root, true to four places. Without more details, it will be seen that the one formula of correction is

$$y = -\frac{f(x)}{f_1(x)};$$

where x is the preceding approximate value, and y the new correction, the number of decimal figures in y being twice as many as in x. The successive approximations given by this method are far from being universally correct. The subject has been discussed at large by Lagrange, Fourier, and other more recent writers: the cases of probable failure have been clearly fixed, and precautions of a simple kind have been indicated which are sufficient to insure a correct result in every case. And these are matters that must continue always to be of interest, on account of the great simplicity of principle, and easiness of application, that are characteristic of Newton's method. Here, however, it must suffice to observe, that each approximate value may be submitted to a simple test, which is perfectly adequate in every case, although the work be tedious. To verify the value b, (true, as we have supposed, to two decimal figures), find the values of $f(b)$ and $f(b + \cdot 01)$. If these have contrary signs, the approximation (b) is correct; if similar signs, it is incorrect: and in the latter case we must begin anew, with a more approximate value instead of a, &c. Example: required the roots of

$$x^3 - 5x - 3 = 0.$$

We find easily $+ 3$ and $- 2$ as a superior and an inferior limit of the roots. Substituting the numbers

$$-2,\ -1,\ 0,\ 1,\ 2,\ 3,$$

for x, we find the values of $f(x)$ to be

$$-1,\ +1,\ -3,\ -7,\ -5,\ +9.$$

Hence, (sect. 18), the three roots are real, one between 2 and 3, one between 0 and -1, one between -1 and -2. To determine, first, the positive root,

$$f(2{\cdot}5) = +{\cdot}125;\ f(2{\cdot}4) = -1{\cdot}176;$$

therefore, the value 2·4 of x is true to the last figure. Applying now the formula of correction

$$y = -\frac{f(x)}{f_1(x)} = -\frac{x^3 - 5x - 3}{3x^2 - 5}, \text{ we find}$$

$$x = 2{\cdot}4 - \frac{f(2{\cdot}4)}{f_1(2{\cdot}4)} = 2{\cdot}4 + \frac{1{\cdot}176}{12{\cdot}28} = 2{\cdot}49;$$

and in verification, we find

$$f(2{\cdot}49) = -{\cdot}011751;\ f(2{\cdot}5) = +{\cdot}125.$$

As a second approximate value, we find

$$x = 2{\cdot}49 - \frac{f(2{\cdot}49)}{f_1(2{\cdot}49)} = 2{\cdot}49 + {\cdot}0008$$

$$\therefore x = 2{\cdot}4908;$$

which is true to four places, as may be verified by calculating $f(2{\cdot}4908)$ and $f(2{\cdot}4909)$. By changing x into $-x$, and finding the two positive roots of the transformed equation, we obtain the negative roots of the given equation: they are $-{\cdot}6566$ and $-1{\cdot}8342$.

22. We come now to Lagrange's method of approximation; the conception of which is remarkably simple and elegant. It consists in the development of each real root under the form of a continued fraction. Compared with Newton's, this method is very laborious; but, on the other

hand, it gives approximate values that may be relied upon in all cases without discussion. In what follows, it will be assumed for convenience, that all the real roots differ from each other by more than unity. If this were not the case, we could change all the roots in any proportion, and proceed from the transformed equation. We shall suppose also, that the integer parts of all the roots have been found. Let the given equation be

$$f(x) = 0; \qquad (1.)$$

and let (a) be the integer part of the required root, which may be supposed positive. Assume

$$x = a + \frac{1}{y}.$$

This expression, substituted for (x) in eq. (1), gives an equation

$$F(y) = 0, \qquad (2.)$$

which is of the same degree as eq. (1), and has the same number of roots. Observing now that the roots (y) of eq. (2), when substituted in the expression $a + \frac{1}{y}$, must give the roots (x) of eq. (1); observing also that the required value of x lies between a and $a + 1$; we infer, that the required value of y is positive, and greater than 1. It is evident also that the eq. (2) can admit of only one such root; for otherwise, the eq. (1) would have more than one root between a and $a + 1$, which is contrary to the hypothesis. Hence, if we substitute for y in eq. (2) the successive integers 1, 2, 3, 4, &c., we must arrive sooner or later at two consecutive integers, b and $b + 1$, such, that $F(b)$ and $F(b + 1)$ have contrary signs. The number b so determined is the integer part of the required value of y: and we have, as a first approximation,

$$x = a + \frac{1}{b}.$$

But we may now proceed from eq. (2) in precisely the same way as we did from eq. (1). Assuming

$$y = b + \frac{1}{z},$$

and substituting for y in (2), we find an eq.

$$\varphi(z) = 0, \qquad (3.)$$

which has only one root greater than 1; which root is found, by successive substitutions, to lie between the consecutive integers c and $c + 1$. Assuming again

$$z = c + \frac{1}{u},$$

and substitutingi n (3), we find an equation

$$\psi(u) = 0, \qquad (4.)$$

the only root of which, proper to the question, is found as before, to be intermediate between the two integers d and $d + 1$; and so on. Having assumed successively

$$x = a + \frac{1}{y},\ y = b + \frac{1}{z},\ z = c + \frac{1}{u}, \text{\&c.}$$

we have finally

$$x = a + \cfrac{1}{b + \cfrac{1}{c + \text{\&c.}}}$$

The successive convergents $\frac{a}{1}, \frac{ab + 1}{b}$, are alternately superior and inferior limits of the required root, each of them being nearer the true value of the root than the preceding: and the degree of approximation is certainly closer than that expressed by the fraction $\frac{1}{n^2}$, n being the denominator of the last convergent. These are inferences from the simplest properties of continued fractions. It only remains to observe, that for the formation of the successive equations (2), (3), &c., which is the most tedious part of the process, there is a simple and uniform course of work assigned by Taylor's theorem. Supposing the equation to be of the nth degree, and denoting the successive differential coefficients of $f(x)$ by $f_1(x), f_2(x)$, &c., we find

$$f\left(a + \frac{1}{y}\right) = f(a) + f_1(a) \cdot \frac{1}{y} + \frac{1}{2} f_2(a) \cdot \frac{1}{y^2} + \ldots + \frac{k}{y^n},$$

where k is the coefficient of x^n in eq. (1). Multiplying by y^n, and equating to 0, we find eq. (2)

$$f(a) \cdot y^n + f_1(a) \cdot y^{n-1} + \frac{1}{2} f_2(a) \cdot y^{n-2} + \ldots + k = 0;$$

and similarly for the others. Example: required the positive root of the equation

$$x^3 - 5x - 3 = 0;$$

which we know to be between 2 and 3. Here

$$x = 2 + \frac{1}{y},$$

and equation (2) is found to be

$$F(y) = 5y^3 - 7y^2 - 6y - 1 = 0;$$

whence, $F(1) = -9$, $F(2) = -1$, $F(3) = +53$; and, therefore

$$y = 2 + \frac{1}{z},$$

and eq. (3) is found to be

$$\varphi(z) = z^3 - 26z^2 - 23z - 5 = 0;$$

whence $\varphi(26) = -(603, \varphi(2= 7) + 103$; and

$$z = 26 + \frac{1}{u}.$$

We find thus, for x, the continued fraction

$$2 + \cfrac{1}{2 + \cfrac{1}{26 + \cfrac{1}{1 + \cfrac{1}{6 + \&c.}}}}$$

The convergents are

$$\frac{2}{1}, \ \frac{5}{2}, \ \frac{132}{53}, \ \frac{137}{55}, \ \frac{954}{383}.$$

The last differs from the true value of the root by a fraction less than $\frac{1}{(383)^2}$ or $\frac{1}{146689}$.

23. We have now to notice Horner's method of approximation; and this must be done very briefly. We shall suppose that the integer part of each incommensurable root has been found in the first place by Sturm's theorem, or otherwise. We may assume also, for simplicity, that all the roots of the equation differ from each other by more than unity, and that the root required is positive, and lies between 0 and 10, so that its integer part consists of not more than one figure. Let the given equation be

$$f(x) = 0; \qquad (1.)$$

and let h be the integer part of the required root. The first step in Horner's process is, to diminish each root of eq. (1) by the number h, the first figure of the required root. If we suppose $y + h$ substituted for x in eq. (1), and represent the transformed equation by

$$F(y) = 0; \qquad (2.)$$

each root (y) of eq. (2) will be less than a corresponding root (x) of eq. (1) by the number h, as required. The second step is, to multiply each root of eq. (2) by 10. If we suppose $\frac{x}{10}$ substituted for y in eq. (2), and denote the transformed eq. by

$$\varphi(x) = 0; \qquad (3.)$$

each root (x) of eq. (3) will be ten times a corresponding root of eq. (2), as required. Now, from the nature of the process, it is evident that the root of (2) which corresponds to the required root of (1) lies between 0 and 1. It is evident also, from the hypothesis, that eq. (2) has only one root between these limits; for otherwise, some of the roots of (1) would differ from each other by less than unity. Hence, of the roots of eq. (3), that corresponding to the required root of (1) lies between 0 and 10; and eq. (3) has only one such root. The third step is, to find the integer part, k, of the only root of eq. (3) that lies between 0 and 10. This number k is evidently the *second figure of the required root.* Having found k; to carry on the approximation, proceed from eq. (3) precisely as before from eq. (1): diminish each root of (3) by k; multiply each root of the transformed equation by 10; find the integer part, l, of the only root of the last equation that lies between 0 and 10. The number l is evidently the *third figure of the required root,* and so on. The method is very easily extended, beyond the special hypothesis that we have been proceeding upon, so as to apply to any equation, however small be the differences of the roots, and whatever be the integer part of the root required. But we have space here only for first notions. With regard to the form of the calculations, this will be best indicated by an example. Required the positive root of the equation

$$x^3 - 5x - 3 = 0;$$

which lies, as we already know, between 2 and 3. The first step, the passage from eq. (1) to eq. (2), is effected thus:

$$\begin{array}{ccccc}
1 & 0 & -5 & -3 & \underline{|2} \\
 & \underline{2} & \underline{4} & \underline{-2} & \\
 & 2 & -1 & -5 & \\
 & \underline{2} & \underline{8} & & \\
 & 4 & 7 & & \\
 & \underline{2} & & & \\
 & 6 & & &
\end{array}$$

The reader will easily understand this work, if he has a distinct recollection of the method of Detached Coefficients, as explained in sect. 16, and also of the solution of problem 6 in sect 7. In the first two lines, the first member of the equation is divided by $x - 2$: the quotient is found to be $x^2 + 2x - 1$, and the remainder is -5. In the next two lines, the preceding quotient is divided by $x - 2$: the quotient is found to be $x + 4$, and the remainder 7. In the last two lines, the preceding quotient is divided by $x - 2$: the remainder is found to be 6. Thus, the numbers (-5, 7, 6) at the feet of the columns, are precisely those denoted in sect. 7 by R_1, R_2, R_3: and therefore, by the simple work above, we find the eq. (2) to be

$$y^3 + 6y^2 7y - 5 = 0.$$

The second step, the passage from eq. (2) to eq. (3), is effected at once, according to probl. 3, section 7. Thus, from the last equation we find

$$x^3 + 60x^2 + 700x - 5000 = 0. \qquad (3.)$$

We find next the integer part, k, of the root of (3) that lies between 0 and 10, and, in continuation, we diminish each of the roots of (3) by k; thus:

1	60	700	— 5000	4
	4	256	3824	
	64	956	— 1176	
	4	272		
	68	1228		
	4			
	72			

This piece of work is precisely similar to the former: only, the condition by which 4 is determined to be the value of k, ought to be distinctly noticed. It is this: that 4 is the largest number of the series, 0, 1, 2, 3 . . . 9, which gives a remainder (— 1176) at the foot of the last column, having the same sign as the number (— 5000) at the top of the column; and the reader should be able, at this stage, to see the reason. We have thus found the first two figures of the root to be 2·4; and by proceeding similarly from the set of coefficients

$$1 \qquad 720 \qquad 122800 \qquad -1176000,$$

we should find the third figure to be 9, the fourth 0, the fifth 8, &c. Observe now that we may present the second piece of work as a continuation of the first, by affixing one, two, three, ciphers, respectively, to the numbers at the feet of the columns in the first piece, and then proceeding from these numbers as we have done in the second piece. In this manner it is evident, that however far we have to carry the approximation, we can throw the whole calculations into a continuous piece of arithmetical work, and this work as easily managed as any of the operations in elementary arithmetic. And here lies the indisputable superiority of Horner's method. There is one inconvenience incident, at first sight, to the process: as the approximation is carried on, the numbers involved in the work become very large. But this difficulty is completely removed by the use of certain contracted modes of working, which present no difficulty, and which are similar to those employed in the multiplication and division of decimals. For Horner's own exposition of the method, see the *Phil. Trans.* for 1819. For what is considered a better exposition of the whole subject, see Young's *Treatise on Equations.*

24. The only thing that remains to be noticed, on the resolution of numerical equations, is the determination of imaginary roots: but this branch of the subject is of little practical interest. When the number of real roots of an equation has been determined by Sturm's theorem, or in any other manner, it is known at once whether the equation has imaginary roots, and how many. To find such a root, when it is known to exist, substitute $a + b\sqrt{-1}$ for x in the given equation. The result is an equation

$$\mathrm{H} + \mathrm{K}\sqrt{-1} = 0,$$

where H and K are algebraic expressions involving a and b, and independent of $\sqrt{-1}$. Any set of values of a and b that satisfy this equation, or the equivalent pair of simultaneous equations

$$\mathrm{H} = 0,\ \mathrm{K} = 0,$$

give an imaginary root $(a + b\sqrt{-1})$ of the proposed equation. According to this method, the determination of imaginary roots depends upon a process of elimination.

III. On the Algebraic Resolution of Equations.

The coefficients of an equation being arithmetically given, or being simply supposed known, but left indeterminate, and represented by letters; required an expression composed of the coefficients, which, when substituted for the unknown, shall satisfy the equation identically. Such is the principal question with which we are now concerned. The researches of the modern mathematicians upon this and other cognate questions form a large part of the higher Algebra. They are almost without exception much more difficult and abstract than anything we have yet had before us; they are encumbered also, for the most part, with tedious and intricate preliminary theories: so much so, that nothing more can be given here, upon the higher parts of the subject, than a slight historical sketch.

25. The roots of unity, or the roots of the equation

$$x^m - 1 = 0, \qquad (2.)$$

are of such importance in what follows, that some of their properties must be noticed. First: If in the three equations

$$x^m = 1,\ x^n = 1,\ x^p = 1,$$

m and n are any whole numbers, and p their greatest common measure; the roots common to the first two equations are precisely those of the third. To prove this: Let a be any root common to the first two. Let m and n be submitted to the process of G. C. M.; and let q be the first quotient and r the remainder, so that $m = nq + r$: then

$$a^m = a^{nq+r} = a^{nq} \cdot a^r.$$

But, by hypothesis, $a^m = 1$; and therefore

$$a^{nq} \cdot a^r = 1.$$

Again, by hypothesis, $a^n = 1$; and therefore

$$a^{nq} = 1.$$

Dividing the last equation by this, we find

$$a^r = 1:$$

so that every root, a, common to the first two equations, is a root also of the equation $x^r - 1 = 0$. By a simple extension of this reasoning it appears, that every root common to the first two of the given equations is a root also of the third.

We have only to see further, that every root of the third given equation is common to the first two. Let b be any root of the third, so that $b^p = 1$: then, the numbers m and n being multiples of p, the quantities b^m and b^n are powers of b^p, that is, powers of 1, and therefore equal to 1. Which proves the theorem. Hence, if m and n are prime to each other, the first two equations have no root in common except unity. If we understand by Primitive Roots of eq. (2) those roots of (2) that do not belong to any equation of the same form and less dimensions (a definition that will be useful immediately); we infer from what has been proved, that when m is a prime number, all the roots of eq. (2) are primitive roots except unity.

Further: If a be a root of the eq. (2), every power of a is a root of the same equation. By hypothesis, $a^m = 1$. Raising both members of this equation to the nth power, we find

$$(a^m)^n = a^{mn} = (a^n)^m = 1:$$

whence, a^n, or each term of the indefinite series

$$a,\ a^2,\ a^3,\ a^4,\ a^5 \ldots\ldots$$

is a root of the given equation $x^m = 1$: which was to be proved. Since the equation is of the mth degree, the series of powers of a cannot contain more than m distinct quantities, however prolonged. And this is evident otherwise, from the condition by which a is determined: for if we multiply the equation $a^m = 1$ by a, by a^2, by a^3, &c., we find $a^{m+1} = a$, $a^{m+2} = a^2$, &c.

Further: If m be a prime number, and a be any root of eq. (2) except unity; the m roots of the equation will be represented precisely by

$$a,\ a^2,\ a^3,\ \ldots\ldots\ a^{m-1},\ a^m.$$

We have seen that each of these magnitudes is a root: we have only to see further that they are all different. Suppose that any two of them, a^p and a^q are equal. Divide the equation $a_p = a^q$ by a^q; then, $a^{p-q} = 1$: whence, it would follow that the given equation has a root in common with the equation $x^{p-q} = 1$, which is of less dimensions. This we have seen to be impossible, m being a prime number. Therefore, all the m terms of the above series are different from each other, and each of them is a root: which proves the theorem. This does not hold when m is a compound number, and a any root of the equation. It will be seen, however, by the nature of the proof, that the theorem does hold when m is a compound number, provided a be a *primitive root* of the given equation. The importance of primitive roots will now be apparent: for, whatever be the value of m, if we can find one such root of eq. (2), its m successive powers will furnish all the roots of (2). If we had space for the prosecution of this subject, we would obtain several interesting theorems, almost as simple as the above, leading to this important result, that the resolution of eq. (2), when m is a compound number, is reducible to the resolution of a set of equations of the same form as (2), whose dimensions are equal to the prime factors of m. Thus; if $m = p^n$, where p is a prime number; the eq. (2) is found to have a set of primitive roots, whose number is $m\ (1 - \frac{1}{p})$: and each of the roots of (2) is found to be expressible as a product

$$a_1 \cdot a_2 \cdot a_3 \ldots a_{n-1} \cdot a_n,$$

where a_1 is any root of the equation $x^p = 1$, a_2 any root of $x^p = a_1$, a_3 any root of $x^p = a_2$, &c.: and this product is a primitive root of (2), if a_1 is different from unity. Simplifications of much the same kind are obtained, when m involves any number of unequal prime factors. With regard to the above results, it should be observed for the sake of the beginner, that the subsidiary equations $x^p = a_1$, $x^p = a_2$, &c., present no difficulty, if we know how to resolve the equation $x^p = 1$. Consider an eq. $x^m = a$: let c be the arithmetical mth root of a: introduce a new unknown, y, such that $x = c\,y$. The eq. $x^m = a$ becomes $c^m\,y^m = c^m$, or, $y^m = 1$: and the roots of the latter, multiplied by c, give the roots of the former. After these results, we may suppose m in eq. (2) to be a prime number. The only remaining question is, to resolve (2) on that supposition. The beautiful and very effective researches of Gauss upon this question will fall under our notice more properly afterwards. Meanwhile, the following elementary method is applicable in all cases. Let $m = 2\,n + 1$. By the theory of Reciprocal Equations, explained in (sect. 9), we can reduce the resolution of eq. (2) to that of an eq. (3) of the nth degree. It is easily proved that all the roots of (3) are real. Their values may be obtained, therefore, to any degree of approximation, by Horner's process, or otherwise, if they are not expressible by radicals; and then the roots of (2) are found at once, as they are connected with those of (3) by a quadratic equation. The resolution of (2) effected in this manner is not strictly *algebraic*, unless (3) is resolvable algebraically. For an excellent exposition of this whole subject, see Serret's *Higher Algebra*, the 13th Lesson, already referred to. The solution of eq. (2) by De Moivre's theorem, which is given in most books upon trigonometry, is upon the whole the best that has been obtained: but it is not in any right sense an algebraic solution.

26. We proceed to the algebraic resolution of equations of the third degree. Of the various methods that have been proposed, the following is certainly the simplest: it is due to Hudde. The equation

$$x^3 + a\,x + c = 0 \qquad (3.)$$

is general enough in form, as the second term of the equation

$$x^3 + p\,x^2 + q\,x + r = 0$$

may be made to disappear by a simple preparatory process, whatever be the coefficients (sect. 7, probl. 5). To resolve eq. (3) we assume

$$x = y + z; \qquad (4.)$$

where y is a new unknown, and z a function of y to be determined afterwards. Substituting from (4) in (3), we find

$$(y + z)^3 + a\,(y + z) + c = 0, \text{ or}$$

$$(y^3 + z^3 + c) + (y + z)(3\,y\,z + a) = 0. \quad (5.)$$

If now we determine z by the condition

$$3\,y\,z + a = 0, \text{ we find } z = -\frac{a}{3\,y};$$

and the eq. (5) is reduced to

$$y^3 + z^3 + c = y^3 - \frac{a^3}{27\,y^3} + c = 0; \text{ or}$$

$$y^6 + c\,y^3 - \frac{a^3}{27} = 0. \qquad (6.)$$

This equation, resolved as a quadratic, gives

$$y^3 = -\frac{c}{2} \pm \sqrt{\frac{c^2}{4} + \frac{a^3}{27}};$$

so that, if we represent $\frac{c^2}{4} + \frac{a^3}{27}$ by R, we find

$$y = \sqrt[3]{-\frac{c}{2} \pm \sqrt{\mathrm{R}}}. \qquad (7.)$$

Having obtained y in terms of the coefficients of (3), we find from eq. (4),

$$x = y + z = y - \frac{a}{3\,y}; \qquad (8.)$$

so that the question is solved. The values of y given by eq. (7) are six in number as they ought to be. Thus, if p and q be cube roots of

$$-\frac{c}{2} + \sqrt{\mathrm{R}} \text{ and } -\frac{c}{2} - \sqrt{\mathrm{R}},$$

and if a and b be the imaginary cube roots of 1, the values of y are

$$p,\ a\,p,\ b\,p,\ q,\ a\,q,\ b\,q.$$

It might be thought, therefore, that the values of x determined by eq. (8) would be six in number, which they ought not. Observe, however, that the eq. (6) is not changed by the substitution of $-\frac{a}{3\,y}$ for y; whence it follows that the six roots of eq. (6) are related to each other in pairs thus:

$$r \text{ and } -\frac{a}{3\,r},\ s \text{ and } -\frac{a}{3\,s},\ t \text{ and } -\frac{a}{3\,t};$$

and it is seen at once, that x, as determined by eq. (8), has one value for the first pair of roots, one for the second, and one for the third pair, three values in all. It follows also, that in the formula

$$x = y + z,$$

considered as a solution of the question, we have to substitute for z as well as for y one of the six values of the second member of (7); and these values are to be chosen under the one condition that the product of y and z shall be equal to $-\frac{a}{3}$. Observing then that

$$p\,q = \sqrt[3]{-\frac{c}{2} + \sqrt{\mathrm{R}}} \cdot \sqrt[3]{-\frac{c}{2} - \sqrt{\mathrm{R}}}$$

$$= \sqrt[3]{-\frac{a^3}{27}} = -\frac{a}{3};$$

observing also, that of a and b the imaginary cube roots of unity, one is the square of the other (by last sect.), and therefore their product $a\,b$ is equal to 1; we find the following to be the three values of x, the three roots of eq. (3):

$$p + q,\ a\,p + b\,q,\ b\,p + a\,q:$$

and these may be represented by one formula, usually called Cardan's:

$$x = \sqrt[3]{-\frac{c}{2} + \sqrt{\mathrm{R}}} + \sqrt[3]{-\frac{c}{2} - \sqrt{\mathrm{R}}}.$$

The resolution of cubic equations appears to be due to Scipio Ferrei and Tartaglia, two Italians of the sixteenth century; but how it was effected by them is not known.

27. Lagrange's method of resolving the general equation of the third degree must be noticed. Let the eq. be

$$x^3 + p\,x^2 + q\,x + r = 0; \qquad (1.)$$

and let its roots be x_1, x_2, x_3. Lagrange begins by determining the value of a function t of the three roots, such, that

$$t = x_1 + \mathrm{A}\,x_2 + \mathrm{B}\,x_3; \qquad (2.)$$

and from this function he obtains the expression of the roots themselves. It is evident that if the roots (x) change places in all possible ways, the function t will assume six different forms or values. Hence, the equation for determining t will be of the sixth degree. However, it will be resolvable as a quadratic if it can be obtained so as to involve no other powers of t than t^6 and t^3; and this is done as follows. Let A and B be the two imaginary cube roots of unity, and denote them by a, a^2: then

$$t = x_1 + a\,x_2 + a^2\,x_3. \qquad (3.)$$

Multiplying this eq. by a, and by a^2, we find

$$a\,t = a\,x_1 + a^2\,x_2 + x_3, \text{ and}$$
$$a^2\,t = a^2\,x_1 + x_2 + a\,x_3.$$

Comparing the second members of these three equations, we see that the last two are derivable from the first by simply making the roots (x) change places. It follows, that if we assume

$$\left.\begin{array}{l} t_1 = x_1 + a\, x_2 + a^2\, x_3, \\ t_2 = x_1 + a^2\, x_2 + a\, x^3; \end{array}\right\} \quad (4.)$$

the six values of t will be

$$t_1,\ a\, t_1,\ a^2\, t_1,\ t_2,\ a\, t_2,\ a^2\, t_2:$$

the first three of which, and the last three, are the roots respectively of the two equations

$$t^3 - t_1^3 = 0, \text{ and } t^3 - t_2^3 = 0.$$

So that the equation for determining t is of the form

$$(t^3 - t_1^3)\,(t^3 - t_2^3) = 0, \text{ or}$$
$$t^6 - (t_1^3 + t_2^3)\, t^3 + t_1^3\, t_2^3 = 0, \quad (5.)$$

as required. To find the coefficients of (5) in terms of those of the given equation, we make use of the equations (4), together with the relation

$$1 + a + a^2 = 0, \qquad (6.)$$

which is evident from this, that 1, a, a^2, are the three roots of the equation $x^3 - 1 = 0$. Multiplying the two equations (4), we find

$$\begin{aligned} t_1\, t_2 &= x_1^2 + x_2^2 + x_3^2 - x_1\, x_2 - x_1\, x_3 - x_2\, x_3 \\ &= (x_1 + x_2 + x_3)^2 - 3\,(x_1\, x_2 + x_1\, x_3 + x_2\, x_3) \\ &= p^2 - 3\, q. \qquad (7.) \end{aligned}$$

In a similar manner, but more tediously, we find

$$t_1^3 + t_2^3 = -2\, p^3 + 9\, p\, q - 27\, r.$$

Thus, the *reducing equation* (5) is found to be

$$t^6 + (2\, p^3 - 9\, p\, q + 27\, r)\, t^3 + (p^2 - 3\, q)^3 = 0.$$

Let r_1 and r_2 be the two roots (t^3) of this quadratic: then

$$t_1 = \sqrt[3]{r_1} \text{ and } t_2 = \sqrt[3]{r_2}.$$

To find now the roots (x), we make use of the equations

$$\begin{aligned} -p &= x_1 + x_2 + x_3, \\ t_1 &= x_1 + a\, x_2 + a^2\, x_3, \\ t_2 &= x_1 + a^2\, x_2 + a\, x_3. \end{aligned}$$

Remembering the eq. (6), we find easily

$$\begin{aligned} -p + t_1 + t_2 &= 3\, x_1, \\ -p + a^2\, t_1 + a\, t_2 &= 3\, x_2, \\ -p + a\, t_1 + a^2\, t_2 &= 3\, x_3. \end{aligned}$$

In these expressions, the value of t_1 is any of the three values of $\sqrt[3]{r_1}$; but one of these being assumed, the value of t_2 or of $\sqrt[3]{r_2}$ is connected with it by the relation already found,

$$t_1\, t_2 = \sqrt[3]{r_1} \cdot \sqrt[3]{r_2} = p^2 - 3\, q.$$

28. To resolve the general equation of the fourth degree. Ferrari's method, sometimes called Waring's, is the oldest and simplest. Let the given eq. be

$$x^4 + a\, x^3 + b\, x^2 + c\, x + d = 0. \quad (1.)$$

Putting it in the form

$$x^4 + a\, x^3 = -b\, x^2 - c\, x - d,$$

and adding $\frac{a^2\, x^2}{4}$ to both members, it becomes

$$\left(x^2 + \frac{a\, x}{2}\right)^2 = \left(\frac{a^2}{4} - b\right) x^2 - c\, x - d. \quad (2.)$$

If the second member of (2) were a complete square, the equation might be depressed to the second degree by the extraction of the square roots of both members. Now, to this particular case Ferrari's method reduces all other cases by the introduction of a new unknown, z, as follows: Adding to both members of (2) the quantity

$$\left(x^2 + \frac{a\, x}{2}\right) z + \frac{z^2}{4}, \text{ we find the eq.}$$

$$\left(x^2 + \frac{a\, x}{2} + \frac{z}{2}\right)^2 = \left(\frac{a^2}{4} - b + z\right) x^2 + \left(\frac{a\, z}{2} - c\right) x + \frac{z^2}{4} - d. \quad (3.)$$

The second member of (3) will be a complete square, if z have such a value as to satisfy the eq.

$$\left(\frac{a\, z}{2} - c\right)^2 = 4 \left(\frac{a^2}{4} - b + z\right) \left(\frac{z^2}{4} - d\right). \qquad (4.)$$

This, when simplified, gives the cubic eq.

$$z^3 - b\, z^2 + (a\, c - 4\, d)\, z - d\,(a^2 - 4\, b) - c^2 = 0; \qquad (5.)$$

which, accordingly, is the *reducing equation* in this method. Supposing that any root of (5) has been found; put for brevity

$$\frac{a^2}{4} - b + z = \frac{m^2}{4}, \text{ and } \frac{a\, z}{2} - c = s: \quad (6.)$$

then the condition (4) gives

$\frac{z^2}{4} - d = \frac{s^2}{m^2}$, and the second member of (3) becomes

$$\frac{m^2}{4}\, x^2 + s\, x + \frac{s^2}{m^2}, \text{ or } \frac{m^2}{4}\left(x + \frac{2\, s}{m^2}\right)^2.$$

Therefore, the eq. (3) takes the form

$$\left(x^2 + \frac{a\, x}{2} + \frac{z}{2}\right)^2 - \frac{m^2}{4}\left(x + \frac{2\, s}{m^2}\right)^2 = 0;$$

which decomposes into the two quadratics

$$x^2 + \frac{a\, x}{2} + \frac{z}{2} \pm \frac{m}{2}\left(x + \frac{2\, s}{m^2}\right) = 0; \text{ or}$$

$$\left.\begin{aligned} x^2 + \frac{a+m}{2}\cdot x + \left(\frac{z}{2}+\frac{s}{m}\right) &= 0, \\ x^2 + \frac{a-m}{2}\cdot x + \left(\frac{z}{2}-\frac{s}{m}\right) &= 0. \end{aligned}\right\} \quad (7.)$$

Since the roots of the cubic equation (5) are expressible algebraically in terms of the coefficients of the given eq. (1), it follows that the roots of (1) have the same property; for by means of the last two equations (7), we can express the four roots (x) of eq. (3), which are just the roots of (1), in terms of z and the coefficients of (1). The question, therefore, is solved, but the expressions of the roots are excessively complex. Other solutions, quite distinct from the above, and from each other, have been given by Descartes, Euler, and Lagrange. Looking back to the reducing equation (5), and remembering how Lagrange obtains the reducing equation in the resolution of the equation of the third degree, we may inquire here what function z is of the roots of eq. (1). Let x_1, x_2, and x_3, x_4, be the roots of the two equations (7). Then, these are the roots of eq. (1); and from the equations (7) we find at once

$$x_1 x_2 + x_3 x_4 = \left(\frac{z}{2}+\frac{s}{m}\right) + \left(\frac{z}{2}-\frac{s}{m}\right):$$

whence $$z = x_1 x_2 + x_3 x_4.$$

This *reducing function*, z, or $x_1 x_2 + x_3 x_4$, is of such a form, that if the roots (x) change places in all possible ways, it assumes only three distinct values: and hence we might infer, *à priori*, that the equation for the determination of this function must be of the third degree, as the eq. (5) is in fact. To this circumstance, that we can form a function of four letters which has only three values, is the success of Ferrari's and of every other known method to be attributed; for hence it is that the resolution of biquadratics is reducible to that of cubics. In Lagrange's method, the reducing function is $x_1 x_2 + x_3 x_4$, the same as in Ferrari's. There are other functions, almost equally eligible, such as

$$y = x_1 + x_2 - x_3 - x_4,$$

which is the reducing function in Euler's method. It admits evidently of six different values; but since these are equal and of contrary signs by pairs, the reducing equation of the sixth degree can contain no odd power of y, and may therefore be resolved as a cubic in y^2. The reducing function in Descartes' method is

$$u = x_1 + x_2.$$

It admits of six different values $x_1 + x_2$, $x_1 + x_3$, &c.; so that the reducing equation is of the sixth degree. But if we have previously deprived the equation of its second term, which may always be done, we shall have

$$x_1 + x_2 + x_3 + x_4 = -p = 0, \text{ or}$$
$$x_1 + x_2 = -(x_3 + x_4),$$

so that the values of u are equal and of contrary signs by pairs, and the same remark applies to the reducing equation in this case as in the last. In like manner, the success of Hudde's method and of Lagrange's, in the resolution of cubic equations, depends upon this, that we can form functions of three letters which admit of only two values, by permutations of the letters. Thus, with regard to Lagrange's reducing function t, we have seen that

$$t^3 \text{ or } (x_1 + a x_2 + a^2 x_3)^3$$

admits of only two values; and the same is true of the reducing function y (sect. 26) employed in Hudde's method.

29. The algebraic resolution of an equation is effected, according to every method yet known, by being made to depend upon that of another equation which is more easily resolved, and whose root is a function of those of the original equation. Upon this general view of the subject large developments were given by Lagrange, especially in those famous researches that were published in the *Memoirés of the Academy of Berlin* for 1770 and 1771. He proved that all the known processes for the resolution of quadratic, cubic, and biquadratic equations, however different in appearance, were virtually dependent upon a common principle, and reducible to one general method. He then extended the method to the general equation of the mth degree, by the employment of a reducing function, t, of the form

$$x_1 + a x_2 + a^2 x_3 + \ldots + a^{m-1} x^m,$$

where x_1, x_2, &c., are the roots of the given equation, and a a root of the equation $x^m = 1$. He found that, when m is a prime member, the resolution of the reducing equation, which is generally of the degree 1. 2. 3. . . . m, is reducible to the resolution of another equation whose degree is $m - 1$, and whose coefficients depend upon an equation of degree 1. 2. 3. $(m - 2)$. Results of an equally definite kind were obtained for composite values of m; but it would be of little use to present them here. Generally, when the given equation is of a degree higher than the fourth, the reducing equation found by Lagrange is of a still higher degree, and does not appear to be generally susceptible of depression: a result that almost decides the question of the resolvability of general equations of higher degrees than the fourth, so far at least as the above form of reducing function is concerned, so far, we might even say, as all known principles of resolution are concerned. In Lagrange's work on the Resolution of Numerical Equations, the 14th note, his method is applied in a very elegant manner to the resolution of Binomial Equations, a question that had been solved before by Gauss.

30. Why is it that the general equations of the first four degrees are algebraically resolvable? To this question Lagrange gave a most suggestive answer in his researches last mentioned. As a result of his analyses, and in the distinctest manner, he connected the property of resolvability of an equation with certain properties possessed by functions of sets of letters, in relation to permutations among the letters; a subject upon which a few hints have been just given in the end of sect. 28. The equations of the third and fourth degrees are algebraically resolvable, because we can form functions of three letters which have only two values, and functions of four letters which have only three. And if we could form functions of five letters, having only four or three values, we have reason to believe that such functions would enable us to resolve the general equation of the fifth degree. Here a new line of investigation was presented to mathematicians, and a very difficult one. Lagrange himself discovered one theorem upon the subject, which is, That the entire number of values which a function of n letters can assume, in virtue of permutations among the letters, is either equal to

$$1.\ 2.\ 3.\ \ldots\ n,$$

or some submultiple of this number. Rufini advanced another step: in his Theory of Equations he considered particularly functions of five letters, and he seems to have succeeded in proving, though in an excessively complex manner, That if a function of five letters has less than five distinct values, it cannot have more than two. Rufini's is a case of the following theorem, which has been obtained very recently: n being greater than 4, a function of n letters which has less than n distinct values, has two values at most. The subject has been studied carefully and with success by several eminent mathematicians, Abel, Cauchy, Bertrand, and Serret. The results yet obtained, as well as the principal modes of investigation, are presented fully, and in a very interesting form, in Serret's *Higher Algebra.* Much apparently remains to be done in this field, but enough has been obtained for the object with which the inquiry originated. From Rufini's theorem, for instance, though we cannot infer strictly that the general equation of the fifth degree is irresolvable, we do infer that it cannot have a reducing equation of a lower degree than the fifth.

31. These considerations lead only to a strong presumption; and, accordingly, they have not been found sufficient to deter sanguine algebraists from attempting the resolution of the general equation of the fifth degree. Among others, the illustrious Abel made the attempt; and the result of his investigations was, a rigorous proof of the impossibility of resolving algebraically a general equation of any degree above the fourth. In this demonstration it is shown, that the assumption of the algebraic resolvability of a general equation of the fifth or higher degree leads to an impossibility: while a similar assumption for general equations of the second, third, and fourth degrees, leads to this result, that the first radical (in the order of operation) which is involved in the expression of the root must be a square root, and the second radical a cube root; conditions fulfilled in fact by Cardan's formula, and by every other algebraic expression of the roots of equations that has been yet obtained. Abel's proof has been simplified in some points by Wantzel; but as it stands at present, the principles involved in it are so very high and general as to throw it quite out of elementary science: and perhaps there is no remedy. It will be seen that the question is one of extreme generality, being properly this: Is there any algebraic function whatever of the coefficients, which, when substituted for x, shall satisfy the equation identically? It is evident that one of the first things to be done towards the solution was, to study the general form of algebraic functions. This was done by Abel: he examined and classified algebraic functions; he distinguished them as of different degrees and orders, according to the number of radicals involved in them, and the number of times that the radicals succeed one another, or are superposed upon each other: he obtained finally a general expression, comprehensive of all possible algebraic functions of a set of magnitudes. Having assumed that such an expression, substituted for the unknown, would satisfy the equation, he obtained the following result: If an equation is resolvable algebraically, such a form can be given to the root that all the algebraic functions of which it is composed shall be rational functions of the roots of the given equation. This theorem, with some others upon the changes of value undergone by a function of several letters in virtue of permutations among the letters, constitute the main principles of the demonstration.

32. The researches of Gauss upon the resolution of Binomial Equations, were published in 1801, in his *Disquisitiones Arithmeticae;* and gave a large extension and a powerful impetus to algebraic science. He proved that the equation to which we are led by the problem of the division of the circle into a prime number n of equal parts is always resolvable by radicals; and that the index of each of the radicals involved in the expressions of the roots is a prime factor of the number $n - 1$. A few notions will be now presented upon the elements of this remarkable method. It has been proved already (sect. 25), that if r be any root of the equation

$$\frac{x^n - 1}{x - 1} = 0, \ldots \qquad (1.)$$

when n is a prime number; the entire system of the roots may be represented by the series

$$r, r^2, r^3, \ldots . r^{n-1}, \qquad (2.)$$

or by the series of powers of r whose indices form the arithmetical progression

$$1, 2, 3, 4, \ldots (n-1). \qquad (3.)$$

Gauss represents the roots by the series

$$r, r^a, r^{a^2}, r^{a^3}, \ldots r^{a^{n-2}}, \qquad (4.)$$

or by the series of powers of r whose indices form the geometrical progression

$$1, a, a^2, a^3, \ldots . a^{n-2}. \qquad (5.)$$

Where it is to be remarked, as essential to the method, that the number a is a *primitive root* of n, or a number such, that all its powers from the 1st to the $(n-1)$th, divided by n, leave different remainders, that for the $(n-1)$th power being 1. The properties of primitive roots of n, as well as their existence for every value of the prime number n, are proved in all the books on the Theory of Numbers. Assuming the properties of a above stated, we obtain the following theorems:—1*st*, If the index of r in any term of series (4) leave a remainder s when divided by n, the term itself is equal to r^s. For if m be the quotient, the term is r^{mn+s}, which $= r^{mn} \cdot r_s = r^s$, because by hypothesis, $r^n = 1 = r^{mn}$. We may call r^s the reduced form of the corresponding term of (4). 2*d*, All the roots of eq. (1) are represented by the series (4). To see this, conceive another series (6) drawn out, consisting of the reduced forms (r^s) of the terms of (4). The indices s in series (6) must be all different from each other, because a is a primitive root of n; they are all integers, less than n, and their number is $n-1$. It follows that the indices of r in series (6) must form the entire series of numbers $1, 2, 3, 4 \ldots (n-1)$. And hence, each term of series (4) has a corresponding equal term in series (2), which proves the theorem. 3*d*, If the series (4) be indefinitely prolonged, it simply repeats itself. Each term of (4) is a constant function (the ath power) of the preceding term. Suppose the series (4) prolonged according to this law, and the series (5) along with it. The next term of (5) is a^{n-1}. By a property of primitive roots of n already mentioned, the number a^{n-1} divided by n leaves a remainder 1. Therefore, the first term in the prolongation of the series (4) is, in its reduced form, r; which proves the theorem. 4*th*, If $n-1 = h\,k$, h and k being whole numbers: the series (4) may be thrown into h groups or into k groups of terms, each group forming a series which proceeds according to the same law as the series (4), (each term a constant power of the preceding), and which possesses the same property of self-repetition. It will be sufficient in the proof to consider the indices of the terms. Putting $a^h = m$, we may write the series (5) in a succession of vertical columns thus:

$$\begin{array}{llllll} 1 & m & m^2 & \ldots & m^{k-1} \\ a & m\,a & m^2\,a & \ldots & m^{k-1}\,a \\ a^2 & m\,a^2 & m^2\,a^2 & \ldots & m^{k-1}\,a^2 \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ a^{h-1} & m\,a^{h-1} & m^2\,a^{h-1} & \ldots & m^{k-1}\,a^{h-1} \end{array}$$

The index of a in the last term is $h\,(k-1) + h - 1$, which $= h\,k - 1 = n - 2$, as it ought. Consider the first horizontal line; collect the corresponding set of terms out of series (4) in order: the result is, a series of k terms, each term the mth power of the preceding: and if this series be prolonged, the next index is m^k, which $= a^{hk} = a^{n-1}$, so that the series repeats itself. A similar series is found from the second line; and if it be prolonged, the next index is $m^k \cdot a$, which $= a^{n-1} \cdot a$. Now this divided by n leaves a remainder a, because the factor a^{n-1} divided by n leaves a remainder 1: so that the second series repeats itself. Similarly for all the lines. We have thus distributed the terms of series (4) into h groups of k terms, each group possessing the properties assigned. We could have distributed them likewise into k groups of h terms. And it is obvious that if h and k are composite numbers, the groups just obtained may be distributed each into a set of subordinate groups possessing the same properties, and so on; there being no stop to the process till we reach the prime factors of $n-1$. 5*th*, If the sum of the roots of eq. (1) be divided into parts, or periods, corresponding to the groups considered in the last proposition, the continued product of any number of the periods will be equal to the sum of a certain number of the same periods. This is the fundamental principle of reduction in Gauss's method. To prove it, consider merely the indices; and represent two of the periods, which are to be multiplied together by the two series

$$\begin{array}{l} b,\ b\,m,\ b\,m^2, \ldots .\ b\,m^{k-1}; \\ c,\ c\,m,\ c\,m^2, \ldots .\ c\,m^{k-1}; \end{array}$$

which are two of the lines of last proposition, b and c being some powers of a. We find as a first part of the series representing the product, the set of quantities

$$(b+c),\ (b+c)\,m,\ (b+c)\,m_2 \ldots (b+c)\,m^{k-1}.$$

As a second part (by adding each term of the lower line to that in the upper line one place to the right), we find

$$(b\,m+c),\ (b\,m+c)\,m, \ldots .\ (b\,m+c)\,m^{k-1}.$$

As a third part (by taking terms two places to the right),

$$(b\,m^2+c),\ (b\,m^2+c)\,m, \ldots .\ (b\,m^2+c)\,m^{k-1}.$$

By reference to the properties already proved, it will be seen that the k groups of terms thus found constitute the whole set of indices which represent the required product. It will be seen also that each of these groups represents some one of the h original periods. The proposition being proved for the product of two periods, it follows easily for the continued product of any number. It is evident now, that whatever be the prime number n, we can find (sect. 5) the coefficients of an equation whose roots are equal to the periods which we have been considering. In this manner we can depress the given equation to one whose degree is equal to the greatest prime factor of $n - 1$: which is the most interesting result of Gauss's method.

For example, the equation

$$x^{17} - 1 = 0$$

may be resolved (its roots expressed by radicals) by the resolution of several quadratics. For a simple exhibition of the work, see Hymers on Equations, Sect. 4.

33. In what cases can an equation be resolved algebraically? This is the highest question that has occurred in the theory of equations, and the solution of it is not yet completed. Many researches have been made upon it, at the head of which, as starting points, must be placed those of Lagrange, already noticed (sect. 29), and those of Gauss upon Binomial Equations. Abel's theorem upon non-resolvability was a very important step towards the solution, as it puts out of question all equations of the fifth and higher degrees, with indeterminate coefficients. The next advance made was a very remarkable one. We have seen that in the series of the roots of a binomial equation, as arranged by Gauss, each root is a constant power of the preceding, the first root being this same power of the last. Abel generalized upon this hint. He showed that when each of the m roots, $x_1, x_2, x_3, \ldots x_m$, of any equation is a constant rational function of the preceding, this function of x_m being farther equal to x_1, the equation is resolvable by radicals: also, that if two roots of an irreducible equation of prime degree be so connected that the one is expressible rationally as a function of the other, the equation may be resolved by radicals. (By an irreducible equation is meant one whose first member admits of no *commensurable divisor*, no divisor whose coefficients are rational functions of given numbers.) Abel improved upon Gauss's theory: he gave it completeness and symmetry, and made many valuable additions. The next important step was taken by Galois, who proved the following beautiful theorem:—To render an irreducible equation of prime degree resolvable by radicals, it is necessary and sufficient that any two of the roots being given, the others be deducible from them rationally. The inquiry has been prosecuted more recently by Kronecker, and with much success. But as the space properly allowable for this article is now more than occupied, we must refer the reader, for a full account of Kronecker's researches, to the second edition of Serret's *Algebre Superieure.*

Standard English works upon the subject: Peacock's *Algebra; Treatises on Equations* by Hymers, Young, Murphy (*Library of Useful Knowledge*), Ivory (*Encyclopædia Britannica*). The last is a very masterly production.

Equations of Condition. The Equations so designated arise as follows:—The aim of observation and experiment generally is to determine unknown quantities connected with each other by some such relation as this:

$$a\,x + a'\,y + a''\,z = 0.$$

The quantities whose value is sought, (x, y, z,) can rarely be determined directly, but it is usually possible to find numerical values for a, a', a'', &c. in separate cases. It is easy to see that if these numerical values were absolutely correct, three sets of experiments or observations would yield three equations, and therefore suffice for the determination of the three quantities, x, y, and z. But the experimental valuation of a, a', and a'', is always subject to error; and it is not therefore held sufficient to obtain only three equations. We have thus a case, exactly the reverse of that of *Indeterminate Equations*, i. e., we have many more equations than there are unknown quantities; and the question is, how shall these equations be combined so that the most accurate attainable values of x, y, and z be extracted from them? The mode of combination indicated by the term *Equations of Condition* is exceedingly simple. Suppose, for instance, that we had obtained *nine* numerical equations, these may be reduced to the requisite *three*, by a process of simple addition, viz.—

$$\begin{aligned}(1) + (2) + (3) &= 0\\(4) + (5) + (6) &= 0\\(7) + (8) + (9) &= 0;\end{aligned}$$

or, again,

$$\begin{aligned}(1) + (4) + (7) &= 0\\(2) + (5) + (8) &= 0\\(3) + (6) + (9) &= 0,\end{aligned}$$

and also in other forms. The mean of the different values of x, y, and z, will generally be pretty near the true value. It is necessary, however, to take account of the *weight* or relative importance of the several observations or experiments, and to express that relative importance numerically.—Let us follow an example of the determination of an unknown quantity, by three methods—the ordinary method, or that of the *Mean;* the method just indicated; and the method of the *Least Squares.* See MEAN, and SQUARES THE LEAST. Suppose we have three equations,

$$x - 1 = 0$$
$$10x - 11 = 0$$
$$20x - 24 = 0.$$

By taking the *mean*, we find

$$x = 1 \cdot 1.$$

By the foregoing method,

$$x = 1 \cdot 16.$$

By the method of Least Squares,

$$x = 1 \cdot 17.$$

The errors or discrepancies between these results and the three equations are as follows:

$$\text{I. } \varepsilon = + \cdot 1 : \varepsilon' = 0 : \varepsilon'' = - 2 \cdot 0$$
$$\text{II. } \varepsilon = + \cdot 16 : \varepsilon' = \cdot 6 : \varepsilon'' = - 0 \cdot 8$$
$$\text{III. } \varepsilon = + \cdot 17 : \varepsilon' = \cdot 7 : \varepsilon'' = - 0 \cdot 7.$$

There is a very serious error in the first determination as regards the third equation. The method illustrated in this article compares favourably with the method of the least squares; but the case given is not exactly a fair one.

Equation, Annual, refers to the motion of the moon round the earth. When the sun is at different distances from the earth it produces different effects of perturbation on the earth's position and on the moon's generally. It is not at the same distance from each, and does not attract them equally or make them preserve their relative positions. Thus, supposing the moon to remain constantly moving in an ellipse round the earth—which would be something like the case if they remained in a constant relative position—this inequality of position may be conceived to shift the position of the earth from the focus on either side; sometimes taking it farther from the moon, at other times bringing it nearer. Supposing the moon to describe the same spaces in her revolution in the same times, it is clear that the earth being thus at different distances from her, she will seem to a spectator to go faster when she is nearer than she does when she is farther away. She would seem to do so, even if the earth were fixed in the focus, but manifestly not in the same degree. This is the origin of the Annual Equation. See LUNAR THEORY.

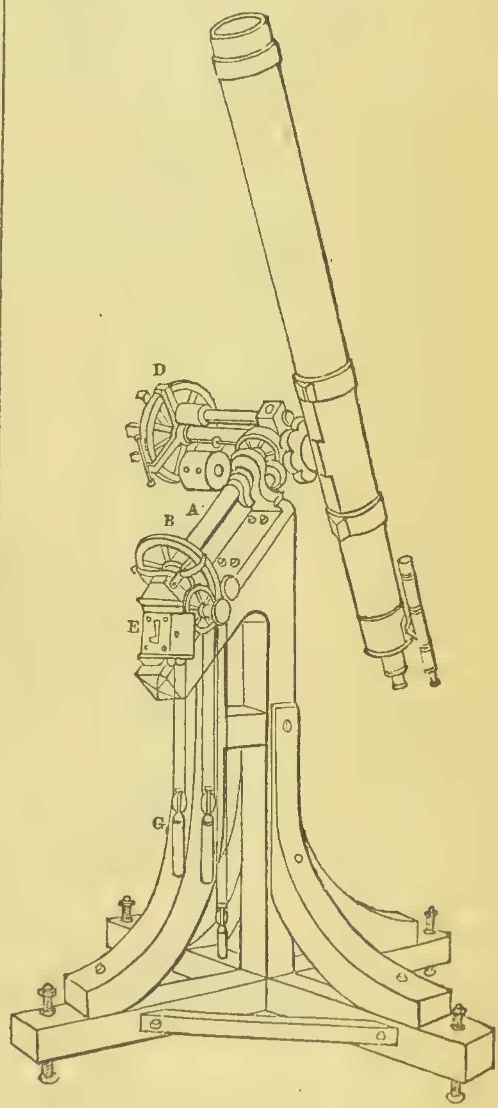
Equations, Functional. A special kind of equations, in which there is no limit whatever to the *form* in which the relations of the unknown or unknowns may be stated. In algebraical equations, they are connected by any of the ordinary algebraical or arithmetical processes, addition, subtraction, multiplication, division, involution and evolution. Here, there is no such necessity—the unknown is, besides, rather a *form* than a number or magnitude in this case. Thus the following is a functional equation:—Required such an algebraical expression as will be increased by 1, whatever value is assigned to x, when x^2 is substituted for x. It is stated so $\varphi(x^2) = \varphi(x) + 1$. Required $\varphi(x)$.

The solution of such questions is the object of the calculus of functions. See FUNCTIONS.

Equator, Celestial. That great circle in the sky, which represents the prolongation of the plane of the Equator of the Earth.

Equator, Terrestrial. The great circle on the Earth's surface, half-way between the two Poles. It divides the Earth's surface into the Northern and Southern Hemispheres.

Equatorial, or **Equatorial Telescope.** An Equatorial Telescope is a telescope so mounted as to have two axes of motion at right angles to each other; each axis carrying a graduated circle, and one of them being parallel to the axis of the Earth. In the adjoined cut, the



two axes are represented carrying the graduated circles B and D; and the axis, A, points to the pole of the Earth's axis. The consequence of this latter provision is, that so soon as the telescope is fixed on a star, it may be clamped at the

circle D; for by moving simply around the axis A, it will follow that star,—inasmuch as this axis A, is really the axis of that diurnal motion of which the star partakes. To keep the star in the field of view, it is thus needful to move the telescope in one direction only; and further, it must—since the diurnal motion of the heavens is uniform—be moved with entire uniformity round A; so that it may be moved by *clock work*. A clock, whose weights are at G, is now accordingly always attached to the axis A; and thus, after having once fixed the telescope on an object, the Observer is freed from all anxiety regarding its motion: the mechanism retains the telescope on the object; and he is not further diverted from the task of examining its nature, or ascertaining its dimensions by the microscope at the eye-piece. It were impossible to exaggerate the importance of this instrument, which has deservedly become a prime one in Observatories. There are various forms of mounting—the one in the cut being that applied by Frauenhofer in the first place, and subsequently by Merz, to the superb Refractors constructed at Munich. Other modes have also been found successful, especially the one applied by Mr. Lassell to his very large and heavy Reflector. — On the attached circles, the *Declination* and *Right Ascension* are marked by graduations of the requisite fineness; D being the declination circle, and B the circle on which the Hours of Right Ascension are indicated.—All Equatorials demand the following adjustments:—1. The Polar axis must be parallel to the axis of the Earth. 2. The index of the declination circle must point to zero, when an Equatorial star is in the centre of the field of view. 3. The line of collimation of the telescope must be perpendicular to the declination axis. 4. The declination axis must be perpendicular to the polar axis. 5. The index of the hour circle must point to zero, when the telescope is in the meridian of the place.—All these adjustments may be attained by suitable observations; and if error remains in either, it can be corrected by appropriate formulæ, to be found in any work on *Practical Astronomy*.

Equilibrium, Technical. The state of rest of a body or system, solicited by various forces. The science which treats of the equilibrium of bodies is STATICS.

Equinoctial. A synonym for the Equator, both of the earth and of the heavens. When the sun's diurnal path is along this circle there is equal length of day and night all over the world; and from this comes the name.

Equinoctial Time. As the date of a phenomenon is a fact of great importance in all physical, and especially in astronomical investigations, and as local measurements of time date from circumstances purely local, it is desirable that some instant should be selected as the central point of a uniform reckoning of time. The moment when the point of Aries passes the vernal equinox is taken as this starting point; and time dated from that, is called equinoctial time.

Equinox. The points where the ecliptic and equator cut one another in the sky are called the equinoxes, because when the sun is in them he appears, in consequence of our rotation, to describe the circle of the equator, and the day and night are equal over the world. When he is passing up from south to north he passes the *vernal* equinox (when the days are lengthening), and in passing from north to south, the *autumnal* equinox (when the days are shortening). The dwellers in the southern hemisphere have our vernal for their autumnal equinox, because while the sun is south of the equator daily, the days are long to them, and short to us. The point of the equinox does not remain stationary. Its motion is due to the perturbations of the planets, which cause the ecliptic to move; and nutation and precession both operate. See these articles.

Equivalent, Joule's.—Nearly two centuries have passed since Locke gave a definition of heat, which he probably derived from Bacon.—"Heat is a very brisk agitation of the insensible parts of the object, which produces in us that sensation from whence we denominate the object hot; so what in our sensation is *heat*, in the object is nothing but *motion*." If we add Newton's definition that the force possessed by matter is its power to persevere in its state of rest or motion; and his third law: that reaction is equal and contrary to action, a law including what is now called *conservation of force*—it is evident that at this early period the principles of a true doctrine of heat were enunciated. In the year 1798 Benjamin Count Rumford published his *Inquiry Concerning the Source of Heat which is Excited by Friction*. He caused a blunt steel borer to revolve with great friction in a hollow cylinder of iron. An amount of heat was thus generated, the experiment being sometimes carried on until water in contact with the metal was made to boil. Rumford carefully examined whether any other thermal source existed, as change of specific heat of the iron, oxidation by contact with air, &c., but found no reason to suspect that such was the case. He therefore concluded that it was "extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited, and communicated, in the manner the heat was excited and communicated in these experiments, except it be motion." Further, Rumford gave figures which expressed the relation between the work spent and the heat evolved in his experiments. The heat he estimates at 180° Fahr., in 26·58 lbs. of water; the power, barely that of one horse for two hours and a-half. If we estimate the power at $\frac{3}{4}$ that of one horse, or 24,750 foot pounds per minute, we shall obtain

$$\frac{24{,}750 \times 150}{180 \times 26{\cdot}58} = 776,$$

which is almost identical with the now ascertained equivalent.

It does not seem that any further step was taken to evaluate the mechanism of heat until Séguin published his work on Railways, in 1839. In it he states (p. 382) his belief that a certain quantity of caloric disappears in the very act of the production of mechanical force, and *vice versa*, and that the two phenomena are united to "each other by conditions which assign them invariable relations;" also at page 383 he observes, "that the mechanical force which appears during the lowering of the temperature of a gas, as of every other body which is dilated, is the measure and the representation of the diminution of heat." He then proceeds to state that the mechanical "effect obtained by the expansion of a cubical mètre of vapour compressed by a weight of two kilogrammes on the square centimètre, when it is allowed to expand into a space corresponding to a pressure of one kilogramme, and a lowering of temperature of 20°, is represented by a weight of 6.613 kilogrammes raised to the height of one mètre."

In 1842, Dr. Mayer published a paper entitled, "Remarks on the Forces of Inorganic Nature," in which, without actually going beyond Séguin, he stated the mechanical theory more explicitly, and with greater perspicuity. "Forces," he says, "are causes in which the principal *causa equat effectum* must fully hold. If a cause c produce an effect e, then $c = e$. If the cause c has produced the work e equal to it, then the cause c will have stopped, c has become changed to e, and therefore we must view c and e as different forms of one and the same object." In the case of the fall of heavy bodies the effect is heat. Adopting the hypothesis advanced by Séguin, Mayer assumes that the force employed to compress a gas is equivalent to the heat given out, and thence infers that "the fall of a weight from the height of about 365 mètres "corresponds with the elevation of a like weight of water from 0° to 1°."

It must always be matter for wonder that the work of Count Rumford attracted so little of the attention it deserved. This may be ascribed to the strong hold which the theory of caloric had upon the minds of philosophers, or perhaps that his experiments were not deemed sufficiently complete. The speculations of Séguin and Mayer have far less claim to be received as proofs of the mechanical nature of heat, or as affording sure grounds for the establishment of a numerical relation between heat and force. These speculations, so far as regards the nature of heat, are founded upon the hypothesis of the indestructibility of force, which, though apparently self-evident, and generally received by philosophers as axiomatic, nevertheless was capable of, and required experimental proof. Then an attempt is made to estimate the mechanical equivalent of heat by using the further hypothesis that in compressing an elastic fluid the heat generated is the force which has been expended. This last hypothesis has since been shown to be approximately true; but at the time it was advanced by Séguin and Mayer the published results of experiments led to a different conclusion. From all that was then known, it was impossible to predicate with certainty that it held even approximately. That such an hypothesis could not be safely received without experimental proof, is evident from the consideration that it is possible to conceive an elastic fluid, obeying the gaseous laws, which on compression shall give out no heat whatever, or even shall produce cold.

The researches of Joule date from the year 1840. In the first instance their object was to examine the relation of electricity to heat. He found that the heat evolved by a voltaic current arose solely from the resistance it experienced in passing through the wire or fluid conductor; so that, by increasing the electro-motive force, which permitted the use of a circuit of greater resistance without diminishing the flow of electricity, any amount of heat could be obtained from the same quantity of electricity. As a consequence of this law it immediately followed —1st, That, whether the circuit was composed of good or bad conductors, the same quantity of heat would be invariably produced by a given chemical effect in the battery. 2d, That the heat produced by a voltaic couple is proportional to the intensity of its electro-motive force. 3d, That the heat produced by the combustion of a metal in oxygen gas is proportional to the intensity of the force with which the elements combine, and may be calculated in the same way as if the oxygen and metal were a voltaic couple, the poles of which were connected by a resisting metallic wire. All these laws point to the conclusion that heat is derived from mechanical action, and accordingly Joule proceeded to search for the means of determining the absolute relation between the two. These he found to be presented by the electro-magnetic machine or engine. An electro-magnet, furnished with the means of reversing the current at every half revolution, was placed on a vertical axis, between the poles of a powerful magnet. When a voltaic current was made to traverse the electro-magnet, it revolved with great velocity, *developing a considerable amount of mechanical force.* At the same time the *flow of electricity was reduced in quantity* in a certain proportion as the velocity and work increased. Now Joule had already found that the quantity of heat evolved by a conductor was proportional, in a given time, to the square of the quantity of current. But, the quantity of chemical action being simply proportional to the current, according to Faraday's law, it followed that, if the evolution of heat in the revolving apparatus was governed by the same law which obtained in a stationary circuit,

there must be less heat evolved for a given chemical action when the machine was at work than when it was at rest. An examination of the heat evolved by the coils of the electro-magnet while rotating proved that this was the fact; and on comparing the defalcation of heat with the force developed, a mean of two results showed that—I. For every degree of heat per pound of water expended, 806 pounds were raised to the height of 1 foot by the work of the engine. By turning the machine in the contrary direction there was an expenditure of force, an increase in the flow of electricity, and an increase of heat over and above that due to the chemical action of the battery. In this case a mean of four experiments indicated that—II. A pound of water was raised one degree by the expenditure of a mechanical force, in turning the machine, capable of raising 962 pounds to the height of one foot. In the course of experiments, Joule had discovered that when a bar of iron is rotated between the poles of a magnet it becomes heated, the effect being considerably increased by coating it with a conducting substance, such as copper. He found, at the same time, that force was used up, and it thus appeared from a mean of seven experiments that—III. Each degree of heat required the expenditure of a force able to raise 776 pounds to the height of one foot. This last equivalent was considered to be more correct than the previous ones, on account of the greater facility with which the experiments were made. In the same paper, which was published in 1843, it is stated that its author had proved experimentally that heat is evolved by the passage of water through narrow tubes, and that —IV. In this way he had deduced for the mechanical equivalent of heat, a term which he himself was the first to employ, the number 770. An experimental investigation on the changes of temperature produced by the rarefaction and compression of air, communicated to the Royal Society in 1844, led the same author to the belief that the relation between the heat absorbed and the work performed, or between the heat evolved and the work expended, was the same as he had obtained from the electro-magnetic experiments, and he therefore concluded that the hypothesis of Séguin and Mayer, of whose writings he appears to have been then ignorant, was sensibly correct. The numbers obtained for the equivalent were—V. In the experiments on compression, 809, and—VI. In the experiments on expansion, which were susceptible of greater accuracy, 798. It was at the same time proved that no appreciable absorption of heat took place when air expanded without performing work. This has been since shown by experiments devised by Thomson, and carried out by him and Joule, to be only approximatively correct. It had frequently been observed that the temperature of the sea was raised after long continued agitation in stormy weather. About the year 1840, Mr. Dyer had an apparatus constructed for the agitation of water, in which he observed the development of heat to take place. Mayer soon afterwards announced that he had thus raised water from 12° to 13°. Others, however, denied these results Joule now returned to methods that had occupied him in 1843. His new apparatus consisted of a paddle wheel working in a vessel containing the fluid. Three researches, successively improved in accuracy, gave—VII. For the mechanical equivalent derived from the agitation of water, the numbers 890, 781, and 772. A series of experiments gave—VIII. For the equivalent obtained from the friction of oil, 782. Two researches, successively improved in accuracy, gave—IX. For the agitation of mercury, the equivalents 787, and 774—Lastly, a series of experiments gave—X. As the result of the friction of cast iron, the number 775.

Eriometer. An instrument of some importance and high interest, invented by Dr. Thomas Young.—It has been explained under Diffraction, how, if a divergent ray of light passes the borders of a hair, or fine iron thread, a series of coloured parallel fringes will be found on the edges of the shadow. Now, the distances of these fringes from the shadow, and from each other, increase as the diameter of the thread diminishes; and if, instead of a thread, a number of threads be used, crossing or interlacing with each other, the coloured fringes will be changed into concentric circles, like halos, whose diameters are inversely as the tenuity of the threads.—Again, in a thin circular plate let a very small hole be bored, and all around it, at small distances, a series of similar circular holes. Place behind the central hole, the flame of a lamp, and examine the luminous point through some substance, that it is desired to examine. Around the central hole, a halo will appear, and by moving the substance under examination nearer to, or farther from the plate (also a graduated scale), this halo may be made to coincide with the ring of holes surrounding the central one. The distance of the object or substance from the ring must then be read off at the scale; and this distance will be a correct measure of the diameter of the fibres, or globules, of which the substance is composed. Young computed in this way the diameters of a great number of substances, such as the fibres of finest cambric, the globules of the blood, &c.

Errors. It has been already explained under Correction, that no Observer ventures to assume that the results at which he arrives by aid either of his *senses* or *instruments*, can be accounted perfect. Imperfections inhere in his own *temperament*, and, it may be, in the *sense* he is using; and let the instrument be the best that ever came from Artist's hands, we may be well assured that it does not perform with absolute correctness any one of its professed functions. The

detection of the fixed errors, alike in his person and his instrument, is therefore a first duty of the Observer: and we have shown, under article already referred to, how allowances must always be made for such inaccuracies. This done, however, *errors* still remain, errors from accidental causes, or at all events of irregular origin, and not reducible to any law. For instance, suppose that the problem is to determine the exact position of some remote immovable point, the Observer finds that no two of his determinations exactly agree, neither do the observations of different observers. To insure the utmost attainable accuracy, observations are repeated on the point, and numbers of determinations accumulated; and the practical question is—*in what manner, out of all these imperfect determinations, may the truth as to the required position, be deduced as accurately as possible?* Two points bearing on this inquiry demand especial notice.—1. We shall here presume that the Observations to be dealt with, are, in so far as the Observer knows, equally good,—in other words, that he has no reason to place greater reliance on one set of them, than on another set. If this is not the case, the numbers given by the different observations must be first brought to a condition that will permit all being dealt with, as if it were the case. And for the mode of reduction, in this respect, the reader is referred to OBSERVATIONS, WEIGHT OF. The mode of combining a set of such equally probable Observations, in most general use, is, by taking what is termed their *average* or *mean: i.e.* the separate results are added together, and divided by their number,—a rule which holds as the best, in a certain number of cases, but by no means in all cases—not even in the largest and most important class. For instance, if the Observation belongs to a class of which the following may be taken as an illustration, the rule of the Arithmetical *mean* or *average* would be the true one. Suppose the Observer, seeking to deter-

Fig. 1.

mine the position of A, took five observations whose results gave him the points A^1, A^2, A^3, &c. instead of A, then, assuredly, the most probable position of A as deduced from these results, would be that of an intermediate point, the sum of whose distances from A^1, A^2, A^3, on the one side of it should just be equal to the sum of its distances from A^4 and A^5 on the other side; and this would be equivalent to the Arithmetical Mean. But suppose the points were not in a straight line, but scattered, as below, around A, in space, the Arithmetical rule would not directly apply. To find the most probable locality of A in this case, by aid of the principle of the average, we should require to enclose the foregoing polygon within rectangular co-ordinates, and find the position of A with regard to each of them. But there is a simple method,—one that contains the germ of the most general method, and which will be readily understood. The most probable position of A, will, in this case, be the *centre of figure*, or the *centre of gravity* of the polygon. But it is a property of the centre of gravity of such a polygon, that the sum of the squares of these lines, AA^1, AA^2, &c. AA^5, is less than the sum of squares of similar lines drawn from *any other point* to the angles of the polygon. These lines, AA^1, &c. are the *errors* of the separate observations; therefore, the rule for finding the average is simply this,—determine the mean point so, that the sums of the squares of the deviations of the separate Observations from it, or *the sums of the squares of the* ERRORS *shall be the least possible.* This is the root of that famous principle of the LEAST SQUARES, introduced by Gauss and Legendre, and now of universal acceptance. See SQUARES, THE LEAST.—2. The true mean, or average, thus determined, it is of greatest importance to know *how far* the mean may be supposed still *to diverge from the absolute truth*, or how *near* it may be supposed *to approach it?* In other words, what is the *probable Error* of the *Mean?* The rules above given apply to any set of Observations, limited or numerous, or whether made by a good or indifferent Observer. The *best Mean*, will in all cases be found by this rule; but the weight due to that mean, or the amount of reliance we can put on it, will, of course, vary with the circumstances just mentioned: and that variation may be expressed by the *probable error* of the *Mean.* It is clear that the value of the mean as to correctness, or its reliability, must depend on the nearness of the separate observations to it, whereas we shall measure its *unreliability*, or its probable *inaccuracy*, mainly by the remoteness of the separate observations from it. The following are the rules that guide us in our appreciation of all its qualities:—the demonstration of these rules is found in every good treatise on *Probabilities.* The qualities of a result obtained as above, that need to be determined, are its *precision*, its *weight*, its *probable error*, and its *mean error.* Taking m as the mean square of the errors, these qualities are determined by the following formulæ:—

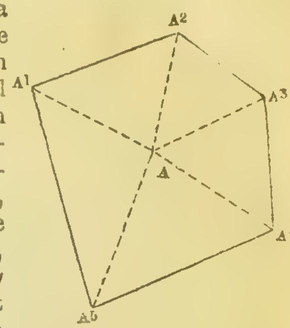


Fig. 2.

$$Precision = \sqrt{\frac{1}{2m}}.$$

$$Weight = \frac{1}{2\,m}.$$

$$Probable\ Error = \cdot 674489\,\sqrt{\ }\,m.$$

$$Mean\ Error = \cdot 398942\,\sqrt{\ }\,m.$$

Eudiometer. An instrument chiefly of chemical interest, originally employed in the analysis of atmospheric air;—a result of Priestley's great discovery of the non-homogeneous nature of airs and gases. It was thought for a long time air and gas was one simple substance. Priestley discovered that in atmospheric air there are at least two sorts of substances. The Eudiometer is an instrument invented for the sake of analyzing air quantitatively as well as qualitatively—that is, obtaining the knowledge how many proportions of each different element, a given quantity of air (*e.g.* 100 parts) contains. The principle is generally that of presenting some body to the oxygen of the air or other gas (for they are employed for analyzing all gases), which may be capable of taking it up completely, leaving the others free. The details are entirely chemical. See *Penny Cyclopædia* and *Thomson's Cyclopædia of Chemistry.*

Eunomia. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Euphrosyne. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Euterpe. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Evaporation. It appears that every substance, *solid* or *liquid*, tends to convert itself, to an extent depending on the temperature and other circumstances, into invisible vapour: water, for instance, sends off vapour from its surface at all temperatures, even when it is *ice*. This process is termed *Evaporation: Vaporization*, as distinguished from evaporation, signifies the conversion of a liquid into vapour by the mode of *ebullition*. It appears that all vapours enjoy the property of gases, in mixing with other aeriform fluids, according to their own laws, and as if they were diffused through a void. Every special atmosphere of vapour within our compound atmosphere, therefore, acts on its own particles alone: *i.e.* the aqueous vapour in the air acts by pressure only on itself. The limit to evaporation is therefore this; if an amount of vapour has already accumulated in the air, so that its weight equals the elastic force of vapour at the temperature of the surface of the exposed liquid, no more vapour will rise from that surface; and, *cœteris paribus*, the rapidity of evaporation will depend on the amount by which the weight of the vaporous atmosphere is less than this elasticity. The second main physical character of evaporation is this—it cannot take place without producing *cold*. A body in the state of vapour has a much greater capacity for heat, than when in the condition either of liquid or solid; the change of state necessarily causing absorption of heat, by the vapour, from all surrounding bodies:—a fact readily illustrated if one pours a few drops of ether, or any liquid of rapid evaporation, into the palm of the hand. If the evaporation proceed with great rapidity, this absorption becomes sufficiently powerful to produce noticeable effects. Evaporation of ether *in vacuo*, may be made to freeze mercury: ice is formed in Leslie's beautiful experiment, under the receiver of an air pump, by the evaporation of water itself: by the same power of rapid evaporation, carbonic acid gas was solidified by Thilorier, and Boutigny has recently shown us ice produced by the same energy in an *incandescent crucible!* The icy caverns of the Jura, the ice cavern of Connecticut, as well as that remarkable one of Illetzkaya, in the steppes of the Kirghis, owe their frigorific character to intense evaporation of moisture caused by the adjacency of the warm and dry external air: but it is among the general phenomena of Meteorology that this important agency plays its most conspicuous part. The reader is referred to HYDROMETEORS, HYGROMETRY, METEOROLOGY, and VAPOURS.—Instruments of several simple forms are used to note the rate of Evaporation, as an element in the meteorological character of the time of observation.—See further, HEAT, sec. 19.

Evaporation, Mechanical Theory of. In various parts of this Cyclopædia proofs are advanced of the truth of the modern theory that those affections of substances, which we term their relations to heat, spring from *motions* of their molecules. The inferences from this theory, in reference to evaporation, are curious and interesting, and are intended to be briefly explained in the present article.—The nature of the motions of the molecules of substances, in different states of aggregation—solid, liquid, or gaseous—is of course not as yet matter of experiment, and has been variously conceived; some, with Professor Rankine, prefer the vortical theory; others lean to the idea of a motion of translation or oscillation. Without rejecting the notion of vortices, which also will account for most of the phenomena, *Joule*, in 1848, represented the possible state of gases as a state in which the molecules are in rapid translation, impinging on each other like elastic balls, and finally on the sides of a vessel containing them, with a force represented by their elastic or expansive energy. Taking hydrogen gas as an example, he computed that the velocity of its particles at 60° F., and a barometrical pressure of 30 inches, must be 6,225 feet per second. *Krönig* and *Clausius* afterwards worked under the same conception.—The condition of the molecules of a liquid must be more complex and less easily realized. They may be turning round their centres of gravity, and these centres may also be moved out of their place, or have a limited motion of translation. In fact, a rotating, an oscillating, and a translatory motion may all take place simultaneously, although in

such a manner that these molecules are not thereby separated from each other, but remain within a certain volume. Let us now contemplate the state of the surface of a liquid thus constituted. Amongst those varied motions of its molecules to and fro—which must be *individually* irregular, although their *mean* is constant—it can scarcely happen otherwise than that, under a favourable co-operation of all its constituent motions (translatory, oscillating, and rotating), a molecule will separate itself with violence from its neighbours, and receding from the sphere of their action, continue its flight into the space above the liquid. Suppose that space enclosed, and empty,—it will gradually become more and more filled with such molecules, which, having assumed the motions of the molecules of a gas, will strike, as already described, on all the enclosing surfaces. But the liquid is one of these surfaces; and when a molecule strikes against that, it will most probably be absorbed or retained, in consequence of the renewed attraction of the other molecules into whose vicinity it has been driven. A state of equilibrium will ensue when the number of molecules in the enclosed space is such that, on the average, as many strike against the surface of the liquid as are expelled from it in the same time. This equilibrium, therefore, is not a state of rest or of the cessation of evaporation, but a state in which evaporation and condensation continually take place, and compensate each other in consequence of their equal intensity. The density of the vapour necessary for this compensation is evidently dependent upon the number of the molecules expelled from the surface of the liquid in the unit of time—*i. e.*, on the activity of the motions within the liquid; in other words, upon its *temperature*.—It is easy to see that the foregoing change of motion must be accompanied by the disappearance of a large amount of thermometric heat.——*Clausius*, to whom we owe the previous speculation, proceeds to explain why the presence of another gas above the liquid cannot impede its evaporation. The pressure of the gas on the liquid arises solely from the fact that, here and there, single molecules of the gas strike against the liquid's surface. But, inasmuch as these actually fill but a small part of the space, that space may virtually be considered as empty, and offering a free passage to the molecules of the liquid. "In general, these molecules will come into collision with those of the gas only at comparatively great distances from the surface, and the former will then deport themselves towards the latter, as would the molecules of any other admixed gas. We must conclude, therefore, that the liquid also expels its molecules into the space filled with gas, and that in this case also the quantity of vapour thus mixed with the gas continues to increase, until, on the whole, as many molecules of vapour strike against, and are absorbed by the surface of the liquid, as the latter itself expels: so that the number of molecules of vapour to the unit of volume requisite thereto, is the same, whether the space does or does not contain additional molecules of gas." It will be easily seen, however, that the pressure of the new or foreign gas exercises a very different influence on the *interior* of the liquid.—As in *liquids*, so in *solids*—the possibility of an evaporation may be comprehended; nevertheless, it does not follow from this that an evaporation *must* take place on the surface of all bodies. It is, in fact, readily conceivable that the mutual cohesion of the molecules of a body may be so great, that so long as the temperature does not exceed a certain limit, even the most favourable combination of the several molecular motions is not able to overcome this cohesion. It will be readily seen that the theory now given quite concords with the older speculations of Faraday.—Joule's memoir, to which we have referred, was read to the Manchester Philosophical Society on October 3, 1848, and is republished in the *Philosophical Magazine and Journal* for 1857, vol. ii. Krönig's work is entitled *Grundzüge einer Theorie der Gase.* The very interesting memoir by Clausius, "On the Nature of the Motion which we call Heat," is reprinted in the volume of the *Philosophical Magazine* just mentioned.

Evection. A lunar inequality. See LUNAR THEORY.

Evolute. A term correlative to *involute*—applied to a curve. One curve is the evolute of another when it is formed by unrolling a thread wrapped round this first curve; and to the end of which thread a pencil is attached. The involute is the curve from which the evolute is formed—the one round which the thread is wrapped. It is evident that any given curve can only have one evolute; but it is also manifest that the same evolute may be traced from many involutes. One can easily trace the evolute of a circle by taking a crown piece and unwrapping a thread as described. The circle may itself be considered as the evolute of a point. Any curve may have an infinite number of involutes. The mathematical equations which connect the evolute and involute are the following: Let

$$y = \phi\, x,$$

be the equation of the Involute; then

$$(\mathrm{X} - x) + (\mathrm{Y} - y)\,\frac{d\,y}{d\,x} = 0.$$

$$1 + \left(\frac{d\,y}{d\,x}\right)^2 - \frac{d^2\,y}{d\,x^2}\,(\mathrm{Y} - y) = 0.$$

From which x and y, being ordinates of the involute, must be eliminated. If the evolute be given and the involute be required, then $\mathrm{Y} = f x$ must be used instead of the first equation, and from this with the other two the ordinates of the evolute X and Y must be eliminated, giving a differential equation of the second order for the involute, with no means of determining the con-

stants. The number of involutes of a given evolute is therefore infinite.

Evolution. An algebraic term correlative with *involution*. In the technical sense in which the two words are commonly used they mean the *extraction of roots*, and the *raising to powers*. When any number, as 5, is multiplied by itself, we obtain 25, the *second* power, or *square* of 5. By another multiplication we obtain the *third* power, or cube; by still another, the *fourth* power, and so on, the name of the power corresponding to the number of recurrences of the quantity. Thus, $5 \times 5 \times 5 \times 5 \times 5$, is the *fifth* power (3125) of 5. This continual multiplication is called *involution*. Suppose now that we have the number 3125 before us, and that we are required to find a number, such that when it is thus multiplied 5 times into itself, the result will be 3125 *i.e.*:—to find the *fifth root* of 3125. We chance to know that the result at present is 5, but when we do not, as is oftenest the case, we have to enter into complicated processes to determine it. These processes are called *evolutions*. A more extended meaning has been given to the terms, according to which they are thus defined:—Involution is the performance of any number of successive multiplications with the same multiples, interrupted or not by additions or subtractions; and evolution is any method of finding out from the result of an involution what multiplier was employed, provided that the said method proceeds by involutions. Thus $[(2\,x + 4)\,x - 3]\,x + 10$, is the result of *involution*, which consists in the performance of the operation indicated. The deduction from the simple result, $2\,x^3 + 4\,x^2 - 3\,x + 10$, that those operations have been performed, is what *evolution* consists in. This extension of the term makes the theory of equations a branch of evolution, as unquestionably it ought to be considered. For the discovery that a certain quantity is equal to (*e.g.*) $10^3 \times a + 10^2 \times b + 10 \times c + d$, &c. though a little less complex in practice, depends on the very same principle as the more general question of evolution. In its more confined sense—in the obtaining of powers and roots of arithmetical quantities—Barlow's table of square and cube roots is invaluable to all who have many arithmetical calculations to make. In physical inquiry it is constantly required to extract roots, or to raise to powers, and the waste of time in going through very long processes would be enormous. Such tables are not only useful for determining square and cube roots, and second and third powers, but also for determining all other powers and roots by simple rules. The simple formulæ applied for these are, $(a^n)^m = a^{m\,n}$, and $a^n \times a^m = a^{m+n}$, for involution; and for evolution, $\sqrt[n]{a^{\frac{1}{m}}} = a^{\frac{1}{m\,n}}$, and $\dfrac{a^{\frac{1}{m}}}{a^{\frac{1}{n}}} = a^{\frac{1}{m} - \frac{1}{n}} = a^{\frac{n-m}{m\,n}}$. Thus, the sixth root of 15625 may be thus found, $15625^{\frac{1}{6}}$

$$= \sqrt[3]{15625^{\frac{1}{2}}} \left(a^{\frac{1}{m\,n}} = \sqrt[n]{a^{\frac{1}{m}}}\right) = \sqrt{125} = 5.$$

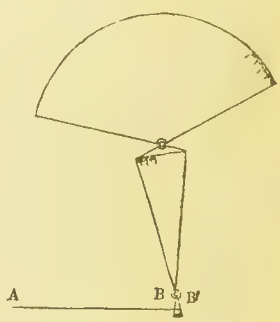
Again, the sixth power of 5, may be thus found,

$$5^6 = 5^3 \times 5^3 = 125 \times 125 = 15625$$

$(a^{m+n} = a^m \times a^n)$; or thus, $5^6 = (5^3)^2 = (125)^2 = 15625$ $[(a^n)^m = a^{m\,n}]$.

Exhaustions, Method of. The process by which the Greek Geometers passed from the areas, &c. of *rectilineal* figures to those of *curvilineal*. The latter class of questions could not, on the ground of the accepted principles of the ancient Geometry, be treated directly, or unless through such a transition; and the method of Exhaustion was the logical process of that transition. The process referred to must be carefully distinguished from one of mere approximation: the results it demonstrates are fully and rigorously demonstrated. The Greek Geometer, for instance, was not satisfied with showing that we could come as near as possible, or nearer than any given finite quantity, to the area, let us say of a circle, by increasing indefinitely the number of the sides of inserted polygons. It is true that a polygon might thus be conceived, differing from the circle by a quantity less than any that could be stated; and it might seem quite allowable to transfer any general property of the polygon to the circle itself, or to consider the two identical: but this would not have satisfied the critical spirit of these exquisite reasoners. And that there might be no *lacune* whatsoever, or the substitution of a mere approximation for the absolute truth, Euclid—in all probability—*first* sought to make the transition absolutely legitimate;—resting it on the two following propositions.—1. If from A, more than its half be taken, and from the remainder more than its half, and so on, the remainder will at last become less than B, where B is any similar magnitude named at the outset and however small. 2. Let there be two magnitudes, P and Q, both of the same kind, and let a succession of other magnitudes, which we may call P_1, P_2, P_3, be each nearer and nearer to P, so that any one, P_2, shall differ from P less than half as much as its predecessor differed. Let Q_1, Q_2, Q_3, &c. be magnitudes of the same kind in regard to Q: and let the ratios of $P_1 : Q_1$ of $P_2 : Q_2$, &c. be all the same ratio,—the ratio, we shall say, of $A : B$; then must the ratio of $P : Q$ be that of $A : B$. It will easily be seen by the student, that if P and Q be two circles, P and Q inserted squares, P_2 and Q_2 inserted octagons, &c. and A and B the squares in the diameters of the circles—it will follow at once that $P : Q = A : B$.—The Method of Exhaustions had the same relation to the modern Method of *Limits*, as the Method of *Indivisibles* to the *Infinitesimal* Method of Leibnitz.

Expansion. The two forces which chiefly operate in maintaining the old, or causing a new state of aggregation in bodies, as solid, liquid, or gaseous, are the forces of *cohesion* and *heat.* The latter is the disuniting principle, tending to render solids liquid or gaseous, and liquids gaseous. The former principle produces the reverse effect.—A certain amount of *heat*, however, is required to produce such change of aggregate condition; and quantities short of this, may be expected to produce some change, though not one so complete. In fact, heat does expand bodies, *i.e.* it drives the particles farther from another, before it produces a change of state; and this phenomenon is called *expansion.* The methods of measuring expansion are detailed in the articles PYROMETER and THERMOMETER; and we shall proceed here to give some of the results of measurement, assuming these methods to be known.—We should expect to find, and actually do, that in consequence of the difference of molecular constitution in solids and liquids and gases, there are great differences also in their capacities for expansion, and in the amounts of it under different circumstances. — In experimenting on solid bodies, it is most usual to heat long prismatic bars through certain ranges of temperature, and watch their increase of length. An important deduction from the *fact* of expansion is the danger of embedding metals—bodies readily expanding or contracting — in buildings, the stone and wood of which expand but little. In winter, the metal draws the stone towards itself, by contraction, and in summer, pushes it away—so, breaking up the stone. If the metal cannot quite free itself from its connection, it will destroy the building. Several buildings have required to be taken to pieces, because the architect very imprudently made use of iron bars for increase of security.—The *amount* of this expansion, as we have said, is usually measured by the dilatation in length, of rods of the substance in question, heated through a given range of temperature. Such a method is evidently very convenient, as the amount wanted, is capable so of easy measurement. But in liquids and gases we cannot employ it. We cannot take a bar of air or water, and measure how much longer it becomes, under a given amount of heat. We must, therefore, find some relation between the methods of measuring expansion by linear dilatation, and increase of volume. Suppose a rectangular prismatic bar, whose length, breadth, and thickness are a, b, c, respectively. Its contents will bear a certain ratio to the regular cube a^3. If it be expanded, it will continue to bear the same ratio to the cube of the side corresponding to a, for all parts expand proportionally. Hence, if h be the linear dilatation of a, the new length will be $a+h$, and the ratio of the contents or volumes will be $a^3 : (a+h)^3$. The measure of linear dilatation is then $\frac{(a+h)-a}{a}$, and of cubical expansion $\frac{(a+h)^3-a^3}{a^3}$, the first equal to $\frac{h}{a}$, and the second to $\frac{{}^2\ h33\ h+a^2+h^3}{a^3}$, or to $\frac{3\,h}{a}+\frac{3\,h^2}{a^2}+\frac{h^3}{a^3}$. Now, in all cases of solid expansion, $\frac{h}{a}$ the linear dilatation is a very small fraction. Thus, for *tin* heated from 32° to 212°, it is only 1-462. The ratio of cubical expansion to linear dilatation, which is $\left(\frac{3\,h}{a}+\frac{3\,h^2}{a^2}+\frac{h^3}{a^3}\right)\div\frac{h}{a}$, or $3+3\,\frac{h}{a}+\frac{h^2}{a^2}$, differ very little, indeed, from 3, and we may take this rule when we are not requiring exactness within, perhaps, 1-150th of the whole quantity measured, that the cubical expansion may be found *by trebling the linear dilatation.*—In measuring the amount of such expansion, in any case, the great difficulty is to avoid errors in the detection of a quantity so small. The methods employed to magnify the visible result are in principle the same as this one. The bar, A B (see figure) is heated, and expands. It is kept firmly fixed at one end, and at the other end pushes against the short end of a lever. The long end moves through a space so much larger than the bar and the short one which moves with it, as it is longer than the short one. If this long end move the short end of another lever, we may have the required quantity again multiplied, and so indefinitely.



This same principle of multiplying small motions by levers, in order that they may become more easily measured, is very frequently employed in similar determinations.—Another requirement in making such measures, is, that the moving bar be uniformly heated, so that the real dilatation of the bar may be due to precisely the amount of heat which you measure. Lamps arranged under it, at equal distances, are sometimes employed for this, but the more usual, and much the better method, is to place it in a vessel of liquid, at one extremity of your temperature range, leaving it there till it gets time to take its temperature exactly, and then heat the liquid up to the other extremity of your temperature range, giving the bar time to assume perfectly the same temperature also. Taking this last method, the following measures of linear dilatation are obtained:—

Substances. Heated from 32° to 212°.	Linear Dilatation. Decimals.	Vulgar Fractions.
Lavoisier and Laplace.		
English flint Glass..........	·00081166	1-1248
Ordinary French Glass......	·00091750 to	1-1090 to
.. ..	·00087199	1-1147
Steel (tempered)............	·00123956	1-807
Steel (not tempered)........	·00107880	1-927
Soft Iron....................	·00122045	1-819
Standard Gold..............	·00151361	1-661
Copper......................	·00171733	1-582
Brass........................	·00186670	1-535
Standard Silver..............	·00190868	1-524
Tin (Indian)..................	·00193765	1-516
Tin (Falmouth)..............	·00217298	1-462
Lead........................	·00284836	1-351
Smeaton's observations give—		
White Glass (Barometers) ...	·00083333	1-1175
Steel (not tempered)........	·00115000	1-870
— (tempered)............	·00122500	1-816
Iron..........................	·00125833	1-795
Bismuth......................	·00139167	1-719
Copper-yellow, from mould..	·00187500	1-533
Copper (hammered).........	·00170000	1-588
Brass and Speculum metal...	·00193333	1-517
Pure Tin......................	·00228333	1-438
Lead..........................	·00286667	1-349
Zinc..........................	·00294167	1-340
Troughton's observations give—		
Platinum......................	·00099180	1-008
Steel..........................	·00118990	1-840
Iron-thread from draw plate.	·00144010	1-644
Copper......................	·00191880	1-521
Silver........................	·00208260	1-480
Dulong and Petit give—		
Platinum......................	·00088420	1-1131
Glass..........................	·00086133	1-1161
Iron..........................	·00118210	1-846
Copper......................	·00171820	1-582

This table gives, pretty accurately, the observed dilatations within the range of temperatures, 32° and 212°. When we proceed to apply it to other temperatures, however, we find it at fault; the cause being, that the amount of dilatation is not the same for equal increase of measurable temperature from different starting points. It requires a different amount of heat—less considerably—to raise a body from 212° to 213°, than from 32° to 33°. The cause of this one can understand. In solids, there is a balance of cohesive and expansive forces. After these latter become considerably developed, the particles of bodies are much farther distant than before. Now, cohesion, like all other forces that we know of, diminishes with distance, and therefore, new heat, in causing still farther separation of the particles of bodies, has to act against considerably less resistance than before. It accomplishes more sensible work, therefore, *i.e.* gives a larger dilatation. The subject has not been much experimented upon, but the following few results are trustworthy:—

	Linear dilatation in rise from 212° to 213°	and from 572° to 573°
Glass,	1-69660	1-59220
Platinum,	1-67860	1-65340
Iron,	1-50760	1-40673
Copper,	1-34120	1-31860

As platinum is here least irregular in its expansion, giving dilatation most nearly equal for the equal rise of temperature, it would be best fitted for a metallic thermometer.—The difference in the rates of expansion of metals can be employed as a very delicate thermometric means. Two metals are combined which expand differently, and this difference is measured. Breguet's thermometer is an admirable illustration of this. See Thermometer.—We have said that the rate of dilatation is increased for equal ascents from increased temperatures. This is generally, but not always true. Sometimes, on the contrary, the expansion takes a turn downwards, and the body, if heated further, will expand, but will also do so if cooled down. In passing, therefore, up to this point of greatest density, it has contracted instead of expanded. This property exists also in liquids, as we shall immediately see. Of solids, the one which best exemplifies it is that called Rose's fusible metal, which, when heated from 32° to 111°, expands from 100 to 100·83 parts. On heating from 111° to 156°, the body contracts instead of expanding, until at 156°, the body is only 99·291 in volume. From 156° to 178° it expands, rising to 100, and from 178° up to 201°, continues to expand, reaching 100·862 parts, and there it begins to melt. It is curious that the body has no point of maximum density when in this liquid state, as most other liquids have.—Passing to the expansion of liquids, we find this same phenomenon. When a liquid is heated from a certain point, it frequently—although not always—contracts, instead of expanding, and expands in coming down from that point. The general law seems to be, as in solids, that the amount of dilatation for equal rises of temperature is greater the greater the initial temperature is. The explanation is probably quite the same as that given for solids, that there is a diminished cohesive force, against which the expansive power of heat must act. The amount of expansion of some liquids in passing through 180° Fahr. is herewith given. Alcohol, 1-9—Nitric Acid, 1-9—Fixed Oils, 1-12—Sulphuric Ether, 1-14—Oil of Turpentine, 1-14—Sulphuric Acid, 1-17—Water, 1-23—Mercury, 1-55. In comparing the expansions of liquids, it has been well noted by Guy Lussac, that we must take them at the same point where the cohesive forces act similarly; since these so modify the visible results due to the expansive forces. Taking this principle, we find such tables as the following. The starting point in each case is the boiling point of the liquid, for Water, 212°; for Alcohol, 173°; for Sulphuret of Carbon, 134°; for Sulphuric Ether, 96°·3. Taking 1,000 volumes of each, and allowing them to cool—

Through	Water Constants.	Alcohol.	Sulphuret of Carbon.	Ether.
18°	6·61	11·43	12·01	16·17
36	13·15	24·34	23·80	31·83
54	18·85	34·74	35·06	46·42
72	24·10	45·68	45·77	58·77
90	28·58	56·02	56·28	72·01
108	32·42	65·96	66.21	

The remarkable coincidence in the contractions of alcohol and sulphuret of carbon, here given, shows a similarity, probably an identity, of molecular constitution. The forces which bind two such dissimilar bodies together must act in the same way, and the discovery of such a relation is only one instance of the value of the principle. A glance at this table will show the reader that the law of increase of dilatation with temperature, already noticed in solids, holds here also.—It must be remembered in comparing such expansions and contractions with those given for solids—that in the latter *linear* dilatation, *here cubical* expansion is noted. The method of reducing the one to the other has been sufficiently indicated.—The fact of a maximum density in many liquids was first noticed in water; and its value in the economy of nature can be best seen by it. If ice were heavier than water, as water at 39° is heavier than water at 40°, ice would be found only at the bottom of our streams, or rather, formed in its passage to the bottom, would sink to it, and when a sufficient degree of cold was obtained, they would freeze up from the bottom, making the whole water one solid mass. As it is, ice at 32° is lighter than water, and water at lower temperatures below 39° is lighter than water at higher temperatures still below that point, and therefore in the process of freezing, the water which is sinking in temperature from 39° to 32° keeps at the top, and when ice is formed it is on the top and remains there. Now ice does not transmit heat very well—not nearly so well as water, and the warmth of the water below is therefore not so quickly given away to the cold spaces above as it would otherwise be—(scarce at all by conduction through ice, and in neither case much by radiation), or if the ice were at the bottom and the warmer water was in direct contact with the air. The complete freezing of our rivers and many of our seas, probably uncompensated by corresponding thaws in summer, would render navigation most difficult or altogether impossible except in the seas of the temperate and torrid zones. The same property is quite as noticeable, however, in other bodies as in water. In fact most liquids, in passing to the solid state, exemplify it. Thus melted metals poured into a mould would give no impress of it if they contracted constantly as they cooled—or by no means so good a one as they now do. From certain points of temperature they are expanded in cooling, and their expanding mass presses with great violence against the sides of the mould. The theory, which alone seems to give plausible explanation of the phenomenon—and which is still, unfortunately, a mere theory, is this, that such bodies *crystallize* before we see them do so. Now, bodies crystallizing take up generally more space than before. The peculiar constitution of crystalline bodies does not admit of great economy of space. The particles assume a definite arrangement, and vacua result in the spaces which they prevent each other from filling up. Hence it is natural that bodies expand in crystallizing, even though the cold under which they do so, tends to make them contract. If the theory be true, the amounts of heat given off by the fluid during the expansion of cooling ought to be considerably greater than for equal diminutions of temperature at other points of the scale—just as in the actual crystallization of ice there are 142° of heat given out without any noticeable lessening of the temperature. One considerable difficulty in the way of this hypothesis is its seeming inadequacy to explain the perfectly analogous phenomenon of Rose's fusible metal. The liquid and the solid have maximum densities in all likelihood for the same physical reason, and it does not seem possible to suppose crystallization in the process of cooling from 156° to 111°, while it does not take place from 110° to 32°, nor from 211° to 156°. Until some physical fact more than the mere expansion due to crystallizing can be brought forward, it must remain a hypothesis, with no other value than as indicating a direction in which researches should be carried on. As the amount of expansion of water for the ordinary range will give a notion of the like expansion in other bodies, and is besides of very great interest in itself, we give Despretz's table of its volumes for increase of 1° at each successive degree :—

Cent.	Fahr.	Volumes.	Cent.	Fahr.	Volumes.
9°	15 8	1·0016311	34	93·2	1·00555
8	17·6	1·0013734	35	95	1·00593
7	19·4	1·0011354	36	96·8	1·00624
6	21·2	1·0009184	37	98·6	1·00661
5	23	1·0006987	38	100·4	1·00699
4	24·8	1·0005619	39	102·2	1·00734
3	26·6	1·0004222	40	104	1·00773
2	28·4	1·0003077	41	105·8	1·00812
1	30·2	1·0002138	42	107·6	1·00853
0	32	1·0001269	43	109·4	1·00894
1	33·8	1·0000730	44	111·2	1·00938
2	35·0	1·0000331	45	113	1·00985
3	37·4	1·0000083	46°	114·8	1·01020
4	39·2	1·0000000	47	116·6	1·01067
5	41	1·0000082	48	118·4	1·01109
6	42·8	1 0000309	49	120·2	1157
7	44·6	1·0000708	50	122	1205
8	46·4	1·0001216	51	123·8	1248
9	48 2	1·0001879	52	125·6	1297
10	50	1·0002684	53	127·4	1345
11	51·8	1·0003598	54	129 2	1395
12	53·6	1·0004724	55	131	1445
13	55·4	1·0005862	56	132·8	1495
14	57·2	1·0007146	57	134·6	1547
15	59	1·0008751	58	136·4	1597
16	60·8	1·0010215	59	138·2	1647
17	62·6	1·0012067	60	140	1698
18	64·4	1·00139	61	141·8	1752
19	66·2	1·00158	62	143·5	1809
20	68·0	1·00179	63	145·4	1862
21	69·8	1·00200	64	147·2	1913
22	71·6	1·00222	65	149	1967
23	73·4	1·00244	66	150·8	2025
24	75·2	1·00271	67	152·6	2085
25	77·0	1·00293	68	154·4	2144
26	78·8	1·00321	69	156·2	2200
27	80·6	1·00345	70	158	2255
28	82·4	1·00374	71	159·8	2315
29	84·2	1·00403	72	161·6	2375
30	86	1·00433	73	163·4	2440
31	87·8	1·00463	74	165·2	2499
32	89·6	1·00494	75	167·0	2562
33	91·4	1·00525	76	168·8	2631

Cent.	Fahr.	Volumes.	Cent.	Fahr.	Volumes.
77	170·6	2694	89	192·2	3500
78	172·4	2761	90	194	3566
79	174·2	2823	91	195·8	3639
80	176	2885	92	197·6	3710
81	177·8	2954	93	199·4	3782
82	179·6	3022	94	201·2	3852
83	181·4	3090	95	203	3925
84	183·2	3156	96	204·8	3999
85	185	3225	97	206·6	1·04077
86	186·8	3293	98	208·4	1·04153
87	188·6	3361	99	210·2	1·04223
88	190·4	3430	100	212	1·04315

The maximum of density in water is therefore about 4° centigrade, or 39·2 Fahr. degrees. In sea water it is 25·39 Fahr. In alcohol 36·14. In sulphuric acid of different strengths it ranges from 30·92 to 26·534, and so on for other liquids. —In gases the laws of expansion are much more simple. This is precisely what we should expect. All gases are in the same condition as regards the cohesive forces;—the cohesive force being simply non-existent in them. Hence the expansive force of heat does not act against a resistance growing weaker and weaker every moment, but not against any resisting force at all. We should expect from this want of cohesion that all gases expand alike for given increase of heat; as each gas does expand alike for equal increase of heat to itself when rising from different temperatures. It was long supposed that this was the case; but the singularly accurate investigations of Regnault have proved the incorrectness of the notion. Thus common air expands, under certain pressure, from 1,000 volumes to 1,366 in passing from 32° to 212°, and in the same passage 1,000 volumes of carbonic acid become 1,371, of nitrous oxide 1,372, of cyanogen 1,388, of sulphurous acid 1,390. These results lead us to infer that the usual postulate that the particles of gases do not at all act upon one another so as to resist expansion, is somewhat hasty; and the subject offers an admirable field for further investigation and for enlarged theoretical views. It is worth notice that those gases are most expansible which, when subjected to pressure, liquefy most readily. Thus, sulphurous acid liquefies under a pressure of two atmospheres, and expands 390 thousandths of its bulk in passing up from 32° to 212°. Carbonic acid again takes 36 atmospheres to liquefy it, and accordingly only expands 371 thousandths of its bulk, while ordinary air, quite incapable of liquefaction, by a pressure at all events of 800 atmospheres, only expands 366 thousandths of its bulk. This remarkable agreement in nature it is not easy to represent in theory. It should follow that there is certain molecular action between the particles of gases besides the ordinary repulsive action attributed to them, and that there may be different amounts of dilatation for the same rise of temperature from different points on the thermometric scale. In fact we actually find such differences. Thus in expanding from—33° to 32° Fahr., 8,650 volumes of air become 10,000, and there is for every rise of a degree an expansion of 20·77 ten thousandths. From 32° to 212°, the 10,000 parts become 13,750, being 20·83 for a degree. From this to 300° they become 15,576, being 20·70. From this to 387° we get 17,389, or 20·82 per degree. From this to 473°, 19,189, or 20·84. From this to 559°, 20,976, or 20·83, and from this to 660° they become 23,125, or 20·9 for a degree. These results, though not quite accurate, as later experiments by Rudberg, Magnus, and Regnault have shown, sufficiently indicate the result. These latter, which are very accurately made and may be fully relied on, give 2·033 thousandths or $\frac{1}{492}$ as the cubical expansion of air for each rise of a degree between 32° and 212°. For each rise elsewhere also; for the differences, as the above list shows us, are very small and almost inconsiderable. In consequence of this, the volume of a gas obtained by measurement at a given temperature must either be stated along with the temperature (*e.g.* 20ft. hydrogen gas at 60°), or must be corrected for variations of its heat, and reduced to the volume which would be occupied at a given temperature. Taking atmospheric air as an example, a volume, say 100 cubic inches of air at 132°, must be reduced to so many at 32°, when we wish to compare its volume with a series of such volumes.—In connection with this subject, it may be proper to notice here that the steam engine was originally driven by steam of not very high pressure, which was kept pouring into the piston during the whole stroke. It was found economical to use high pressure steam instead of low pressure steam, prepared as in Papin's digester. See DIGESTER. This, however, could not be let in during the whole stroke, for it would send the piston alternately against the top and bottom of the cylinder with a violence that would cause great inconvenience in any building in which the engine might be placed. Besides, there would be an enormous proportion of the work of the engine used in merely producing this mischievous effect. It was evident, therefore, that (even with low pressure engines) this must somehow be stopped. This knocking of the piston on the ends of the cylinder did much harm to the machine, and cost a large part of the fuel. The idea was suggested to Watt, that in order to prevent it it might be possible to admit the steam for only half the stroke. This would give a certain amount of velocity to the piston rod, and when the steam was cut off that velocity would continue diminishing if all force upon the rod were immediately to cease. But in the ordinary engine the piston is pushed towards vacuum, or rather against a resistance something like 4 or 5 lbs. per square inch. Now the steam admitted into the cylinder in an engine may be, suppose of 20 lbs. pressure per square inch. Let it be cut off after one-fourth of the stroke of the piston is accomplished. Then by Marriotte's law,

it goes on through the rest of the stroke constantly diminishing in elastic force. At one-half of the stroke it has come to have a pressure of only 10 lbs. per square inch—half of what it was left with. In coming up to the end of the next quarter it lowers its pressure to $6\frac{1}{2}$ feet, and at the top it has a pressure of 5 feet. In the first quarter of motion then, the piston has moved, because of the excess of the one pressure 20 lbs. over the other, 5 lbs. per square inch, and therefore has moved with an effective pressure of 15 lbs. per square inch through it. This gives it a very considerable velocity, which it is its tendency to keep and the tendency of the friction and other causes to destroy. During the next quarter it moves with an excess of pressure diminishing from 15 to 5 lbs., but always acting in the same direction. As the piston tends of itself, without any pressure, to keep its old velocity, this constant pressure will probably be sufficient to increase the velocity, though the steam has been cut off, and the body tends again to go on with this velocity. During the next quarter the excess, still in the same direction, goes down from 5 to $1\frac{1}{3}$ lbs. per square inch, and this is perhaps sufficient in ordinary engines to prevent the friction from diminishing the velocity with which the piston started in this quarter, and in the next quarter it will go down from $1\frac{1}{3}$ lbs. to zero, and be perhaps capable of preventing any very rapid decrease of velocity, certainly letting the body be carried up fully to the end. Here, therefore, only one-fourth of the steam has been used and the same motion produced. The same velocity indeed has not, but if the steam which enters came with a pressure of 30 instead of 20 lbs., there would be quite as much of this velocity obtained, while the employment of steam of that pressure, desirable on account of its economy, is not possible with the old methods. Such is the *principle of expansion*, without which the modern steam engine would be a clumsy and inconvenient contrivance, and to which chiefly are due all that perfect quietness of movement which is so wonderful to those who see for the first time the engines of a first-rate steamer at work. An idea of the value of this contrivance in point of economy may be conceived from this table:—

If the steam be stopped at the piston stroke	The performance is multiplied by
One-half	1·7
One-third	2·1
One-fourth	2·4
One-fifth	2·6
One-sixth	2·8
One-seventh	3
One-eighth	3·2

Methods of calculating the value of this economy more accurately depend upon the higher mathematics; but the above table will give the reader a notion how much can be thus done. The simplest general rule we can give is, that the proportions of work done by the same fuel are expressed as equal to 1 + Neperian log (1 + 4), taking for example the case of steam cut off at one-fourth of the stroke; and always taking, instead of 4, the reciprocal of the fraction of the stroke through which the steam is freely admitted. The importance of the utmost economy of fuel for mercantile purposes, and especially for the purposes of steam navigation, has been already dwelt upon at length in the article AIR ENGINE *(q.v.)* See further ELASTICITY; and HEAT, *sections* 10, 14, 16, and 18.

Exponent. The number indicating the degree of power or a root. Thus in x^3, 3 is the exponent of the given power of x, and $\sqrt[4]{x}$, 4 is the exponent of the given root. Descartes originated the use of exponents. Before his time the quantity a^3 would have been written *a a a*.

Exponential. Quantities representing powers whose exponents are variable. Thus a^x is called an exponential. Quantities in the expression of which such a simple quantity is contained are also so called.

Eye. The apparatus by which the sensation of vision in the animal body is attained has at all times been regarded with interest by the scientific observer from the time of Aristotle down to our own day; and even now, when optical and physiological research has advanced so far, it cannot yet be said that the subject of this article is fully understood. In the lowest or most simply constructed animals the organ of sight seems to be confined to a mere eye spot, as it is called, or a slight expansion of a nerve on which the rays of light impinge, and as we may suppose, communicate vague sensations of vision. Throughout the animal series many instructive and remarkable variations in the structure of the visual apparatus are observed, but in all, the essential parts seem to be a sensitive nervous expansion on which the light is to be received, a protecting cavity to keep out the general glare of rays from other objects than those to which the vision is directed, and an arrangement more or less complete for the formation of an *image* on the sensitive nervous surface. In what follows, attention will be directed to the human eye as being the most perfect, and the type of all the others. Only so much of the anatomy and nomenclature will be given as is necessary for reference in what follows. The eyeball is about nine-tenths of an inch in diameter from before backwards. It is nearly spherical, except at the front part, where there is a slight projection of the transparent part or *cornea*, a tough membrane about $\frac{1}{36}$ of an inch thick throughout. The *cornea* C is fitted at its borders into the front

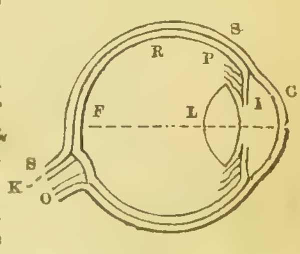


Fig. 1.

opening in the *sclerotic* S, a tough and strong opaque coat which, under the common designation of the *white of the eye*, gives form to the ball and protection to the soft parts within, whilst at the same time it perfectly excludes all light except what enters by the cornea.—Lining the sclerotic interiorly is the *choroid* K, a thin membrane covered by the black substance or *pigmentum nigrum*, which, by absorbing the rays of light, causes the pupil of the eye to look black, and which answers the same purpose as the black lining of the camera obscura, viz., the prevention of internal reflection and the consequent confusion of rays which would ensue. Within the choroid, and also forming a concentric coat, is spread the delicate and nearly transparent membrane, the *retina* R. The retina is in direct communication with the chord coming from the brain called the *optic nerve* O. The optic nerve does not enter at exactly the back part of the eye, but at a point nearly $\frac{1}{8}$th of an inch nearer the nose. At the extremity of the line running directly from the middle point of the cornea through the centre of the eye-ball, which is called the axis of the eye, there is a small perfectly transparent spot with a yellowish margin named the *foramen centrale* F. It has been thought by many to be altogether destitute of the nervous substance of the retina, and hence called a foramen or hole, and yet it would appear to be the seat of the most distinct vision, as will afterwards be referred to. It is, however, not a hole. The interior of the eye is divided into the *anterior* and *posterior* chamber by means of the moveable curtain called the *Iris* I. The Iris is the coloured part seen on looking through the cornea; it is pierced by the opening of the pupil and is formed of circular and radiating muscular fibres which give it the admirable property of contracting and expanding that aperture so as to modify the amount of light admitted to the interior.—In front of and behind the Iris is the *aqueous humour*, in which it moves and which serves to distend the cornea. Almost immediately behind the Iris the remarkable structure called the *crystalline body* or *lens* L of the eye is situated. It is a dense and transparent membranous substance in the form of a double convex lens, the front surface being less curved than the other. The lens is attached by its outer border to the ciliary processes P or folds forming the front edge of the choroid, or at least intimately connected with it.—The *vitreous humour* is of a jelly-like consistency, and serves as a support for the coatings of the eye—it fills the whole cavity of the ball behind the lens and has the retina expanded on its surface.—From this brief description it is evident that the eye consists essentially of a spherical chamber lined with a dark curtain and having an opening in front to admit a limited quantity of light which, in passing back towards the sensitive screen or retina, traverses the different transparent humours with which the interior is filled. Were there no special adaptation of the interior humours we can easily perceive that the rays of light proceeding from the point A of an external object would, in passing through the pupil and falling on the retina at the back of the ball, be diffused over a space *a a'* and constitute a confused spot of light. So also with the rays proceeding from another point B, and likewise from all the other points of the object, and thus the impression made by light from one point would be interfered with by those from another, so that no distinct indication of form could result. This could be prevented in two ways, either by limiting the size of the opening in the front of the eye to a mere point, or by interposing a lens or other apparatus in the course of the rays whereby they might be *arranged*, the whole cone from each *point* which entered the pupil being again collected into a single *point* on the retina, whereby no point would interfere with another. The first mode, viz., that of restricting the opening of the pupil to a mere point, is represented in the figure, where only an extremely attenuated pencil of light being admitted from each point it cannot spread itself over and interfere with those from the neighbouring points. It is evident, however, that a very faint image would result from this restriction of the light, so the other method has been adopted, viz., that of interposing in the course of the rays behind the pupil a *lens* of such power as to refract all the rays which strike it from each point to meet in another point on the back of the eye, as represented in fig. 4. In this way each point has its corresponding point depicted on the retina, so as to constitute an *image* as it is called.—That such an image is really formed can be seen by taking the eye of an ox or sheep and with a sharp knife cutting cautiously away the sclerotic coat at the back part till it becomes sufficiently translucent to allow of the observation, when, if the pupil be directed to a window or other luminous object a distinct and well defined picture may be observed depicted on its interior. In this way the eye is seen to bear a close analogy to a *camera obscura*, and most of its peculiarities may be illustrated

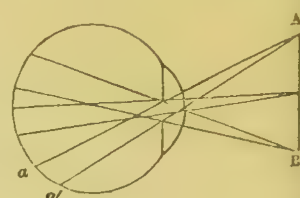


Fig. 2.

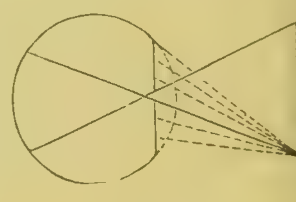


Fig. 3.

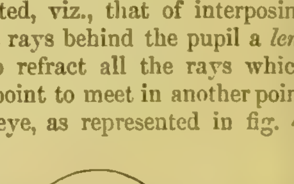


Fig. 4.

by means of that instrument. The use of the lens may also be made matter of demonstration in this experiment, for if a needle be taken and pushed through the side of the sclerotic coat a little behind its junction with the cornea it may be made to depress the lens so as to remove it from the course of the rays. The image of the distant window will then be seen to disappear on the back of the eye, and a confused glare of light to take its place, just as in the case of the camera obscura when its lens is taken away. There is no doubt that in the eye, as in the camera, the function of the lens is to arrange the rays of light into a distinct image, by concentrating the whole pencil which enters the pupil from each external point into another point similar and similarly situated in the back part of the darkened chamber. Thus far all Anatomists, Physiologists, and Opticians are agreed. The pupil admits and regulates the quantity of light; the lens arranges it into an image, and the concave surface at the back of the ball receives that image, but in what manner that image becomes converted into a mental perception has not been so clearly made out. Perhaps an unnecessary degree of mystery has been introduced into the question by the mode of stating it. We are asked how it is that the image on the eye can be transferred as a corresponding and exactly similarly arranged impression to the brain, yet the channel of communication to be merely the pulpy cord of the optic nerve. In answer to this it might be stated that there is not more difficulty in perceiving the mode of action of the sense of vision than of the sense of touch or of any other sense, if we merely grant that the rays of light have the property of exciting the sensation of light. For though it is true that if any nerve, such for instance as that of one of the fingers, be severed from communication with the brain all sensation in that nerve will cease, yet it is still equally true that the sensation in the ordinary state of the nerve occurs in it, that is, the finger, and not at a distance in the brain, as the ordinary language of physiologists might at first sight inculcate. If, then, in the sense of vision, the perception occurs in the eye itself on the expanded portion or surface of brain called the retina, then there is no difficulty in perceiving how a sensation similarly arranged in its parts to the external series of objects emitting the rays should be produced, as each point acts for itself and is impressed only by the point corresponding to it exteriorly. No doubt it may be objected to this assertion of the percipient faculty being in the eye itself and not in the brain—that if the optic nerve be severed vision is gone; but this is no answer, as *then* the normal arrangements of the parts are destroyed—vision may still be produced and yet the conscious perception of it by the other parts of the organization be wanting. The optic nerve may merely serve for producing the unity of action between the eye and the other parts of the system in the same way as the uniting filaments which pass between the different ganglions act, as is well seen in the articulated animals, such as centipedes, where, if several joints be severed from the posterior part of the body they will still continue to move, though the head and other parts of the animal will be entirely unconscious of the continuance of that action. It would seem, then, that there is not more difficulty in conceiving the mental perception of the image in the back part of the eyeball than in understanding the action of any other sensation, and that the question has perhaps been rendered unnecessarily complicated by reference to the conveyance of sensations along fibres to distant points, there to make their mental impression with relative arrangement and position of all its multitudinous parts as in the external scene which has emitted the luminous radiations. It is undoubtedly possible that by the analogy of the telegraphic wires we may conceive an arrangement by which from each point of the retina there should arise a distinct fibre which should proceed along the optic nerve to a corresponding point in the brain where it should transmit its impression, and that every point of the surface of the retina should in the same way have its own fibre originating there and passing behind the other points to the optic nerve and along it to the brain, where there would thus be a series of as many points as in the retina similarly placed, and that the impressions there produced should be read off by the mind. But it is evident that it would have been as easy at first to suppose that this reading off power was in the eye itself, and that it required no such transmission of impressions. It is futile to object that if the eye be severed from the brain its power is gone, and yet that none of the mind is gone, and thus that the mind could not have been in the eye, as this is a mere untruth; the power of vision is gone and this is part of the mind.—Passing, however, from such discussions, which savour too much of mere speculation, it must be stated that Physiologists are divided in opinion as to whether the retina or the choroid is the seat of the perception of light. No doubt both are supplied with nerves, so, without any violation of the analogies of the other senses, either may be the seat of sensation, and probably both are to some extent. The cause of doubt as to the retina being the seat of vision was Marriotte's discovery of the fact that the part of the retina where the optic nerve itself enters the eye is insensible to distinct vision. The mode of proving this is related in all works on optics. But certainly the mere fact that the bulb or termination of the optic nerve where it diverges into the retina is incapable of giving a distinct image is very far from proving that the retina is not the seat of distinct vision, as at this point the retina is differently arranged from what occurs in other situations, the fibres from all points are here collected and pass directly back to the optic nerve, so they may easily be supposed to be in a

state less fit for receiving a distinct image. It appears then rather from the fact that light *is* perceived by the termination of the optic nerve, though no *distinct* impression is produced, that the optic nerve and its continuation, the retina, are the seat of vision. Another fact has lately been brought forward in favour of the choroid as being the seat of vision, that in the cuttle-fish an opaque membrane is interposed in front of the retina so that the impression can only be conveyed to the latter by the impressions of this membrane; but even if this observation on the eye of the cuttle-fish were thoroughly confirmed, of course it is also an argument against the choroid being the seat of vision, as the membrane is likewise in front of *it*. Again, it has been said by Sir David Brewster, that in the eyes of young persons the choroid reflects a pinkish light which can be seen after it has emerged from the pupil, and that as this light must have passed through the retina it ought to have excited a sensation of the same colour in it if the retina were the seat of vision. In answer to this, it ought to be borne in mind that the first impression of the rays as they pass back through the retina will be so much more powerful than the effect of the comparatively feeble portion reflected from the choroid, as to, in a great degree, overpower it, and also that, as everything seen by the eyes of those persons from whom this red light is emitted must be tinged with the same tint, and as the degree of colour must be feeble, it will not be in general perceptible as there will be no objects of the ordinary hue to act as grounds of comparison. Again, another argument against the fitness of the retina to act as the direct recipient of luminous impressions is its transparency, the idea being that the rays of light not expending themselves in its substance but passing through it to the choroid cannot produce the effect of sensation; but this argument seems to be founded upon a narrow view of the nature and propagation of light. It is known from the recent progress of science that the medium which transmits light must be itself capable of vibration, and indeed that the transmission of light is and constitutes a vibration of the particles of the medium. Why then should not the retina be so endowed that, as its particles are thrown into vibration by the passage of the luminous undulation, this of itself should give rise to the sensation of vision, without the necessity of the stifling of the rays as in the opaque substance of the choroid where indeed the motion which constituted this luminous ray is converted into heat and is not fitted to convey any other impression? According to these views no argument has hitherto been brought forward which should induce a doubt as to the retina being the true seat of vision.

It is well known to those familiar with optical instruments that the image formed by a lens becomes more and more indistinct, or as it is called, less defined, as a greater and greater portion of the lens is used, that is, with a given focal distance, as it is made larger and larger. This arises from what is called spherical aberration, the nature of which may be understood as follows. Let O represent one of the points of an object of which an image is to be formed by a lens, then in order that the image may be well defined it is requisite that the whole rays emerging from O and falling on the exposed surface of the lens should be collected into another point such as F; but it is found both by calculation and experiment that with lenses of ordinary construction the portions toward the edge refract the rays too much so as to bend them to meet the central rays at points too near the lens, as at *f*, so that instead of having them collected at F they proceed to a position L on the screen and confuse the image of neighbouring points. This defect can be prevented in three ways—1st, by using other curves than portions of spheres as the surfaces of the lens, but this is difficult of attainment; 2d, giving the lens a less density as it approaches the edges, so that its refracting power will only serve to bring the whole cone of rays to a point at F; or 3d, using only a very small part of the lens by the interposition of a diaphragm before or behind, so as to exclude such rays as are not brought sufficiently near to the focus of the central pencils, which is the method used in optical instruments on account of its simplicity, but it has the disadvantage of causing a dimness of the image by reason of the small amount of light allowed to go toward its formation. In the eye the second mode, viz., that of giving the lens a less density towards its edges, is the one chosen in nature, and it cannot be denied that in practice it is successful; the varying density of the lens from the centre to the circumference may be at once observed by pressing the lens of the eye of one of the lower animals, such, for instance, as the sheep. There is another cause of indistinctness in the image formed by an ordinary lens to which it is necessary here to advert, it is that known by the name of chromatic aberration or dispersion. It depends on the fact that a ray of light when refracted by a lens is spread out into a coloured band instead of giving the image of a point as it ought to do. This may be illustrated by the accompanying figure. At O is seen a ray O I, emerging from the object at O, and passing through the lens undivided, as where there is no refraction there is no dispersion; but another ray, as O S, on entering and leaving the lens is refracted or bent from its

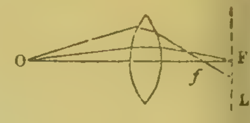


Fig. 5.

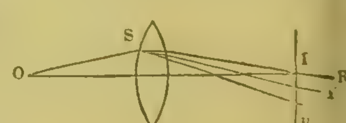


Fig. 6.

course not in one line, not as a single ray as it ought to be, to give a well-defined image by its junction with O I at the focus; but as a multitude of coloured rays, three of which, the red, yellow, and blue, are represented as meeting O I at different points and crossing it so that if a screen be held at I the focus, instead of a point a spot of coloured light, would be seen. This, as may be supposed, would encroach on the position occupied by the images of other points and produce confusion. It may easily be imagined that in general in the central parts of such an image the mixture of different colours from neighbouring points would be so great as to prevent any one from being seen; hence it is only at the edges of the image that such coloured fringes are observed—giving rise to the common mistake that it is only in the outline or border limit that the figures of bodies seen by chromatic aberration are distorted, which is far from being true, as is abundantly evident. There are two ways of remedying such a fault in the image formed by a lens—1st, the employment of only the central parts of the lens where there is little refraction, by the use of a stop or diaphragm, as then there is little refraction and of course little dispersion; or 2d, the employment of a combination of concave and convex glasses of different refractive and dispersive powers, so that the one glass may reverse or undo the dispersion of the other without altogether neutralizing its refraction, so as to leave still the converging power to form an image. Practically this is now to a great extent accomplished in the finest optical instruments, such as telescopes, cameras, &c., which, as has been said in their principles of action, are nearly identical with the organ of vision. Much has been written on the mode by which the eye is rendered achromatic by means of the mutual adaptation of its different humours. It would seem, however, that all such inquiries are rendered useless by the observation that the eye is not achromatic. It has been supposed that this is demonstrated by looking through a fine slit at the light of the sky, when, if the eye were achromatic, the light ought to appear colourless, instead of which a fringe of red and yellow are seen on one side and of blue on the other. It may be remarked, however, that this is not seen if the sight is so arranged that the edges of the slit are seen distinctly, or if the eye is focussed for its precise distance, to use an optical phrase. Whence it would seem that the eye is so constituted that when the place of the distinct image falls exactly on the retina that image is achromatic, but in other circumstances it is not so. Those who adopt the opinion that no arrangement is made in the construction of the eye for preventing chromatic dispersion, say that it was unnecessary, the confusion resulting from it being imperceptible.

On looking at the course of the rays through the pupil, or in examining the image actually formed in the back of the dissected eye, all observers have been struck with the fact that the image is inverted, that is, that the upper part of the actual object occupies the lower part of the image, the right side the left, and so on. And great amazement has been expressed that all objects are not seen by the eye in an inverted position. Many have gone so far as to assert that children and young animals see objects thus turned upside down, and that it is only experience that teaches them the error of the impression.

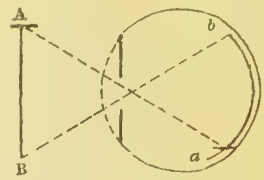


Fig. 7.

The following explanation of this ancient puzzle is perfectly sufficient, and it is therefore unnecessary to notice any of the numerous speculations which have been at different times brought forward. As will be more particularly dwelt on at a further part of this article, every point of the retina acts for itself and sees in its own direction (if the expression may be allowed), and there is no doubt that this is in the direction perpendicular to its surface at the point, so that any point of the retina may be considered for the purpose of explanation as a small eye distinct from the other parts. Consider now in the figure an eye placed at *a* and getting a ray of light from the pupil; of course it would perceive the impression in that direction and would see the object in that direction, or as at A, hence, then, it looks up for the top of the object where of course it is, and so with every other point, acting and perceiving distinctly for itself and independently of the others, and seeing the part of the object in the direction from which its rays come, that is the top of the object in an upward direction. The whole difficulty seems to have arisen from imagining the mind or consciousness as a separate existence contemplating the inverted image on the back of the eye-ball from without, as another eye looks at such an image in the dissected ball formerly referred to; whereas the case is totally different, it is the conscious retina itself perceiving the parts of the image each in the direction of the rays which indicate its presence.

Few persons are aware of the very limited extent of the whole field of vision which can be seen distinctly at the same instant, yet the effort to decipher the one part of a printed line or even a word while the eye is kept fixed to another point in its immediate vicinity will convince them of the fact. It may be said indeed that distinct vision is confined, strictly speaking, to one point of the retina, and that the reason that this is not more generally observed is the rapidity of movement of the eye which passes the point of distinct vision over every part of an object with such facility that no inconvenience is felt at its not being all with equal distinctness seen at once. Different

EYE

causes have been assigned for this want of distinctness in the greater part of the field of vision. On the very centre of the retina, or rather the part exactly opposite the centre of the pupil, or at the termination of the axis of the eye, as it is called, there is a spot subtending an angle of about 4° from the centre of the ball, consisting of a part rather more transparent than the remainder of the nervous membrane and surrounded by a faint yellow margin. It is called the *foramen centrale* or central hole of the retina, and has by some been thought to be the cause of distinct vision. That this is not the case is evident from the fact that it is only, according to Sir D. Brewster, found in man, apes, and some lizards; and, moreover, that distinct vision does not extend over so large a field as 4°, and besides that the area of distinct vision is not circumscribed by any boundary as is the case with this spot. The distinctness of the perceptions gradually diminish from a single point to the margin of the retina, where all sensation is lost, so that distinctness can in no degree be connected with any particular line or margin. It is, however, certainly confined to a particular point of the retina, as no degree of mental effort or practice will serve to change the place of distinct vision from one point of the retina to another. It is known that the images formed by lenses are more faint and confused towards the edges of the field, and that even in them perfect definition can only occur at the centre. In such cases, however, a very much larger amount of the field remains comparatively distinct than in the eye, so that it does not seem to be owing to the obliquity of the incident pencils of light and the inherent imperfection of the image formed on the retina that its margins seem to the sense of vision so ill defined. To put this to the test of experiment we have only to place a very small opening in a card in front of the eye so as to shut off all but the central parts of the lens from action, when if it were on account of the obliquity of the pencils of incident rays, the indistinctness ought to be greatly diminished, which is not the case. So we are forced to the conclusion that it is by a peculiarity in the structure of the retina itself rather than of the image that the field of distinct vision is confined to so small a range. Indeed it is easy, considering that it is so, to find reasons for it in the fact that concentration of attention is thus produced and the mind confined in its observation, at each instant, to a single point.—Closely connected with the foregoing peculiarity of the eye is the question of the period of duration of impression made on the retina. It is more than probable that this impression is an undulating motion similar to that excited in a pool of water by the impact of a stone, so that it might be expected some time would elapse before this wave motion would subside and leave the surface tranquil and ready for another impression. This is accordingly found to be the fact, and is the origin of some interesting peculiarities in the sense of vision as well as the foundation of curious toys and philosophical experiments. The common amusement of children in whirling pieces of burning wood rapidly in the air to give rise to the appearance of ribbons and luminous figures may be given as an instance of an effect depending on the duration of the impression on the retina. Supposing the luminous point at A to have its impression at *a* on the retina in the annexed figure, and that while it moves on to B the image is made on other points as at *b* in the eye, then if while this has occurred the image is still perceptible at *a*, it is evident that the whole of the images along the line which were successively made by the point will be seen at once, as if the original line of motion of the luminous point had been itself luminous.

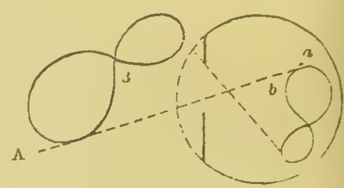


Fig. 8.

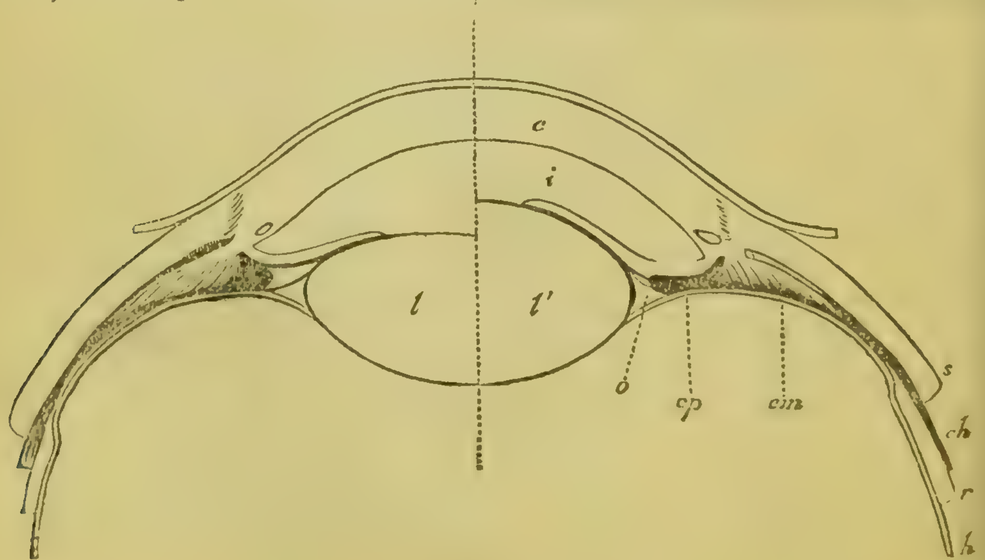
If, then, any figure be completely described by the moving point before the impression has begun to fade at any part of the retina passed over, the figure will appear complete. It is evident that the conditions of this are the rapidity and dimensions of the original movement, its distance from the eye, and the time during which the impression continues on the retina after the light which gave rise to it has vanished.—It is easy to arrive at a knowledge of the duration of the impression by means of the experiment now noticed. Say that such a point is whirled round on a wheel in front of the eye more and more rapidly till it is just seen as a perfect circle, then it is evident that the time of one revolution of the point is precisely equal to the duration of the impression. In this way it is found that impressions of different strengths and colours differ in the period of their duration, as might, from the analogy of the pool of water, be expected. Different experimenters have given the $\frac{1}{4}$, $\frac{1}{2}$, $\frac{1}{3}$d of a second as the average period of duration, and Plateau finds the impression least permanent in blue light and most so in yellow and white, the yellow remaining 35 hundredths of a second while the blue only remained 32 hundredths.—The appearance of forked lightning as a continuous thread of light is no doubt to be ascribed to the peculiarity of the eye now under notice, as also the elongated form of shooting stars, falling rain, &c., &c. It has been said that feeble lights take longer to make their impression than strong ones, and instances have been given of the cannon shot passing across the sky being invisible while the same at a white heat against a dark sky is easily perceptible; but this does not seem to be the true explanation, but rather that the duration of the impression of the luminous sky prevented the perception of the impression of the dark ball,

the retina being in a state of previous agitation which had not time to subside.—The Thaumatrope (Wonder Turner) invented by Dr. Paris is a philosophical toy founded on the duration of the impression on the retina. A flower is painted on one side of a piece of cardboard and a flower-pot on the other, the whole is suspended by a string and a rapid whirling motion communicated, when the two sides will be seen at once and the flower appear to grow from the flower-pot. Plateau's magic disc depends on the same cause. —A circular piece of card has, at intervals of one or two inches round its circumference, slits of $\frac{1}{4}$ inch broad and an inch deep cut out, and in the intervals the same figure painted in different but similar attitudes; for instance, the action of dancing, the different positions which such an action would give rise to in its successive phases round the disc. When a pin is put through the centre of such a card, and the face of it held in front of a mirror, the eye of the observer being directed through the slits at the images in the mirror, while the card revolves, the images in the mirror being in all respects reversed, appear to stand still. And if such rapidity be given to the motion, that the impression of one figure shall come on the eye through a chink just as that of the former is about to vanish, the figure will appear to have moved and taken up the new position, so that all the semblance of a living object may be communicated. Twelve slits answer well for such a disc, and a diameter of eight or nine inches. Other and still most ingenious arrangements of this curious instrument have been described by its inventor.—Of all questions concerning the faculty of vision, the one which has given rise to most discussion and has been most difficult of solution is the mode in which the eye accommodates itself so as to produce distinct vision at different distances. That such a power is required and actually exists, may be perceived by such an experiment as the following. Place the eye at a distance of a foot from the glass of a window, and with a pencil endeavour to delineate on the glass a tracing of the outlines of distant objects seen through the window. It will soon be found that it is impossible to see distinctly the point of the pencil and the outline of the distant object at the same time, so as to cause the one to follow the other. Either can be seen distinctly separately, but not together; the eye must accommodate itself to either distance separately. This is precisely analogous to what occurs in the formation of the image by means of a lens on a screen. It is found that the distance of the lens from the screen must be varied to suit different distances, and that all the images of the distant objects seem confused, when the near ones are distinct, and *vice versa*. What, then, are the means of this adaptation of the eye? In the case of the lens, for instance, of the camera obscura, it is found to be easy to focus all distances at once, by employing none but nearly central rays, that is, by covering up all but the centre of the lens by a diaphragm, leaving only a small opening. In this way, the blades of grass close to the camera, and the scars on the distant mountains, are all depicted at the same time equally distinct. M. Pouillet, from observing this, and also noticing the fact, that the pupil contracts itself when effort is made to see near objects, ascribes the accommodating power of the eye wholly to the contraction and dilatation of the pupil. But that this is wholly untrue, may be at once made out by the simple experiment of looking through a small hole in a card, when it will be obvious that the circumstances are the same as in the contracted pupil, and that though it is true that the eye is now accommodated to very near vision, yet this accommodation differs from that which naturally takes place in this, that both far and near objects are seen distinctly at the same time, which is never the case with the natural pupil.—A very prevalent opinion is that the eye is wholly elongated by the pressure of its external muscles, and that this is equivalent to withdrawing the retina to a greater distance, and thus rendering its situation adapted to the images of near objects. But such an elongation would necessarily lead to a projection of the front part of the eye farther forward. Such a projection has never, even by the most careful observation, been noticed.—It ought to be borne in mind, that in what Dr. Young has called indolent vision, that is, in the state of the eye when its vision is indifferent, or directed to no particular distance, it is always fitted for the most distant vision; so that only one kind of adjustment is required, the other taking place without effort. The celebrated Dr. Thomas Young, who devoted much time to the investigation of the physiology of vision, arrived at the conclusion, that the lens has a muscular structure, and can thereby change its figure to a more spherical form, thus diminishing its focal distance, and that thus the accommodation to near objects is produced. He instituted experiments and calculations, to show that the amount of change necessary would be but trifling in amount, and by no means beyond the bounds of probability: muscles, however, are liable to irregular and spasmodic action, and it is *à priori* questionable how far, where such perfection of form is required as in the lens of the eye, it should be left liable to such changes of shape. Besides, our more accurate knowledge of the minute anatomy of the texture and of the lens itself, affords no support to this view; and it has been still more decidedly disproved by the direct experiments of *Cramer* and others, who found that the crystalline lens of recently-killed animals undergoes no change of form whatever when subjected to the powerful stimulus of a romting battery. There is therefore only this alternative,—the alteration in the form of the lens must depend on the action of the internal muscular apparatus of the eye, which in man consists of

the *iris* and the *ciliary muscle.* The question remains, therefore, what part is played by the *iris* and what by the *ciliary muscle?*—a question exceedingly difficult and delicate, but which has at last been satisfactorily answered. Having carefully studied all the more modern researches of Cramer, Donders, Helmholtz, and Van Reeken, and followed up their experiments by independent researches, Dr. Allen Thomson has exposed and settled the whole intricate subject in one of the most interesting memoirs that we possess concerning it. His conclusions are as follow:—

"(1.) In adjustment for distant vision, which is usually the state of rest of the eye, the flattened form of the lens is maintained principally by the elasticity of the parts, and more immediately by the tension of the elastic fibres of the zonule of Zinn, or suspensory ligament, which pass to unite with the capsule of the lens, both on the anterior and posterior surface, near its margin. (2.) In adjustment for near vision, which is an active condition, the increased curvature and advance of the anterior surface of the lens, together with the other attendant changes in its form, are effected by the combined action of the ciliary muscle and iris, which compress the marginal part of the lens by their own direct action, and through the ciliary processes and fluid of the canal of Petit. (3.) By the contraction of the ciliary muscle, the attachment of this muscle itself, and of the base of the iris, along the line of junction of the cornea with the sclerotic, is drawn somewhat backwards; and thus the outer part of the anterior aqueous chamber recedes, and is widened transversely. The pupillary margin of the iris, though contracted by its sphincter muscle, is advanced by the bulging of the lens, upon which it lies closely applied. (4.) It may be doubtful whether the radiating fibres of the iris contract actively, or are only in a state of tension between the sphincter and the ciliary muscle; but, in either case, the outer part of the iris will press upon the margin of the lens in front. (5.) The external and longest fibres of the ciliary muscle, whose contraction, as Van Reeken remarks, must be the greatest, proceeding towards the ora serrata, draw forward that part of the choroid membrane, and thus have the effect of relieving the tension of the elastic fibres of the suspensory ligament of the lens. (6.) The remaining part of this muscle, inserted into the exterior of the ciliary processes and suspensory ligament, must gather them together, rendering them shorter from without inwards, and narrowing them from before backwards, so as to support the vitreous humour behind the lens, and thus prevent its moving backwards. The yielding nature of the attachment of the iris and ciliary muscle to the line of junction of the cornea and sclerotic, may allow the ciliary muscle also to act on the membrane of Descemet, so as to give increased tension and support to the fluid in the aqueous chamber. (7.) The combined action of the inner fibres of the ciliary muscle and of the iris, is to carry the ends of the ciliary processes against or near to the margin of the lens, and to produce compression round that margin; the effect of which pressure may be rendered more equal by its action through the fluid of the canal of Petit. This canal is deepened during the action from before backwards, and its fluid may thus convey the pressure to a greater extent of the margin of the lens. At the same time, the direct pressure of the outer part of the iris on the marginal part of the anterior surface of the lens prevents the advance of that body. The lens therefore bulges, especially at its central portion, and it seems even possible that it may bulge to a greater degree through the pupil than elsewhere."

The subject is made much more intelligible by the following diagram, modified by Dr. Thomson from one given by Helmholtz. On the right side of the diagram the lens and other parts are represented as in adjustment for near vision;



on the left side, for distant vision:—*c*, the cornea; *i*, the iris, *s*, sclerotic; *ch*, choroid membrane; *r*, retina; *h*, hyaloid membrane; *cm*, ciliary muscle; *cp*, ciliary processes; *o*, canal of Petit and suspensory ligament of the lens; *l*, the flat lens; *l′*, the lens altered in form for near vision.

Eye-Piece.—The function of the OBJECT-GLASS of a telescope is simply this,—it presents in its focus an image of the external object which is much *more luminous* than the object appears to the unassisted eye. The *size* of the image varies with circumstances, but it may be magnified by aid of a microscope, in proportion to its brightness. This magnifier or microscope is the small tube through which one looks at the image, and is technically termed the *eye-piece* of the telescope. We shall briefly describe, in the first place, the nature of the eye-pieces generally in use, and then refer to the important subject of their correction for *Chromatic* and *Spherical Aberrations*.

I. THE GENERAL STRUCTURE OF EYE-PIECES.—Eye-pieces may be divided into three classes:—

1. *The Single Lens.*—This is a mere convex magnifying glass, so placed that the image be in its focus. It is still frequently employed when a limited object, or part of an object, requires to be viewed with a very high power. But the object must be limited, inasmuch as it cannot be seen distinctly unless in the centre of the field of view or the immediate neighbourhood of that centre. At any appreciable distance from the centre of the lens, the image is coloured and disturbed through effect of the two aberrations. The chromatism of such a lens may indeed be destroyed in the way that an object-glass is corrected for the same defect; but this can be effected much more easily and certainly when a compound eye-piece is employed. The single lens, therefore, is not now put in use unless in the circumstances above described.

2. *The Astronomical Eye-piece.*—The image of an object formed in the focus of an object-glass is *inverted*, as will appear from the following diagram:

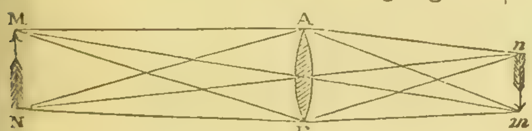


Fig. 1.

The application of a simple magnifying power to that image will, of course, present it inverted also, and enlarged in size. Now, as it is not of the slightest importance in astronomical observations, whether the object appear inverted or upright, the function of all eye-pieces employed in such researches is merely that of magnifying the image; and they show accordingly the object inverted. As we shall see in next section, the image could not be re-inverted, or the object presented in its natural position, without the interposition of a new and special lens; and no new lens can be interposed unless at the costly sacrifice of a quantity of light.—Two varieties of such eye-pieces are in common use—one called the *negative*, the other the *positive* eye-piece. The negative eye-piece is formed of two plano-convex lenses, A and B, fixed with their curved surfaces towards the object-glass, at a distance from each other something less than half the sum of their focal lengths. It is called a negative eye-piece, because the image viewed by the eye is formed behind the inner lens, and this is the form generally used when distinct vision is the sole object.—The *positive* eye-piece is formed of two plano-convex lenses, C and D, having their curved surfaces turned towards each other, and placed at a distance from each other less than the focal distance of the one next the eye, so that the image of the object viewed is beyond both the lenses: this is the form adopted for the Transit Instrument, where spider lines are in the focus of the object-glass, and also for telescopes with micrometers, for the piece containing the lenses can be taken out without disturbing the lines, and is adjustable for distinct vision.

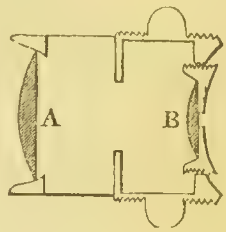


Fig. 2.

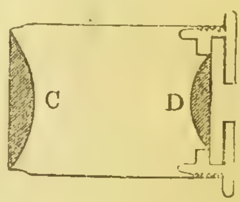


Fig. 3.

As the image formed at the focus of the object-glass lies parallel to the flat face of the contiguous lens, every part of the field of view is distinct at the same adjustment, or, as opticians say, there is a *flat field.*—To obviate the inconvenience of observation at high altitudes, a diagonal piece is often used. A flat piece of polished speculum metal, F, applied beween the two lenses at an angle of 45°, changes the direction of the rays of light, and forms an image that becomes *erect* with respect to *altitude*, but is still *reversed* with respect to *azimuth*. Instead of the speculum, a totally reflecting prism may be beneficially employed. The most elementary acquaintance with dioptrics will enable the reader to trace the course of the ray of light through these different combinations. All varieties of magnifying power, as depending on the curvatures of the lenses, may, of course, be given to either construction.

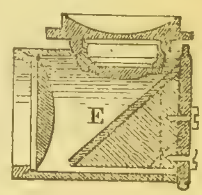


Fig. 4.

3. *The Erecting or Terrestrial Eye-piece.*—It would unquestionably be surprising, and altogether unexpected, that one should have remote *terrestrial* objects presented to the eye, turned upside down; and as the sacrifice of an amount of light is of less consequence in this case, the eye-

piece of an ordinary terrestrial telescope has more lenses than two. The manner in which the object aimed at may be accomplished by the introduction of such additional parts will be readily understood. Suppose that an additional lens of the same nature as A B (see fig. 1, above), were placed beyond the image *n m*, and so that *n m* should be in its focus, it is clear that an image of *n m* would be formed in the focus of the new lens, and that this second image would be an inversion of the first image,—in other words, the position of its parts would correspond with the position of the parts of the object M N, that is to say, it would be *erect*. The terrestrial eye-piece could therefore be completed merely by bringing the magnifying power, or some form of the simple astronomical eye-piece, to operate on that second or erect image. This is not indeed the actual arrangement of any terrestrial eye-piece in use; but it explains the principle of all of them. They are composed sometimes of three lenses, sometimes of four, variously arranged; but one part of the arrangement always has for its object or distinctive function to reverse the original inverted image, and the other to magnify it. The mode of the arrangement of the lenses is determined by collateral considerations,—mainly, by the conditions most favourable for the destruction of the two aberrations. Two of the three arrangements are shown below in figs. 7 and 8, where the places of re-inversion are sufficiently clearly indicated. Of other arrangements the following may be specified as very generally used. Although the subject properly belongs to next section, it may be remarked here that this eye-piece will be achromatic under the following conditions:—*First*, The four lenses, A, C, D, B, reckoning from A, ought to have focal lengths in the proportions of 3, 4, 4, and 3; and the distances between them, reckoning in the same scale, should be 4, 6, 5, and 2. *Secondly*, The radii, reckoning from the outer surface of A, should be,—

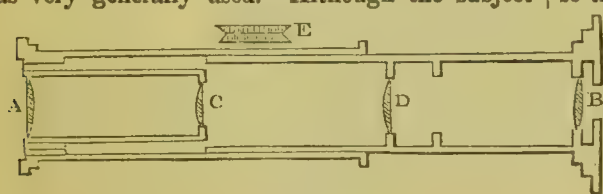


Fig. 5.

A	First surface,......	27	Nearly plano-convex.
	Second surface,....	1	
C	First surface,......	9	A meniscus.
	Second surface,....	4	
D	First surface,......	1	Nearly plano-convex.
	Second surface,....	21	
B	First surface,......	1	Double convex.
	Second surface,....	24	

Sir David Brewster was the first to notice that, by varying the distances between C and D, by means of the moveable tube containing A and C, the magnifying power of this eye-piece may be increased or diminished. This is the secret of Kitchener's *Pancratic* eye-piece. For principles of computation, &c., as to these matters, see LENS.

II. PRINCIPLES OF CORRECTION FOR THE ABERRATIONS.—These aberrations are two—the *Chromatic* aberration, and that of *Sphericity*.

1. *Correction of Chromatic Aberration.*—This most essential correction has occupied the attention of our best physicists and mathematicians, such as Euler, Clairaut, D'Alembert, Boscovich; and in modern times it has elicited elaborate memoirs from Airy and Biot. The researches of Biot are in the *Memoirs de l'Institut* for 1843, and more recently his invaluable treatise, the *Astronomie Physique*. Mr. Airy's work is inserted in the *Cambridge Philosophical Transactions*, vol. ii., bearing date 1824. We shall give a brief account of the latter paper, distinguished alike by its simplicity and exhaustiveness. The analytical process employed is susceptible of a short and popular explanation. Chromatic aberration, as explained under ABERRATION, arises from the unequal refrangibility of the diverse coloured rays or waves of the spectrum. The general principle is this:—The violet pencil is more refrangible than the red, and will therefore enter the eye, making a greater angle with the axis of the telescope than the red pencil makes. Looking from the centre, then, or along the axis, at the refracted sheaf, the violet image will appear as a ring enveloping the inner red ring and all the intermediate ones, so that the object will present as a set of coloured rings or forms. Now, if a second eye-glass be placed at some distance from the first, the violet rays, after refraction at the first eye-glass, will be incident on the second, at a point nearer to the centre, than that at which the red rays are incident on it; and falling therefore on a smaller repeating angle, they may, by the proper adjustment of the lenses, be made to issue parallel to the red rays; when the object will be seen without any tinge of colour. It is singular enough that the early double eye-piece of Huyghens—although originating in other considerations—answered this requisition in the way indicated in diagram, fig. 6: *see below*.

It is clear, however, that an adjustment so important should not be left to chance, or continue surrounded by any degree of vagueness. Calling n the index of refraction for any one colour, or for the mean rays, $n + \delta n$ will be the index of refraction for rays of any other colour. If, then, by aid of this notation, an expression is found for the visual angle, by tracing the axis of a pencil of rays through the eye-piece, the *variation* of this expression depending on the foregoing alteration of the index of refraction, must, in the case of achromatism, be evidently $= 0$. Fundamental equations are thus easily formed, and the relations among the required quantities that are demanded by the condition just

laid down can be determined at once. We have no room to give more than a few of Mr. Airy's results.—(1.) In case of the eye-piece with two eye-glasses. Let D, in fig. 6, be the distance of the first eye-glass from the object-glass, p its focal length, a the distance of the second eye-

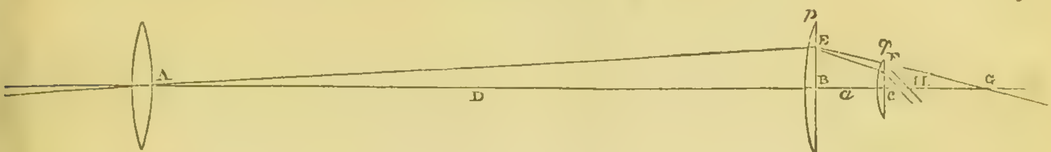


Fig. 6.

glass from the first, q its focal length; then the equation yielded by the foregoing method is—

$$a = \frac{p + q}{2 - \frac{p}{D}}$$

or if, as actually occurs, D be very great,

$$a = \frac{p + q}{2}$$

If $p = 3\ q$, then $a = 2\ q$, which is the Huyghenian construction —(2.) The eye-piece, with three glasses, as in fig. 7.

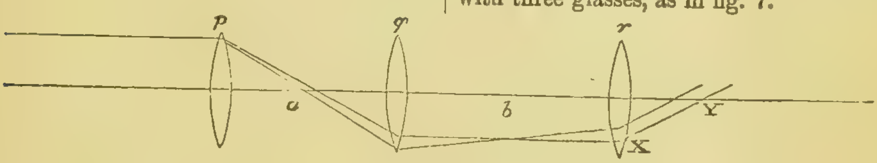


Fig. 7.

The final equation is—

$$3ab - 2bp - 2(a+b).q - 2ar + pq + qr = 0.$$

If a, p, q, and r be given quantities, we have

$$b = \frac{2aq + 2ar - pq - pr - qr}{3a - 2p - 2q}$$

If $a = p + q$ · we obtain—

$$b = q + r + \frac{q^2}{p + q}$$

A formula of Boscovich's. This eye-piece is little used, in consequence of the difficulty of getting rid of spherical aberrations.—(3.) The eye-piece with four eye-glasses is now universally employed. See fig. 8.

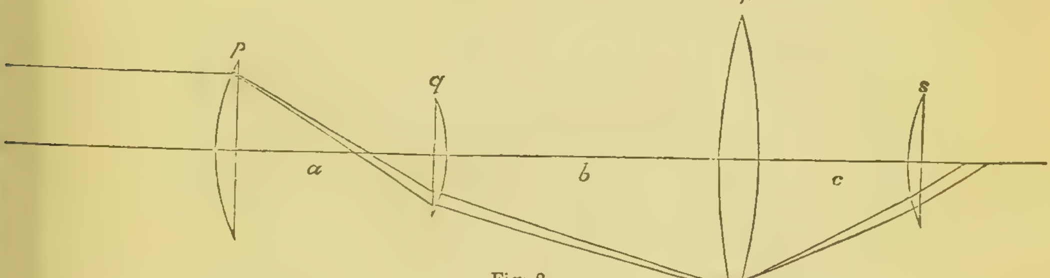


Fig. 8.

The final equation, supposing a, b, p, q, r, s to be given, is

$$c = \frac{\left\{b\cdot(a + 2q) - (2a - p)\cdot(2b - q)\right\}\cdot(r + s) + rs\left\{2a - (p + q)\right\}}{p\cdot\left\{3b - 2(q + r)\right\} + q\cdot\left\{3\cdot(a + b) - 2r\right\} - a(4b - 3r)}$$

In four telescopes examined by Airy, the values of c were as follows:—

1. A common perspective glass, $c = 1·8$
2. Do. do. $c = 40·$
3. One of Dolland's, ... $c = 21·45$
4. One of Ramsden's, ... $c = 1·1$

The true value of c in the different cases ought to have been—

2·31; 37·88; 19·37; 1·12.

The student is further referred to Mr. Airy's *Memoirs*, and to the writings of Biot, already quoted. See also an instructive chapter in Sir David Brewster's *Optics*.

2. *Correction of the Aberration of Sphericity.*—The nature of this aberration has been explained under ABERRATION. It consists in this—all parts or zones of a spherical lens have not the same focus; so that, except in the immediate vicinity of the centre of the spherical lens, the image of an object is disturbed and indefinite. The practical correction of this aberration can never be perfect; and taken in connection with the necessity of

achromatism, the whole subject is an intricate one. Mr. Airy has pursued its theory also through its most important relations, in an exhaustive Memoir in *Cambridge Philosophical Transactions*, vol. iii. Referring the student and scientific optician to that very able memoir, we must be satisfied here with a few practical remarks. (1.) Inasmuch as it is exceedingly difficult to correct for spherical aberration, the first point to be determined is in any given case, *What are the special errors desired to be guarded against?* Airy classes these as follows:—*First*, A deformation of the object. If, after examining an object in the centre of the field of view, we bring it to the outside, we shall frequently find that it is extended in the direction of the radius of the field of view, and that it is increased, though in a smaller degree, in the other directions. If we look at a square, it will appear to be drawn out at the corners, so that the sides are all convex towards the centre. Sometimes the contrary effects will be produced: an object being less magnified at the circumference of the field than near the centre. This defect may, in many cases, be entirely removed. *Second*, If an object be distinctly visible in the centre of the field of view, it is necessary to push in the eye-piece farther in order to see clearly the objects at the outside of the field. In consequence of this it is impossible, with the same position of the eye-piece, to see distinctly all parts of the field, or to see an object distinctly as it passes the field of view. This defect can never be destroyed. *Third*, That no adjustment of the place of the eye-piece will make an object distinctly visible when it is far from the centre of the field of view. If a brilliant point, as a star, be viewed in this situation, with one position of the eye-piece, it appears a bright line in a direction perpendicular to the former; with other positions it appears an ellipse, or a circle. In the case last mentioned, different parts of the image are formed at different distances from the eye: in which case, no distinct image is formed at all, except in the centre of the field; the rays of other pencils never converging accurately to a point. This defect may frequently be entirely corrected, though it is sometimes prudent to leave it partially uncorrected.—As already said, it is necessary in every practical case to consider *on what defect the greatest stress should be laid*. For instance, when the aperture of the telescope is very small, or its magnifying power very great, the breadth of the pencil is very small, and the *second* and *third* defects become insensible; so that the first condition ought chiefly to be attended to. In cases of micromatic measurement, the power is rarely great, and distortion is of less consequence, inasmuch as *object* and *micrometer-wires* are equally distorted. The *second* condition is here, therefore, the important one; and the *third* should, as far as possible, be satisfied also. Mr. Airy's Memoir, which, as we have said, is almost exhaustive, proceeds, of course, on the principle that the spherical aberration of one lens must be used to correct the spherical aberration of another: and the applicability of this principle is evident enough,—it being quite the same as the principle employed respecting chromatic aberration; viz., the ray converging to a point nearer than that to which, without spherical aberration, it would converge, may be made incident on the second lens at a point where the separating angle is so small that it shall emerge in a direction parallel to that which it would have had if there had been no aberration. All the important and applicable combinations of lenses are separately and elaborately considered in Mr. Airy's Memoir. —See OBJECT-GLASS.

Eye-piece, Solar, or Mr. Dawes's.—An eye-piece, recently proposed by the Rev. R. Dawes, and executed by Mr. Dolland, is eminently successful in cutting off the sun's light and heat, in so large a proportion that an observer may view the spots without sensible inconvenience, however large the aperture of the telescope. The result is attained by the insertion of a diaphragm into the tube, and a ring of ivory. The defect of this valuable eye-piece is the contractedness of its field of view. But several observers have already used it with success in reference to the solar spots. Minute descriptions of this eye-piece may be found in the *Monthly Notices* of the Royal Astronomical Society.

F

Fahrenheit. See THERMOMETER, SCALES.

Falling Stars. See AEROLITES and METEORS.

Fata Morgana. A phenomenon chiefly noticed in the Straits of Messina, between Calabria and Sicily. It is described as showing the images of objects on the coast, sometimes in the water, sometimes in the air, sometimes at the line which separates the two media. Sometimes two images of an object are shown, one inverted, and one in the natural position, and sometimes a great number of images are observable. The phenomenon has not yet been perfectly accounted for. Philosophical observers of it have not been numerous; and due records of the meteorological circumstances in which it takes place, and by which it is certainly very much modified in character, have not been preserved. The peculiarities of the coast seem to offer an explanation in so far satisfactory. These coasts enclose a portion of nearly stagnant air, susceptible of rapid alternations of temperature. In ordinary circumstances air is not so, chiefly because the heated or cooled air is immediately replaced by other

air, by *Conviction (q. v.)* Here, however, a series of atmospheric strata are formed, of different densities, which may, and probably do, act as so many mirrors hung in the air, from which reflections are produced. The series of images may be explained by the number of these mirrors; and the coloured haze which frequently surrounds them, may be attributed to the dispersive power of the globules of watery vapour, on the heterogeneous rays of which white light is formed. See MIRAGE.

Fits of Reflexion and Transmission. In the Theory of Light, which makes it to consist of solid particles, emitted from luminous bodies, it was necessary to explain the fact of one portion of the light incident on any medium being reflected from, and another being transmitted through it. Newton's explanation assumed a mutual action between the matter of light and that of ordinary bodies—an action alternately *attractive* and *repulsive*, numerous alternations of attraction and repulsion within distances where this action was sensible, and an equality in the spaces through which these different fits continued uniform. The application of this hypothesis to the phenomenon of thin plates (see PLATES), by which it was first suggested, is obvious. In passing through the first distance, the particle of light is in a fit of easy reflexion—proceeding through a distance twice as great, it reaches the second surface in one of easy transmission; and so on through successive stages; according as they are even or odd multiples of the length of a fit, is the molecule at incidence on the surface in transmission or reflexion, and the resulting rings appear. It is impossible, on *a priori* grounds, to refute a hypothesis quite consistent with the known laws of molecular action. Minuter investigations, however, into the laws of thin plates necessitated the adoption of so many supplementary hypotheses, that it broke down beneath their weight. The explanations this theory affords of ordinary reflexion and refraction, as long as they remain perfectly vague, may meet with a certain acceptance, but they will not stand the application of any quantitative test.

Flame. The nature of flame has recently been successfully investigated by Professor J. W. Draper of New York. In 1848, this eminent physicist established an interesting relation between the colour of a flame and the energy of the combustion giving rise to it. The more vigorous the combustion, the higher the refrangibility of light: for instance, a flame burning in its most tardy way emits *red;* but if burning in the most effective manner possible, the rays are violet. Resuming the inquiry quite recently, Draper has analyzed the whole phenomenon. According to these researches, the flame of a candle or lamp consists of a series of concentric luminous shells, surrounding a central dark core. These shells shine with different colours, the innermost one immediately in contact with the dark core being red, and having a temperature of 977° F. Upon this, in their proper order of refrangibility, are shells, the light of which is orange, yellow, green, blue, indigo, violet. When we look upon such a flame, the rays issuing from all the coloured strata are received by the eye at once, and impress us with the sensation of white light. The differently coloured shells, of which a flame thus consists, may be easily parted out from one another, and demonstrated by a prism. Their cause is the slower rate at which combustion occurs at points more and more towards the interior. On the outside, which we may say is in contact with the air, the combustion is most vigorous and complete, and hence the light there emitted is violet; but in the most interior portion of the shining shell, resting upon the dark combustible matter, the atmospheric air can hardly penetrate, or, rather, its oxygen is exhausted and consumed. Between the exterior and interior surface, the burning is going on with an activity constantly declining, because the interpenetration or supply of oxygen is gradually less and less. But, besides this collection of coloured shells, constituting what may be termed the actual flame, there is another region exterior thereto, and to be distinguished both in its chemical nature and in its optical relations. Chemically, it consists of the products of combustion and of the unburnt residue of the air, that is to say, carbonic acid, steam, and nitrogen. These are all the time escaping out of the true flame, and envelop it as an exterior cone or cloak. Optically, this portion differs from the true flame in the circumstance, that it is shining as an incandescent, ignited, but not a burning body. For physiological reasons, the tint of this exterior cloak seems to be a monochromatic yellow. That, however, is, to a considerable degree, a deception; prismatic examination proving that all the other colours are present, and that the yellow merely exceeds the rest in force and intensity. A flame thus far may be considered as offering three regions:—*First*, A central nucleus which is not luminous, and consists of combustible vapour. *Secondly*, An intermediate portion, the true flame, arising from the reaction of the air and the combustible vapour, and being composed of a succession of superposed shells, the interior being red, the exterior violet, and the intervening ones coloured in the proper order of refrangibility; the cause of this difference of colour being the declining activity with which the combustion goes on deeper and deeper in the flame. As to temperature, the inner red shell cannot be less than 977° F., and the exterior violet one probably more than 2500° F. *Thirdly*, An envelope consisting of the products of combustion, exterior to the true flame, shining simply as an incandescent body, and its light for the most part overpowered by the brighter portion within. By the aid of the facts thus presented, we can easily explain the nature of the other regional

divisions, distinguishable in such a flame. There must be a blue portion below; blue, because it consists of the most refrangible rays, which issue forth in abundance, for there the exterior air is most copiously and perfectly applied. At the upper end of the flame, particularly if the wick be long, and the supply of combustible matter abundant, the light emitted is red; for the products of combustion ascending past that part, make it difficult for the exterior air to get access. —Upon these principles we may also predict what colour a flame will have when we vary the circumstances of its burning. Tallow or wax at temperatures greatly beneath their usually understood point of combustion, oxidize with a pale violet phosphorescent light, quite perceptible, nevertheless, in a dark room, and here the light is violet, for the supply of combustible matter is small, and that of the air abundant. The oxidation is therefore thorough and prompt. For a like reason, sulphur, as we commonly see, burns blue; but if a piece of it is thrown into nitrate of potash ignited in a crucible, the light yielded is of intolerable brilliancy, and absolutely white. Its whiteness does not depend upon the physiological fact, that any colour, if it be intensely brilliant, will seem white to the eye; but it is optically white, as is proved by prismatic examination, when all the colours are perceptible. And the reason of this is, that at the high temperature to which the sulphur is exposed, it volatilizes faster than the nitrate of potash and air together can oxidize it, and offers every intermediate rate of combustion, and emits rays of every refrangibility. —In like manner it may be shown that carbonic oxide must burn with a blue flame, and cynanogen with a red. We can also foresee what must be the optical result of resorting to unusual methods of combustion, as when we throw into the interior of a flame a jet of air from a blowpipe. In this case we destroy the red and orange strata, replacing them by bluer colours. Examining such a blowpipe cone by the prism, we have a beautiful demonstration that such has actually taken place. There is one of these special cases which deserves attentive consideration in connection with the appearance of the electric light; it is the production of Frauenhoferian lines, when things have been arranged in such a way that an incombustible material is present in the substance to be burnt. This state is perfectly represented in the case of cyanogen, which contains more than half its weight of incombustible nitrogen. When the peach coloured nucleus of the cyanogen flame is properly examined, it yields a series of dark lines and spaces, exceeding in number and strength those of the sun-light itself. These fixed lines are the representatives of dark shells, superposed among the shining ones with definite periodicity. In such a cyanogen flame they bear no relation to the burning of the carbon, but must be attributed to the disengagement of the nitrogen.—In other cases dark lines are replaced by bright ones, as in the well known instance of the electric spark between metallic surfaces. The occurrence of lines, whether bright or dark, is hence connected with the chemical nature of the substance producing the flame. For this reason they merit a much more critical examination than has yet been given them; for by their aid we may be able to ascertain points of great interest in other departments of science. Thus, if we are ever able to acquire certain knowledge respecting the physical state of the sun and other stars, it will be by an examination of the light they emit. Even at present, by the aid of the few facts before us, we can see our way pretty clearly to certain conclusions respecting the sun. For, since substances which are incandescent, or in the ignited state through the accumulation of heat in them, show no fixed lines, their prismatic spectrum being uninterrupted from end to end, it would appear to follow that the luminous condition of our sun, whose light contains fixed lines, cannot be referred to such incandescence or ignition. At various times those who have studied this subject have offered different hypotheses: one regarding the sun as a solid or perhaps liquid mass in a condition of ignition; another considering the light to be electrical; a third supposing it to be the seat of a fierce combustion. Of such hypotheses there is sound reason for declining the first. Prismatic analysis, which demonstrates no resemblance between the light of the sun and that of any form of electric discharges with which we are familiar, enables us in like manner to reject the second; and, upon the whole, facts seem most strongly to prepossess us in favour of the third, in artificial combustions similar fixed lines being observed. If such is to be regarded as the physical condition of the sun, we can no longer contemplate it as an immense mass, slowly and tranquilly cooling in the lapse of countless centuries by radiation into space, as so many considerations drawn from other branches of science have hitherto led us to suppose; but it must be regarded as the seat of chemical changes going on upon a prodigious scale, and with inconceivable energy. If the law designated above, that the more energetically the chemical action in combustion the more refrangible the emitted light, be translated into the conceptions of the undulatory theory, it not only puts us in possession of a distinct idea of the manner in which the combustive union of the bodies is accomplished, the quickness of vibration increasing with the chemical energy, but it also enables us to transfer for the use of chemistry some of the most interesting numerical determinations of optics.

Flexion: Flexure. A subject of utmost practical importance, and which, alike from the point of view of Theory and Experiment, has recently engaged the attention of our best Physicists and Engineers. The Physical fact contained in the phenomenon of Flexion is the fol-

lowing:—If a beam, of any material or form, is loaded, the beam bends, and the two surfaces take on opposite curvatures, the one convex, the other concave; the particles of the solid, at the convex portion, have, through effect of this flexure, been in so far separated from each other; the particles at the concave surface, on the other hand, have been forced nearer each other; and between the two surfaces, there is a line of no disturbance,—which line passes through the centre of gravity of all transverse sections of the beam. The mathematical theory of Flexion starts from the basis, or datum of this *Line* of *No-disturbance.*—We owe the foundation of the experimental portion of the subject to M. Charles Dupin, who early established the following laws. All other things being equal, the flexion of solid beams placed on two supports, and loaded at their middle point, are, (1.) proportional to the loads they support; (2.) in the inverse ratio of the product of their *breadths*, and the cube of their *depths;* and (3.) proportional to the cube of the distance between the two supports;—to which be added another law, viz.: that the flexure produced by a load uniformly distributed, is 5-8ths of that due to the same load placed in the middle of the beam. These laws may be conveniently expressed in formulæ. Let us take as an example two different prismatic pieces of the same wood, placed on two supports, and loaded in the middle, and let—

(1.) f, a, b, 2P, and $2c$, represent the flexure, the breadth, the thickness or depth, the load, and the distance between the supports in case of the first piece;

(2.) f', a', b', 2P$'$, and $2c'$, similar quantities for the second piece;

(3.) $f_{,}$ the flexion of a piece whose section and distance between the supports, is the *same* as that of the *first* beam, while the load is 2 P$'$; and

(4.) f_2 the flexion of a piece, where the load is 2P$'$, the distance between the supports $2c$, the breadth a' and the thickness b';—then we shall have

$$f : f_{,} = \text{P} : \text{P}'.$$

$$f_{,} : f_2 = \frac{1}{a\,b^3} : \frac{1}{a'\,b'^3}$$

$$f_2 : f' = c^3 : c'^3$$

whence

$$f : f' = \frac{\text{P}\,c^3}{a\,b^3} : \frac{\text{P}'\,c'^3}{a'\,b'^3}$$

and

$$f = f' \cdot \frac{a'\,b'^3}{\text{P}'\,c'^3} \cdot \frac{\text{P}\,c^3}{a\,b^3}.$$

These propositions, however, contain only the first elements of a subject which is now as extensive as important. Questions immediately arise concerning the amount of strain or load which produce fracture; the point or section of Fracture—called the *dangerous section;* the dependence of Flexure and Fracture on the nature of the material, and, above all, their dependence on the *form* of the *beam.* The student who desires a satisfactory acquaintance with the whole subject, is referred to an excellent and instructive volume by General Morin, entitled *Resistance de Materiaux;* and to the vast and varied experimental researches of Barlow, Hodgkinson, Willis, and Fairbairn. The investigations of Mr. Fairbairn especially, which led to the construction of the immense tubular bridge at Menai, will ever be accounted classical in reference to one department of this inquiry, and may be said to have effected a Revolution in Bridge Engineering. See *Transactions of British Association, passim;* and ELASTICITY and HEAT.

Flexure, Contrary. A curve is said to have a point of contrary flexure, at the point where it changes its character of concavity or convexity towards any given line without. This point occurs where $\frac{d^2\,y}{d\,x^2}$ (the differential of the tangent of inclination of curve to axis) changes its sign. It may do so through either 0 or $\propto$. Hence the roots of the two equations

$$\frac{d^2\,y}{d\,x^2} = 0, \text{ and } \frac{1}{\frac{d^2\,y}{d\,x^2}} = 0,$$

are to be examined for values which will give such a change, and these values, when found, will correspond to a point of contrary flexure.

Floater. A contrivance indicating the height of level of a fluid in a vessel, whose depth we cannot at the time directly examine. A body floating in the fluid bears an index, whose position may be read off, on a scale reaching from the bottom of the vessel. There are various contrivances for effecting this. These indications are often of extreme importance, as in the case of the boilers of steam engines.

Flora. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Fluid. A term applied to those bodies whose particles possess perfect mobility among themselves, so that any force applied to the body, and not resisted, will produce a change of shape. There are certain bodies which are called semi-fluids, where a very little force applied will produce such a change. In the fluid proper, the uniting and separating forces are considered to be quite balanced. See LIQUIDS and VAPOURS. See also ELASTICITY, and HEAT, sec. 12.

Fluxions (*technical*). When the differential calculus was introduced, it was under this more general form,—suppose that a quantity x, increases uniformly, required the corresponding increments of $f(x)$. In this general form it was found difficult of solution, and not so valuable. And so the after question arose:—Required the ratio of the increments of $f(x)$ and x, when the

increment of the latter becomes indefinitely small. The form in which the problem was originally stated involved the notion of velocity. The small increment of x was marked thus $\dot{x}$, and [calling $f(x) = y$] the corresponding increment of y was $\dot{y}$. Newton called x and y *flowing quantities* or *fluents*,—from *his* original connection of the general notion of increments with that of velocity. He called $\dot{x}$ and $\dot{y}$, inversely, the *fluxions* of those quantities. See CALCULUS.

Fly Wheel. In employing the power of nature to supply the wants of man, we frequently find ourselves in possession of a power which acts *unevenly* when our wants can only be completely satisfied by one acting *evenly*. Sometimes, again, our power may be quite constant, but the work to be done by it varies from point to point, so that the same requirement recurs. The *fly wheel* is an instrument for the equalization of the power and resistance, and the consequent economy of the power, along with the needed regularity of the machine. It consists simply of a large wheel attached to the machinery and moving with it. This makes the machinery slower in moving at first, because more work has to be given by the power to the machine, to overcome the total resistance with so great an added weight. It will also make it slower in stopping, because the machinery, once set in motion, tends to continue in motion with so much the more violence, the more work has been given to it. Now, during motion, when a cessation of power for a moment occurs, this allows the machine to move on nearly the same. The difference of the presence or absence of the power for a moment is something; but then it has to be subdivided and distributed over the whole heavy machinery, and so produces very little evident difference of motion. When, again, a greater force than usual acts for a moment, the effect is something of the same sort. There is an increase, but a very slight and scarce perceptible one, of speed. It is evident also, that the increase or diminution of the resistance to motion, due to the effect we wish produced, will —just as these increases or diminutions of acting force—be absorbed in the fly wheel; and as these are almost always in actual cases cyclical, the fly wheel enables us to equalize the power and the resistance. Its power to do so is proportional to the square of its diameter.

Focus. For an explanation of the properties of the FOCUS of a LENS or MIRROR, see those articles, and those on DIOPTRICS and CATOPTRICS. The *principal focus* is the point to which rays, falling parallel on the surface very near the centre of the lens or mirror, actually or virtually converge. The *focus* of a lens or mirror upon which a certain series of rays are incident, is similar, that to which they actually or virtually converge.—The foci of an *ellipse* are those two points at which the string describing the ellipse may be supposed fastened. Any two lines drawn from them to a point in the circumference, have their sums constant. In an *hyperbola*, the difference of two lines drawn from the foci to any point in it, is constant. In a *parabola*, the focus is such a point that, a line drawn from it to any point in the curve, is equal to a perpendicular drawn from that point on a fixed line, called the *directrix*. The connection between the geometrical and optical senses of the word focus, will be readily seen by reference to the articles on CATOPTRICS and DIOPTRICS.

Fogs, are of two kinds,—the *first*, consisting of precipitation of *aqueous vapour* near the surface of the Earth, in sufficient amount to disturb the transparency of the Air; and *secondly*, those apparently anomalous phenomena, named *Dry Fogs*.

(I) It has been stated under article CLOUD, that between *Clouds* and *Aqueous Fogs*, there is only a circumstantial difference;—the *Cloud* is a *Fog*, in an upper stratum of the Atmosphere, and the *Fog* is a *Cloud*, creeping along the surface of the Earth or resting there. The interest belonging to this curious *Hydrometeor*, mainly attaches itself at present to three points, as follows:—1. In regard of its *constitution*, every Fog is composed of small opaque bodies which turn out minute aqueous spherules. Many of these spherules are probably microscopic *drops;* but for the most part they are *hollow vesicles* like soap-bubbles. This alone explains why a pure rainbow is never observed on a cloud, although *Cloud*, *Sun*, and *Spectator*, are in the relative positions most favourable to the development of the phenomenon;—were these spherules, in the main, drops of water, a rainbow, under the fitting circumstances, must always be formed. Compare RAINBOW. But the fact in question can be established more directly in several ways. For instance, Kratzenstein has settled the point by positive observation. Every one who has watched a soap-bubble, must have seen these beautiful coloured rays that develop themselves at its highest point, as the aqueous vesicle grows gradually thinner. See PLATES. Now, on looking at the vesicles rising from hot water, in the Sun, through a magnifying glass, this Physicist detected, *that very System of Rings;* not only convincing himself thereby that the structures are analogous to those of soap-bubbles, but obtaining elements from which, as a basis, he could deduce the thickness of the evanescent envelope.—As already mentioned (see CLOUD), these vesicles vary in diameter according to the season of the year; attaining their greatest magnitude in our northern climates, in December and February, and reaching a minimum in August.—2. The *circumstances* or *conditions*, under which Fogs form, are very simple:—they are the reverse of those that give rise to DEW. The latter is a precipitation from the comparatively *warm air*, on the surface of the *cold ground;*—in the case of the former, the surface from which the vapour rises is *warmer* than the air. Imagine an extent of aqueous surface, or of moist marshy ground to be, from

whatever cause, warmer than the superincumbent air. Evaporation will necessarily proceed from that surface, with a rapidity determined by its temperature. Should the superincumbent air be extremely dry, the aqueous vapour will diffuse itself through it, *up to a certain point*, in the form of pure or invisible vapour, and there will be no Fog. The extreme limit to such diffusion is plainly this,—so soon as the air is saturated, in other words, so soon as the quantity of vapour within it becomes so large that, at its own temperature, it can sustain no additional quantity in the form of pure vapour—the vapour, still arising from the warmer surface below, cannot diffuse itself in the form just alluded to, and *must be precipitated as it arises.* To the formation of *Fogs*, therefore, there are two, and only two essential conditions, viz. a moist surface warmer than the superincumbent atmosphere, and that the atmosphere itself be moist, or nearly saturated with aqueous vapour. These two conditions existing, Fogs are inevitable; and the greater the excess of the temperature of the moist surface over that of the saturated air, the denser and more extensive must be the Fog. It is well known that in winter the surface of masses of water is warmer in general than the atmosphere. From the first of November to the first of March, in our Latitudes, the air is colder than rivers, lakes, or the sea; and as that is the season of the year when the Hygrometer indicates the nearest approach to saturation in the Atmosphere, it is easily seen why that also is pre-eminently the season of *Fogs.* All local and apparently anomalous peculiarities are reducible within the same law. For instance, Kæmtz relates, that on seeing a column of Fog, rising from a small part of a field, on the breaking out of the Sun after a heavy rain, he ran to the spot and ascertained, that the sphere of the fog was a mown meadow, surrounded by pasturage of high grass; the latter being less heated than the mown surface, gave rise, of course, to a far less active evaporation. In Switzerland again, such lakes as those of *Thun* and *Brientz*, are often quite clear, while the neighbouring lakes of *Zug*, *Zurich*, and *Neuchatel*, are covered with a dense mist: the reason is, that the latter do not receive such streams from the glaciers, as feed the lake of Brientz—its tributary the *Aar* flowing directly from the eternal snows of the *Grinsel.*—The extraordinary density of the famous *London* Fogs—of which we have frequently the counterpart in *Glasgow*—is perhaps referable to two causes; viz., the great and exceptional surface temperature due to the presence of an immense city, and the intermixture, with the aqueous precipitation, of an immense development of smoke. In so far as they contain this latter element, such mists partake of the character of *Dry Fogs.*—3. The phenomena of Fogs have been connected with the Electric Forces, by M. Peltier, in one of those memoirs which may be said to be still under consideration of Physicists. The Hydrometeors in question are divided by this inquiry into two classes, viz. *Simple* or *non-Electric Fogs*, whose function is due solely to the phenomenon specified above—the reduction of the temperature of the air, three or four degrees below that of the surface of the water or the moist ground,—and *Electric Fogs*, of which Peltier thinks he has discerned *four* varieties. The fundamental principle of M. Peltier's researches is this:—dissenting from Pouillet and others who discern in the act of Evaporation a *cause* of development of the *Electric Forces*, he considers the development and relation of Atmospheric vapours, as largely owing to the opposite Electric states of the Earth and Atmosphere, and always modified by the degree of that polar opposition. The supposed *effect*, he deems an *efficient cause.* The Earth being negative, all vapour rising from it must be negative also; and, when the negative or resinous tension is powerful, Peltier insists that Evaporation must proceed with great rapidity, inasmuch as the vaporous particles will be propelled upwards with an augmented force. In tropical regions, for instance, where the tension of the Telluric and Atmospheric Electric Forces is very great, the natural stream of Evaporation, must be greatly facilitated, if not increased by this cause; and as the stream of tropic vapour converts itself into a great stream from Equator to Pole in the higher regions of the air, we must expect to find there a comparatively permanent stratum of negative or resinous force. As to Fogs, the following is a résumé of Peltier's views:—"It would seem that *resinous fogs* should be the more numerous, because as the terrestrial globe is a body charged with resinous electricity, the vapours that rise from it are resinous like itself. This species, however, is not common; the cause of transformation in the signs of the electricity is, in the very law itself of electrical induction. The earth repels the resinous electricity toward the higher strata, and thus renders the stratum that is nearest to the ground vitreous. In order that such a fog may remain in contact with the surface of the globe, it is necessary that another power should have a preponderance over the repulsion of the earth, or that this terrestrial repulsion should be reduced by a similar force, acting in the contrary direction. The former effect is produced by the specific gravity which clouds sometimes acquire; and the latter, by the repulsive power of the highly resinous upper strata. Resinous fogs, produced by these two causes, are distinguished by particular qualities, which divide them into two different species.—" *Vitreous fogs* are also of two species, which present very distinct results. The first is that which occurs under a serene sky, without any other electric influence than that of the globe. This species has its lower portions more vitreous than the upper, and they are powerfully attracted by the globe. The other species is that which is formed under the influence

of the masses of highly resinous vapours, which prevail in the upper strata. This latter has its upper portions more vitreous than the lower."—The whole of these speculations are involved in the obscurity still surrounding every question connected with Atmospheric Electricity, and the efficient cause of Evaporation. It is impossible, however, to deny the credit to Peltier, of having regarded the subject under a novel and far from an unnatural point of view.

(II.) *Dry Fogs.*—Atmospheric meteors, which,—although they do not affect the Hygrometer, or in any way indicate moisture—have yet the obscuring power of fogs, are frequent in certain districts, have now and then in a way apparently very anomalous, affected large spaces of the Earth, and are said to occur regularly at certain seasons of the day in various countries. 1. A dry fog is periodical, or recurrent with the season of the year, over a very extensive district of North Germany. Accurate observation has now dissipated the mystery that long concealed the cause of this meteor. It is smoke arising from those vast burnings of the *peat beds* occupying so much of North Germany, which, for agricultural purposes, are annually resorted to. The district within which these systematic and prolonged fires take place is immense; and this *smoke* or *dry fog* is carried over other extensive spaces, whose position depends on the quarter from which the wind blows. Every feature of the once perplexing phenomenon can thus be explained: the same thing on a small scale, is observable in the neighbourhood of every important manufacturing town in Great Britain, or in those Irish districts where corresponding agricultural processes have been introduced. 2. But phenomena of a similar kind, only extending over much vaster areas, and quite independent of year or season, are also on record; for instance, the extraordinary dry fog of 1783—so much noticed at the time,—extending from Norway to Syria, or through 25° of Latitude, and from England to the Altai, or over 120° of Longitude. This extraordinary fog, so often described, as colouring all things blue, and remarkable likewise because of its keen and acrid odour, is unquestionably referable to clouds of *volcanic ashes.* Earthquake shocks and terrible volcanic eruptions shattered the old world in that year, from Iceland to Calabria. In Calabria, the entire relief of the country was changed; more than a hundred mountains were torn up, overthrown, and transported; fifty new lakes arose, because of the blocking up of rivers; and more than one hundred thousand persons were destroyed. Direct observations, now in great number, have established that volcanic ashes, from the eruption of a single volcano, are often carried to great distances, and that as they drift along, they obscure the sun, and destroy the transparency of the air. Such the origin of the terrible fog of 1783; and every other similar record, connects itself with the same class of phenomena.—3. The last sort of dry fog, consists mainly of that dry *smoky horizon* said to be observed in different localities. Its origin is unknown; but we must in fairness express a doubt, whether the facts regarding it have been established. The *Callina* of southern Spain is perhaps its best exemplification. It first appears about the middle or end of June, constituting all round the horizon, a band of bluish-gray mist. In the middle of August, when the temperature has reached its maximum, the Callina covers about a quarter of the whole sky. At the horizon, its colour is then a reddish-brown; higher up, it is yellowish; and from its upper rim, as if a thin gauze of lead, seems at times to spread over all the sky. No odour belongs to it; and the traveller never knows that he has entered amid the curious fog,—its existence is revealed solely by the aspect of distant objects. Towards the end of August, the Callina becomes less remarkable, and disappears wholly about the beginning of October, or at the autumnal equinox.—It may be remarked, without injustice to observers, that it is by no means established, that this Callina, and other similar aspects, have no effect on the Hygrometer, or that they are *dry fogs.* More delicate instruments, such as those now in use, will probably reveal characteristics of them in this respect, hitherto unsuspected, and capable of throwing light on their origin.

Force, in mechanics, means an action between a pair of bodies, which changes, or tends to change, their relative condition as to rest or motion. By an extension of the term "force" to other branches of physical science, it is used to denote any action between a pair of bodies which changes, or tends to change, any physical relation between them, whether mechanical, thermal, chemical, electrical, magnetic, or of any other kind.—Forces are compared in magnitude, and expressed as quantities in three ways, which may be reduced to two:—1. The *Statical Measurement of Forces* is founded upon balancing two or more of them against each other, so that their effect is null. Two forces which balance each other are said to be equal and opposite; two equal and similar forces combined require a third, opposite and of double magnitude, in order to balance them, and so on. Mechanical forces, as thus measured statically, are represented in magnitude and direction by straight lines; and the line representing the resultant of any number of forces, is found by putting together the lines representing the component forces, and joining the extremities of the compound line so formed.—2. The *Dynamical Measurement of a Force*, in mechanics, is effected by finding the change of velocity which it produces while acting unbalanced, during a unit of time, upon a body whose mass is unity. It is known by experience that the results of this and of the preceding method of measurement agree. The common British unit of force is the pound avoirdupois, being the weight in vacuo of a certain piece of platinum, which is kept in the

Exchequer office. The British absolute unit of force is the force which, acting unbalanced for one second upon a mass which, in London, weighs one grain, or $\frac{1}{7000}$ pound avoirdupois, produces in that mass a velocity of one foot per second. This unit is about $\frac{1}{32.2}$ of a grain.—3. The third mode of measurement takes into account not only the magnitude of a force, as determined by the first and second methods, but the magnitude of the change throughout which that force continues, or is capable of continuing, its action; and the *product* of those two magnitudes gives a quantity which, by some writers, is called *Force*, or *Living Force*, and by others, in order to avoid ambiguity, *Energy*. When the magnitude of the force, measured statically, is variable, it is to be understood that the mean value of that magnitude is the factor by which the magnitude of the change is to be multiplied; or, what is the same thing in other words, that the energy is the *integral of the force with respect to the change throughout which it acts, or is capable of acting.* The British unit of energy is a force of one pound avoirdupois weight, acting through a distance of one foot, and is called a foot-pound. —There are different forms of energy, according to the kinds of force and of change by which the energy is constituted. Those forms are distinguishable into two classes—*potential energy*, which is constituted by a force *capable of acting* throughout a certain change of condition; and *actual energy*, which consists in a state of change going on. The following are examples of particular forms of energy:—The potential energy of a pair of bodies which attract each other, is the integral of their attraction with respect to the distance between their surfaces; or, what is the same thing, the product of that distance into their mean attraction throughout that distance. —The actual energy of a system of bodies moving with given velocities, relatively to their common centre of gravity, is the sum of the products of the mass of each body into the half-square of its velocity.—The potential energy of a pair of substances which tend to combine chemically, is the product of the weight of the compound which they tend to form into a specific constant, depending on the nature of the substances.—The actual energy of heat in a hot body is the product of its weight, × its absolute temperature, × its real specific heat, × a constant, called "Joule's Equivalent." (See HEAT, MECHANICAL ACTION OF.)—The actual energy of an electric current is the product of the square of the strength of the current into the resistance of the circuit.—If the meaning of the word *force* were to be extended to all combinations of forces, it would also comprehend couples. A couple consists of a pair of equal and parallel forces, opposite in direction, but not directly opposed, applied to one body, or connected system of bodies. The tendency of a couple is to make the body or system, to which it is applied, turn about in the plane of the lines of action of the pair of forces; and the magnitude of that tendency (which is called a *statical moment*) is measured by the rectangle of the line representing the magnitude of either of the forces, and the perpendicular distance between their lines of action. A couple cannot be balanced by any single force, but only by another couple in the same or a parallel plane, equal in moment and opposite in direction, or by a combination of couples. The word *force*, however, is never employed in the sense of *couple*. See COUPLE.

Force, Conservation of, or **Conservation of Energy**, is the fact that energy, as defined in the preceding article, can neither be created nor destroyed, but only transferred and transformed; in other words, that *in any system of bodies, the sum of the potential and actual energies of the bodies is never altered by their mutual actions.* This fact, or physical law, has been learned by experience; but it can also be shown to be essential to the permanent existence and order of the known universe.—In mechanics, this law is simply equivalent to the two long-known principles of virtual velocities, and of the conservation of *vis viva*. As to its application to other branches of physics, see HEAT, MECHANICAL ACTION OF; MACHINE; THERMO-ELECTRICITY; THERMO-MAGNETISM; also, Grove on the *Correlation of Physical Forces*; Rankine *on the Science of Energetics* (in *Edinburgh Philosophical Journal*, 1855).—Besides energy, there are two mechanical magnitudes which are constant in a system of bodies acted upon by their mutual forces only,—viz., the *resultant momentum* of the system, and its *resultant angular momentum*; but these qualities are purely mechanical, and their conservation in the mechanical form is absolute under all circumstances, being a necessary consequence of the equality of action and reaction; so that they do not connect mechanics with other branches of physics, as the quantity called energy does. See CENTRE OF GRAVITY; and AREAS, CONSERVATION OF.

Forces, Correlation of, is the term used by Mr. Grove to express the fact, that every kind of physical force (in the sense of "energy") may be made the means of producing every other. (See FORCE, and FORCE, CONSERVATION OF.)

Fortification. The art of covering an army with an immovable defence, or of putting a piece of ground into such a condition, that an army behind it or surrounded by it, may be able to resist another army of superior strength.—The character of such artificial fortification must, of course, be largely modified by the nature of the ground; and it has also necessarily changed, as the modes of offence or attack have changed. We cannot, within the limits of this article, touch on the history of this art,—the grand crisis of which naturally attached itself to the invention and use of gunpowder: neither shall we attempt to refer to the innumerable variations and expedients,

which peculiarities in the contour of the spot he is fortifying, will suggest to every good officer of engineers.—A general notion of our modern fortified enclosures, will be obtained from the annexed drawing;—our description of it presupposes a certain elementary knowledge; nor can we spare room for more than a statement and criticism of principles. The main line *a*, *a*, *a*, *a* (fig. 1), is a parapet of no great elevation, sloped by earthworks, towards a ditch *b*, *b*, *b*, *b*. B and B, &c., are its *bastions*, and outside the whole is the ditch, *b*, *b*, *b*. Along

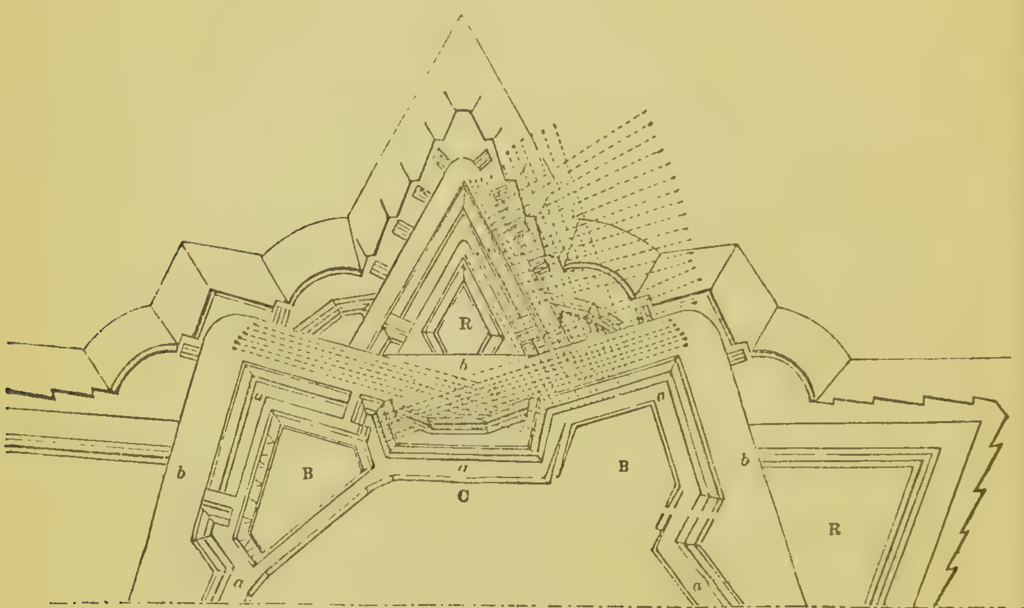


Fig. 1.

these bastions are embrasures with ordnance, whose general sweep is sufficiently represented by the lines of *shot*. The exposed and weak part of the curtain C, is always defended by an exterior triangular work beyond the ditch; viz., the *Ravelin* R. Beyond the ravelin are other *places d'armes*, and the whole terminates in a slope or *glacis*. It is pretty clear that the flanks or sides of the Bastions are all well defended by *direct fire*. The point which is not and cannot be so defended, is the *salient angle* of the Bastion; and it is there accordingly that the breach is most frequently effected by the assaulting force.—Let us next glance at the usual mode of attacking a place so fortified. The attacking army makes its approaches towards the first parallel (which is usually just beyond reach of the fire of the fortress) by zig-zag ditches or trenches. At the distance determined on, the first parallel is thrown up, as in the subjoined cut (fig. 2). These parallels are ditches, with the earth dug out of them accumulated in front, so as to form a sloping earthwork, little liable to be seriously disturbed by the guns of the fort, and being therefore a sufficient protection to the besieging troops. On the edge of this parallel, at convenient places, batteries are established, which, as represented in the cut, ought to be sufficient to disable the artillery of the defence, and which, in practice, quite succeed in destroying the effect of the more advanced armament of the fortress. So soon as this is accomplished, another zig-zag is dug, and the besiegers establish a second parallel, from which the fire of their new batteries is, of course, much more destructive. A third parallel succeeds, from which a practicable breach is made without difficulty; and if the garrison does not capitulate, the *assault* follows. The drawing makes it sufficiently manifest, how certain of the more distant batteries are employed to enfeeble the flanks of those ravelins and bastions, that would play with more deadly effect on the works of the advanced parallels. It is, of course, understood that, all the while, murderous shells, and a vertical *feu d'enfer*, have been sadly thinning the ranks of the garrison, however protected by casemates.—On a cursory glance at the condition of this great mechanical conflict (for such it is), two general considerations of primary importance present themselves. *First.* In order to secure success on the part of the besiegers, it is necessary that the force of the garrison, and its amount of available *materiel*, be limited. Should its power, or its means of supply be such, that the breaches made each day by the batteries of the first parallel, can be repaired during night, and the disabled guns replaced indefinitely by fresh ones, it is not difficult to see that the advance of the besiegers may be impeded also indefinitely, and the capture of the fortress become impossible. These conditions, of course, never do occur in the case of a thoroughly in-

vested or beleaguered fortress; but they did occur at Sebastopol. The siege of that powerful arsenal was far less a regular siege, than, as Lord Palmerston justly described it—a struggle between the entire might of Russia at one extremity of its dominions, and the might of the Allied Powers at a point removed by two or three thousand miles from *its* centre. And the result depended as much on the ability of the several belligerents to put forth their full or disposable strength at that particular point, as on the absolute *weight* of that strength. Russia effected marvels; but she

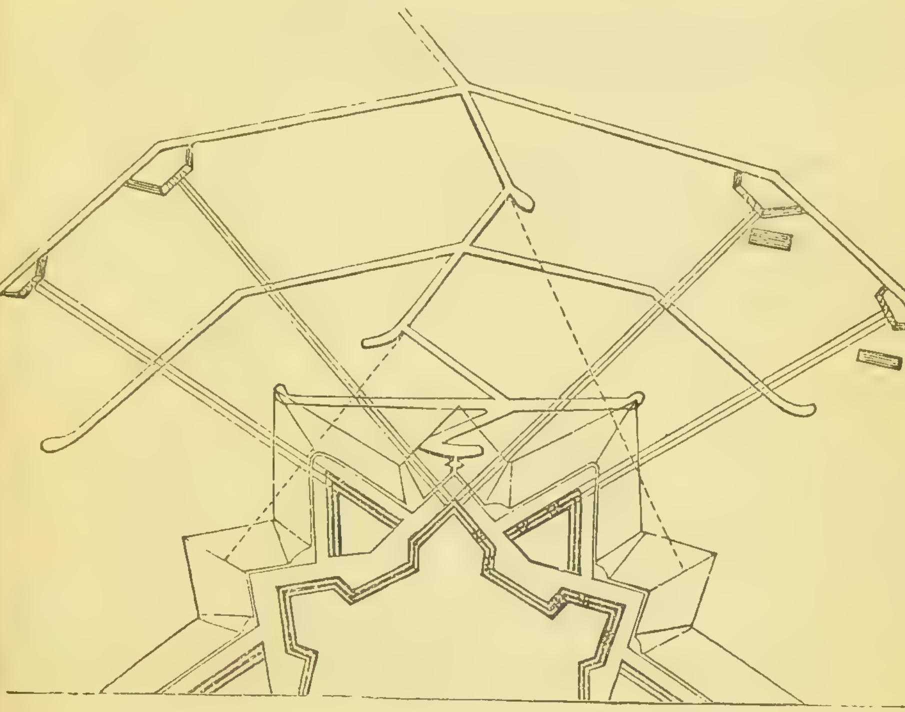


Fig. 2.

had her arsenal prepared; she was resisting an invasion; and her military system had been laboriously constructed with a view to such exigencies. The Allies—especially Great Britain—had the advantage of no such preparation; and the issue of this partial conflict ought to leave no doubt, as to the potential result of the terrible strife now agitating Europe.—But, *secondly*, the superiority of the besiegers depends on this—*can they establish their first batteries?* If the fire of the fortress can be concentrated on any given point, with sufficient force to hinder the establishment of a battery there, no effective approach can possibly be made. The range of the ordnance of a fortress may, of course, be always quite as great, as that of any probable besieging train; the question, therefore, is, as to the ability to *concentrate* its fire. And here is the weak part of the bastion or modern fortification system. It is a maxim in this system that the bastions be low; on which account they carry only one tier or range of guns. The lowness of the bastion wall, is dictated by the necessity that no wall-face be exposed to the hostile batteries; no such, indeed, could stand the shock of a modern battery for an hour,—witness the speedy destruction of the high walls of the MALAKOFF, by the first and comparatively feeble bombardment of the Allies. But this advantage is secured by the immense sacrifice that the opposing batteries can be acted on by only one tier of guns,—a disadvantage whose magnitude must be patent to any one who chooses to compare the shot of an ordinary ship, with the terrific broadside of a three decker. There is, however, another great disadvantage as to the defence of a modern bastion: it is this,—the guns being placed along a *line*, and the embrasures being, for security, necessarily narrow, the *concentration* of much of their fire on a distant point is utterly impossible. And in these two circumstances lies, at the present moment, the great superiority of *besiegers*. In such a conflict as that which has terminated at Sebastopol, the besiegers had no such advantage. From the nature of the ground, the indefinite supply of labour and *materiel* at command of the Russian army, it was not difficult to meet battery by battery, nay—with all allowance from what is due to the genius of TODLEBEN—it is easy to see, that the besieger could in no case expect aid from the operation of circumstances usual in sieges: so that, again, this was not a siege, but a conflict of Empires. We doubt not that when its history shall be fully written, it will be found

that the Engineers and Artillery of the two Allied Armies have well performed their duty.—Such being the defects of the Bastion System, the momentous question arises,—can they be removed? Is it not possible to have *tiers* of guns, without high and *destructible parapets?* And, with all its advantages as to flanking, may not the rectilineal arrangement of guns be supplanted by some curvilineal arrangement, that will permit very largely of *concentration* of fire? The Engineer is requested to glance at the annexed sketch of a fort, proposed by Mr. Fergusson. The general idea and little else is represented by it; but the idea may be carried out into every detail or minute part of a fortress. The lower figure is the *ground* plan; the upper the *relief.* The broad ditch is in this case supposed filled with water from a neighbouring river or coast. But it may be dry. With the earth obtained from this deep and broad ditch, let a mound be framed of great dimensions, encircling the position to be defended; and let this mound be cut, as the *profile* shows, into a succession of terraces (fig. 3). On each terrace is a rampart with its

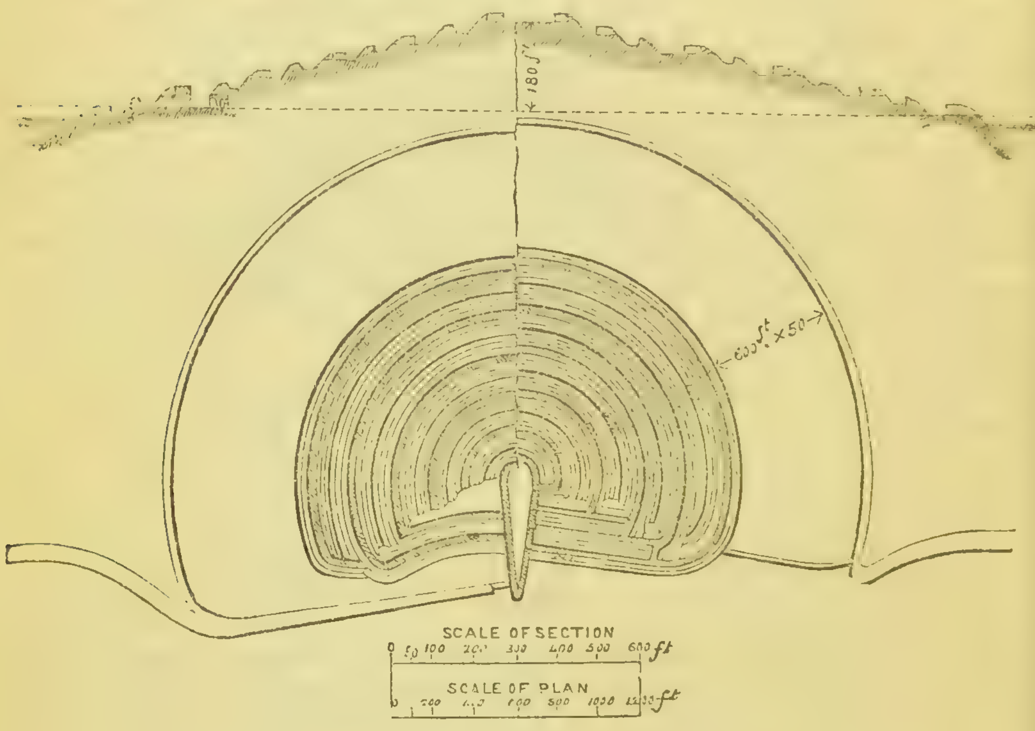


Fig. 3.

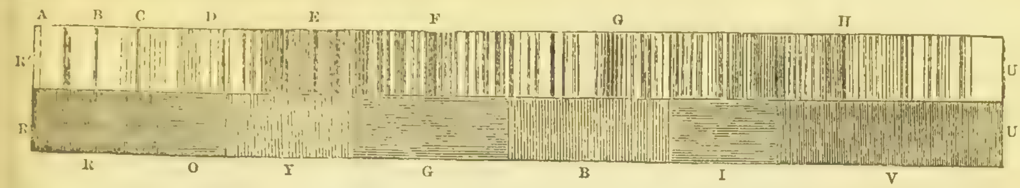
array of guns; and, beneath, the casemates and magazines. The power of concentration of fire, on the part of such a fortress, against any external point, and of course its power to render nearly impossible the construction of any effective external battery, is evident; nor could any great city, with adequate internal supplies, thus fortified, be taken, unless after, what has just occurred—a trial of strength between *Empires.*—In all sieges, the element of *Time* is everything. If a great siege is prolonged, it must be relieved, or the confession made that the State to which it belongs is defeated.—We cannot go further into detail in this Dictionary:—it is enough to have laid down the general mechanical conditions that control the entire subject. As to the question recently raised, regarding earthworks and parapet walls of masonry, we refer with pleasure to two remarkable papers, understood to be from the pen of Sir John Burgoyne, that have recently appeared in the *United Service Magazine.*—See further, the *Sieges in Spain*, by General Jones. The military student will, of course, resort for all technical information to the common well known works; but we especially request the attention of all thinking engineers to the remarkable essay by Mr. Fergusson.

Fortuna. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Fracture. See ELASTICITY.

Fraunhofer's Lines. A name given in honour of the Physicist who first thoroughly examined them—(they were certainly *discovered* by our own Wollaston)—to a singular system of vertical dark lines, which a telescope of sufficient power reveals in the prismatic spectrum. This spectrum, it is well known, consists of seven colours, which are very noticeable, viz. *red*, *orange*, *yellow*, *green*, *blue*, *purple*, and *violet.* That the space occupied by these, does not exhaust the sphere of the true spectrum as we now understand it, is fully explained under

SPECTRUM; nevertheless, we do not require, at present, to overpass the limits of the noticeable or bright spectrum as above defined. As already stated under article DISPERSION, the Spectrum thus examined would appear as if, in the subjoined figure, the upper portion showing the dark lines were superimposed in the lower or coloured portion:—



It will be seen at a glance how irregular is the distribution, and how various the character of these remarkable lines,—some of them being strongly marked, others faint almost to evanescence—part of the spectrum being very free from them, while other portions are quite *ribbed*, now, by multitudes of lines of very perceptible distances, and in other cases by groups so close, that to a small magnifier they appear as one single line. That points of certain and fixed reference, might be found within this confusion, Frauenhofer distinguished the lines marked as B, C, D, E, F, G, H, as offering the double advantage of being readily recognized, and distributed with some regularity through the spectrum; and all physicists since have followed his example. One of the chief practical uses of these lines has been explained under article DISPERSION, *q.v*—It is worthy of remark that while the ratios of the distances of these lines vary in the *Spectrum* of *Refraction* with the refractive medium employed, they never vary in the *Spectrum* of *Diffraction;* and it is to this latter spectrum that we must always apply in the attempt to determine the length of those waves, which, according to the theory of Undulation, produce the different colours.—We shall briefly refer to several points of great interest concerning these lines:—(1). It may be confidently stated that we have not yet obtained a correct idea of their actual *number*. According to Frauenhofer, there are *nine* well defined ones between B and C; between C and D, *thirty;* from D to E he counted about *eighty-four;* from E to F upwards of *seventy-six;* between F and G there are *one hundred and eighty-five;* between G and H *one hundred and ninety*. Taking account of the lines outside these limits, Frauenhofer reckoned that the solar spectrum, in its entire length, shows from *six to seven hundred* of these very enigmatical bands: but the researches of Sir David Brewster have extended this number to something like *two thousand*. It is not yet well determined, whether the conditions of observation (*e.g.*, the region of the sky from which the spectrum is obtained), have serious influence, or what his exact influence is, on the visibility of these bands.—(2). Phenomena similar in kind, but very diverse in detail, are detected in spectra yielded by light not issuing directly from the sun. Take as instances the following facts:—The *Electric Light* yields *bright* instead of *dark* bands—the one of greatest brilliancy being found in the *green*. Solar Light, *reflected* by the moon and planets, yields the same system of lines as the direct spectrum; but with the *Fixed Stars* the case is wholly different,—*Sirius* yields quite a peculiar system, remarkably distinct and definite; the spectrum from *Castor* is like that from *Sirius; Pollux* yields a great many feeble lines; while *Procyon* manifests very few.—The phenomena manifested by spectra, from the flames of different substances when under combustion, present a most perplexing diversity. In some cases we have multitudes of lines, in others none are perceptible; while in a few instances, *coloured* lines also appear. Our limits forbidding the specification of these curious details, the student is referred to the elaborate and admirable researches of Sir David Brewster; to the papers by Professor Miller of Cambridge, on the spectra produced from certain coloured flames, and from the combination of different substances in a jet of oxygen and hydrogen; and still more to the extraordinary results procured by Mr. Wheatstone, M. Foucault, and M. Soleil, on spectra from the ignition of metals, &c., between the poles of a voltaic pile. It is unfortunate that no result of a general nature, or calculated to throw light on the origin of these lines, has hitherto been deducible even from so brilliant a series of experiments.—(3). Perhaps the most remarkable and pregnant phenomenon yet remains to be described. On examining the spectrum formed from ordinary light, transmitted through the thick and red vapour of nitrous gas, Sir David Brewster made the striking discovery of the existence of a multitude of new dark bands, crossing it at all points, and parallel to the dark lines of Frauenhofer,—these lines being broader, darker, and more numerous towards the more refrangible extremity of the spectrum, and always appearing, whatever the nature of the light employed. Many other gaseous substances have since then been examined by the foregoing distinguished inquirer, by Professor Miller, and the late Professor Daniel. Professor Miller expressly engaged in elaborate researches, with a view to some clue to a connection between the phenomena produced and the nature of the substances through which the beam of light is made to pass:—unhappily with very slight effect. The following conclusions, for the most part nega-

tive, appear established by his researches:—*First:* Colourless gases give, in no case, additional lines, or lines different from those of Frauenhofer. *Secondly:* The mere presence of colour is not a security that new lines will be produced; for instance, of two vapours, undistinguishable by the eye, one, *brome*, gives a great number of new lines,—the other, *chlorure* of *tungsten*, exhibits none. *Thirdly:* The position of the new lines has no connection with the colour of the gas:—with *green* perchlorure of manganese, the new lines abound in the *green* of the spectrum; with *red* nitrous acid, they increase in number and density, as we approach the spectrum's *blue* extremity. *Fourthly:* Simple bodies, as well as composite ones, evolve these lines. Two simple bodies, which, when isolated, produce none, often produce multitudes when in combination. And conversely, lines which appear in the vapour of a simple body, frequently disappear when the vapours of its combination are employed. *Fifthly:* The same lines are frequently produced by different degrees of oxidation of the same substances. *Sixthly:* The lines increase in number and density with the thickness of the medium through which the ray of light is being transmitted, or when some accidental cause deepens its colour. When the vapour is dense and uniformly diffused, they are often too numerous for the eye to count them. *Seventhly:* Whether ordinary or polarized light be used, the result is always the same.—It scarcely requires us to add, that the whole subject remains one of the most obscure in physical optics. —(4). Quitting the mere phenomena, and rising to the inquiry as to their *causes*, we enter on a still more arduous path. The phenomena—defying, as we have seen, all attempts hitherto to reduce them within empirical laws—no complete explanation or theory of them is possible. All that theory can be expected to do, is this—it may explain how dark lines of any sort may arise within the spectrum. Of the two opposite general views concerning the nature of Light, the theory of *Emission* offered, apparently, the readiest, although a very vague and unsatisfactory solution. A dark band in the spectrum merely indicates a certain amount of light destroyed,—it has been *absorbed*, said the Emissionists, in passing through the media it has traversed. The dark lines of Frauenhofer, therefore, are so many indications of absorption: and the opinion assuredly received great apparent support, from the action of gaseous media as above described. But in the present condition of science, it were worse than folly to accept any explanation from the theory of *Emission*. See LIGHT. According to the opposite doctrine of *Undulation*, the destruction of Light can only arise from the *Interference* of waves. See INTERFERENCE. But the question recurs, how may we suppose that adequate interferences do in this case take place? The answer to this question is, unfortunately, not possible within the limits of a work like the present. It has been undertaken by Cauchy, by Erman, and especially by Baron Von Wrede. Simply remarking that the attempted solution is in every respect most ingenious, we again refer the student to the dissertation of the latter Physicist in Taylor's *Scientific Memoirs*. See also INTERFERENCE.

Freezing, in its most general signification, is the process by which a liquid body passes into the solid form, when reduced to the requisite temperature;—more usually it refers to the formation of ice or the solidification of water. We shall employ it here in its restricted meaning, partly because the phenomena accompanying the freezing of water may be taken as representative ones. Several important classes of consideration are connected with "Congelation."

1. The *thermal* phenomena which present themselves during Freezing and Fusion, attracted attention long ago. Observers could not fail to be struck by the fact that a block of ice, exposed to any amount of heat, never becomes warmer than 32° F., while the water dropping from it indicates the same temperature: and yet a great quantity of heat must have entered it during that process of melting. The converse also holds, *i. e.*, a mass of water does not freeze suddenly; heat is being abstracted from it during the progress of solidification, but it remains steady at 32° F. The phenomenon is clearly this—a quantity of heat, during fusion, becomes insensible to the thermometer, and takes on some new form or function. The nature of this change of form and function has been variously accounted for, according to the nature of the theory of heat prevalent at the time:—the view at present received is given farther on, in HEAT, MECHANICAL THEORY OF. But apart from speculation, there is a practical problem of highest importance, viz., What is the *quantity* of heat which disappears on fusion, and reappears to the thermometer on freezing? The honour of determining this important physical constant, within narrow limits of error, belongs to the Scotchman, Dr. Black. Black's method was of course the *method of mixtures*. Taken in its full generality the process is this:—Introduce into a vessel containing a known weight W of water at the temperature t, a piece of pure ice at *zero* (0° C.; 32° F.), melting rapidly ensues. On its completion the temperature must be measured,—call that θ. Weighing the vessel containing the mixture, we find the weight of the ice added = W. Supposing that obvious causes of error, eliminated by the care of the experimenter, and the multiplication of experiments, it is easy to see that

$$W \cdot (t - \theta),$$

or the quantity of heat lost by the *water* during the process, must be equal to the sum of those employed in melting the ice, and in raising the water resulting from it to the temperature θ; so

the heat necessary to melt a e must have

$$(t - \theta) = W' x + W' \cdot \theta,$$

or

$$x = \frac{W \cdot (t - \theta) - W' \cdot \theta}{W'}.$$

original determination was 144° F. afterwards, and Laplace and Lavoisier, the constant much less; but the more ..t and far more accurate processes of Provost- ..e and Desains, have nearly reinstated Black's number. The constant now accepted is 142°·65 F., or 79°·25 C. :—*i. e.*, during the melting of a pound of ice this definite quantity of heat ceases to be appreciable by the thermometer. The same method is of course applicable to all solidifications and fusions; and it has been applied successfully to the case of various liquids.

II. In the case of many liquids — (and here, too, *water* may be taken as a *representative* substance) — some apparently anomalous circumstances appear, when the temperature approaches the freezing or congealing point. The fact that *ice floats*, shows that its density is less than the density of the unfrozen, and therefore warmer water, in which it is floating. But this seems contrary to the general law, that the density of substances diminishes as their temperature ascends. About the year 1804 this curious subject was investigated by Tralles, of Switzerland, and Hope, of Scotland; and both drew conclusions from the same experiments. If a vessel full of water—say at 50° F.—is left in absolute rest, in the midst of a space at 32°, or some inferior temperature, the lower strata of the liquid show for some time the growing depression of temperature sooner than the upper strata. But after the thermometer immersed in the lower strata has reached about 40°, it remains invariable, until the upper one shall itself have attained it; after which it descends anew, only less rapidly than the upper thermometer, around which *ice* begins earliest to form. The result evidently proves that water attains its maximum temperature at 40° nearly; after which it begins to expand, and the expansion continues until solidification takes place. *Despretz* has recently explored the whole progress of the phenomenon, and represented by curves the relative march of the two thermometers after the discrepancy begins to disappear. The point of maximum density he fixes at 39°·2 F. He has further extended his researches to a large number of saline solutions.—The explanation usually received is probably the true one. The liquids which manifest the freezing phenomena, all pass into *crystalline* solids; and this point of maximum density seems to mark the commencement of that internal molecular arrangement which constitutes the crystalline state. Now, whenever such ultimate arrangement is not consistent with the closest molecular *packing*, or with the greatest economy of space, expansion must ensue from a certain fixed point, and continue until solidification is completed.—Simple though this phenomena seems, it plays an important part in the economy of nature,—*e. g.*, the icy covering of a stream really defends the water under it from excessive external cold.

III. The temperature of the freezing point is not absolutely fixed and constant,—rigorous scrutiny having brought to light minute irregularities, some of which carry important consequences.—(1.) The temperature of the point of fusion, although more steadfast than its correlative is not steadfast,—several considerations rendering it probable that the interior of a block of ice may be even 3° above 32°. But water may be kept from solidifying until its temperature is 22°, or even lower, just as it may be raised to 270° under the ordinary pressure of the atmosphere, and remain as water. The subject has received much interesting experimental illustration from the researches of M. Donny. Faraday's theoretical views have been given under CONGELATION. He thinks, apparently correctly, that the irregularities in question are due to a certain *range of cohesion*, which enables substances to withstand a change of temperature which, without that cohesion, must have caused a change of state.—(2.) One general and uniform law to which the phenomena of freezing is subjected was recently discovered by Mr. James Thomson, and experimentally confirmed by his well known brother, Professor William Thomson. It occurred to the latter, as a consequence of the great principle of Carnot, *that water at the freezing point may be converted into ice by a process solely mechanical, and yet without the final expenditure of any mechanical work.* But as water in freezing *expands*, and therefore must exert a certain mechanical effect, this is tantamount to the assertion that mechanical work could be got out of nothing! In illustration, suppose that into an indefinite lake at 32° a cylinder with air at 32° is plunged. Compress that air suddenly by a piston. Heat will be given out, and diffused through the lake. Let the piston, being relieved from the compressing force, be permitted to start back to its original position; it is evident that the expanding air will withdraw from the water nearest it the heat it gave out, and that in consequence this water must freeze. Now, at the close of the experiment, all things are as they were at first; the force employed in compressing the air has been returned by the equivalent force of resilience; while the freezing of the water and the mechanical effect due to its expansion are superadded,—in other words, we have obtained these to the bargain! Mr. Thomson, with great sagacity, detected the necessary presence of a new and unsuspected element, and he at once declared that this element must be the truth *that the freezing point becomes lower as the pressure to which*

the water is subjected is increased. The original memoir will be found in the *Transactions of the Royal Society of Edinburgh*, vol. xvi.; and is a very fine instance of cautious and successful general reasoning. Mr. Thomson even deduced the formula applicable to the phenomenon:—it is this, the lowering of the freezing point for n atmospheres of pressure is

$$t = 0°{\cdot}0075\, n \cdot \text{C}.$$

Professor W. Thomson's experimental verification of this formula is described in the *Philosophical Magazine* for August, 1850. This investigation is not more interesting and important through its results than its character. It is a thoroughly *à priori* deduction, or one of those happy previsions of facts, which go so far to illustrate and confirm theory. Nor is it the first time that such predictions have been ventured on by aid of Carnot's Function.—We shall advert to some consequences of the discovery under ICE.

Freezing Mixtures. See "*Cyclopædia of Chemistry.*"

Friction. A form of mechanical resistance to motion, depending on the structure and surfaces of bodies in contact, which demands closest attention on the part of every one having to do with the working of machines. The influence of Friction indeed, alike as it affects the stability of fabrics, and the action of machines, is one of those points whose determination constitutes a chief object of the science of Engineering.—There are two primary kinds of *Friction*,—that which impedes the *sliding* of bodies on each other, when their surfaces are in contact; and that which may be termed, the *resistance* to *rolling* motions,—the resistance, for instance, which a carriage offers to traction. We shall briefly notice each of these:—

I. Frictions of *the first class*, are also of two kinds—friction of sliding properly so called, and the effects arising from the stiffness of the cords employed in pulling or in any other mode by which motions are connected.—The FIRST description of frictions have been made the subject of extensive experimental researches. And three great or primary laws, may be considered ascertained. 1. *The friction is proportional to the pressure:* that is, the resistance is always the same fraction of the force which presses the one surface upon the other. 2. *Friction is independent of the extent of the surfaces in contact*,—provided always, that the pressure, or weight of the body that lies on the other, be not changed. 3. *Friction is independent of the velocity with which the one body is drawn across the surface of the other:* that is, it requires the same quantity of energy to surmount friction, or to make a body pass across a fixed space—whatever the velocity of its motion.—The *coefficient of friction* for any body or any two bodies, is the constant ratio of friction to pressure; so that, if in any case f be the coefficient, p the pressure, and F the friction, we have

$$\text{F} = p \cdot f \cdot \text{—}$$

and for the *work*, caused by friction, while the bodies slide over each other a certain distance s,

$$\text{W} = p \cdot f \cdot s.$$

In the case of wooden axles, the distance passed over per minute is $n \times 2\,\pi \cdot r$; n being the number of revolutions per minute and r the radius of the axle, the work per second will be

$$0{\cdot}1047, f \cdot n \cdot r \cdot p.$$

The coefficient f is of course determinable by experiment, and extensive tables of its values will be found in all works on Practical Engineering. There are two chief modes of diminishing the resistance of friction—*first*, by employing hard and polished surfaces, and *secondly*, by the use of oily or fatty ointments.—SECONDLY, as to the *Stiffness of Cords or Ropes.* This form of passive resistance has a large and determinate effect, in the action of ships' tackling, in systems of pulleys, in the action of machines for raising weights, such as cranes, &c. It varies with the thickness and nature of the ropes used, and also with the hygrometric state of the air. If a and b represent two quantities, constant for the same description of rope, but varying with the diameter of the rope, with the degree of the rope's soundness, and the hygrometric state of the air, the resistance can easily be expressed according to Coulomb, in terms of those quantities. The difficulty is to determine a and b. The following laws appear established by ample series of experiments:—1. In the case of *new* untarred hempen ropes, the values of a and b (all other things being equal), are nearly as the squares of the diameters of the ropes. 2. In the case of half used up ropes, the same quantities are proportional to the square roots of the cubes of the diameters. 3. For tarred ropes the quantity b is proportional to the number of threads in the rope yarn. The quantities a and b, are severally determined by experiment.

II. *Carriage Traction.*—The general conclusions of chief import, that may be deemed established, are the following:—1. The resistance opposed to the traction of carriages, on *paved* or *solid* roads,—referred to the axletree, and to the direction parallel to the road—is sensibly proportional to the pressure, or to the total weight of the vehicle, and inversely proportional to the diameter of the wheels. 2. In *solid* or *paved* roads, the resistance is almost independent of the *breadth* of the wheel. 3. On *soft* roads, the resistance decreases as the breadth of the wheel increases. 4. On *soft* roads, the resistance is independent of the velocity of traction. 5. On *hard* or *paved* roads, the resistance increases with the velocity. This increase is so much the less, in as far as the springs of the vehicle are good, and the road good and solid. 6. The best direction of traction, is evidently the horizontal one; for in that, the resistance from friction is the minimum.—These

simple laws, must not be taken as absolute, but approximative.

III. *Friction in General.*—The phenomena of Friction have recently assumed an importance not at one time supposed to belong to them. The *heat* evolved, is an essential element; and it is found by accurate experiment, that the quantity evolved, is *exactly sufficient to reproduce the effort caused in overcoming the friction.* See HEAT.—Reference is further made to RESISTANCE.

Fringes. A technical term in optics, meant to indicate those coloured bands of *diffraction*, which appear when a beam of Light passes the clean edge of a screen, or is transmitted through a narrow slit or hole. See DIFFRACTION. The term has also been applied to those curious appearances by which Haidinger has recently shown that polarized light can be detected by the naked eye. The name in this case, however, by no means defines the objects. These should be called Haidinger's *pencils* or *tufts*. They are spoken of, at some length, under HAIDINGER'S TUFTS.

Frost. When the temperature descends below 32° F. all superficial moisture becomes frozen, or passes into the condition of *ice*. This is *frost*, in the common acceptation of the word. *White frost*, or *Hoar frost*, is simply *frozen dew*; a phenomenon occurring, whenever terrestrial *radiation* is sufficiently intense and prolonged, to reduce the temperature of the Earth's surface below the freezing point, while that of the Atmosphere continues above that point. See SNOW.

Fulcrum. The prop upon which a lever rests. Thus, in raising by a lever a piece of stone, a bit of wood is thrust in below the lever, and it is pressed down on the wood, to raise the weight. This is the *fulcrum*.

Functions. Any algebraic expression whose value depends on a simple *quantity*, is said to be a *function* of that *quantity*. For instance, x^n, log. x, sin. x, are all said to be functions of x; and so with every conceivable combination of these.—Functions are generally divided into *Algebraical* and *Transcendental*,—the latter including *Exponentials*, *Logarithms* and *Trigonometrical* quantities or any combination of these. Looking at the subject more generally, the most important subdivisions of Functions are the *Circulating* and *Periodic*. A *circulating* Function, is, any combination of the variable x, which for all possible values of x, from 0 to infinity, shall present only a regularly recurring series of definite values. For instance, sin. x is a circulating function: it can as it is well known obtain no values not included between 1 and — 1, although x be varied infinitely. In the largest sense of the word, the Trigonometrical Analysis, may be considered a fundamental and chief branch of the Analysis of Circulating Functions. *Periodic* Functions, again have for their expression an equation like the following,—

$$\varphi^n x = x,$$

in other words, they are combinations of x, of such a kind, that if that operation or combination be repeated any number n of times, the resulting value shall still be x. These are divided into orders according to the value of n: *i.e.*

$$\varphi^2 . x = x$$

is a periodic function of the second order,

$$\varphi^3 . x = x$$

a periodic function of the third order, and so on. The student may be interested in proving the following functions to be periodic functions of the third order:—

1. $\varphi x = \dfrac{a^2}{a - x}$.

2. $\varphi . x = \dfrac{1}{1 - x}$.

3. $\varphi . x = -\log. (1 - \varepsilon x)$.

The following are a few of the *sixth* order:—

1. $\varphi . x = \dfrac{1}{3\,(1 - x)}$.

2. $\varphi x = \dfrac{3}{3 - x}$.

3. $\varphi . x = \dfrac{1}{x}\left(x^n - \dfrac{1}{3}\right)^{\frac{1}{n}}$.

The subject of *Functional Equations*, is a most important and complex one. In all its generality, it comprehends the theory of *Equations of Differences*, as a particular case. Its object is to determine that form of a Function of the variable, which shall have a particular property. Suppose the Equation to be

$$\varphi . x = \varphi\left(\frac{1}{x}\right),$$

the meaning is—what forms or combinations of x have the property, so that if x be changed into $\dfrac{1}{x}$, their value shall not be altered? These Equations are of infinite variety; nor are they perhaps susceptible of general solution. The student is referred to an interesting and valuable Essay by Mr. Babbage, printed at the close of Sir John Herschel's tractate on *Finite Differences*, and also to the article *Calculus of Functions* in *Encyclopædia Metropolitana.*—The *Calculus of Generating Functions*, may still be best studied in the grand work, where its foundations were laid, viz. the *Theoriedes Probabilités* by Laplace.

Fundamental. The gravest note of a series of several concords, is the fundamental note. Thus, in *sol, si, re, fa—sol* is the fundamental note. So also in a tube capable of producing sound. See ACOUSTICS.

Fusion, is that very noticeable phenomenon with which we are presented, when a solid body

becomes liquid. Some solids become *fluid*, under the application of very moderate heats. Others, again, require more violent heats to melt them. It is interesting to inquire if all are capable of being melted. All but carbon give indications of this. Some organic, and a few inorganic compounds, indeed, do not at first seem so, but they are so easily *decomposed* by heat, that we cannot accurately experiment on them, and we know that their solid components, carbon excepted, are capable of being heated to fusion. Some experimenters have asserted that they find the marks of fusion on the edges of diamonds (pure carbon), which they have subjected to violent heat; but this is not certain. There are two special phenomena incident to fusion. The *first* is, that the fusion of a given body takes place at a constant temperature for the same body, under whatever circumstances it may be placed. The *second* is the absorption of heat which does not become sensible to the thermometer, but which is necessary to the liquid constitution. This subject has already been considered. It thus becomes a matter of physical interest to know the temperatures of fusion of different bodies—and so settled is it, that chemists not unfrequently make it a means of distinguishing body from body (an element of *qualitative analysis*). We take from Pouillet, the following table of the fusing points of various substances:—

Names.	Centigrade Degrees.
English hammered iron	1600
Soft iron (French)	1500
The less fusible steels	1400
The more fusible steels	1300
Manganesed cast iron	1250
Grey cast iron, second fusion	1200
Grey cast iron, very fusible	1100
White cast iron, scarce fusible	1100
White cast iron, very fusible	1050
Very fine gold	1250
Standard gold	1180
Silver. very pure	1000
Bronze	900
Antimony	432
Zinc	360
Lead	320
Bismuth	202
Tin	230
Sulphur	114
Iodine	107
Sodium	90
Potassium	58
Phosphorus	43
Stearic acid	70
White wax	68
Unbleached wax	61
Margaric acid	55 to 60
Stearine	49 to 43
Spermaceti	49
Acetic acid	45
Tallow	33 to 33
Ice	0
Oil of turpentine	—10
Mercury	—39

For LATENT HEAT OF FUSION—see HEAT, section 32.

G

Galaxy. See MILKY WAY and STARS. The term *Galaxy* was at one time used exclusively as an equivalent to *Milky Way:* it has recently been employed as a name for remote clusters. See STARS and NEBULÆ.

Galvanism. See ELECTRICITY.

Galvanometer. See ELECTRICITY and MULTIPLIER.

Gas. A gas may now be defined as the aeriform condition of any one substance or description of matter. The physical distinction between what at one time were termed—permanently electric gases—and vapours, having been broken utterly down by those superb experimental researches of Faraday's, which terminated in the liquefaction, and in some cases in the solidification, of many gases formerly considered permanently elastic,—we are now obliged to consider all the different states in which matter exists, as primarily owing to the relations of their atoms to HEAT. Further remarks on this subject will be found under HEAT—*the cause of change of state:* and the whole large subject of the specific relations of gases to heat, now one of the most important in practical as well as theoretical physics, will be discussed under VAPOURS. Suffice it to recognize at present the signal obligations owing by science in reference to this very important question, to M. Regnault, and to an Inquirer of our own, whose name appears in our preface, and whose contributions form one of the most valuable portions of this Dictionary.—Apart from the relation of gases to Heat, there are several physical qualities belonging to them that merit brief notice.—I. As to their specific gravity, or the comparative weights of equal volumes at the same temperature. Tables of specific gravities are inserted at the close of this volume: we shall merely enumerate at present three important laws that connect the specific gravities of the different gases with their *atomic weights.* (1.) In the case of gases of equal atomic volumes, the specific gravity of hydrogen, multiplied by the atomic weight of the gas, gives the specific gravity of that gas. (2.) In gases whose atomic volume is half that of hydrogen, the specific gravity of hydrogen multiplied by twice the atomic weight of the gas, gives the specific gravity of the gas. (3.) For gases whose atomic volume is twice that of hydrogen, the specific gravity of hydrogen multiplied by half the atomic weight of the gas gives its specific gravity.—II. Gases are singularly *absorbable* by other bodies whether solid or liquid. In the case of solids this seems to depend on their *porosity*, for no chemical change is effected by this absorption. In the case of liquids the amount of absorption is proportional to the *pressure* applied to the gas

resting over the liquid surface.—III. The singular phenomena of the diffusion of gases have been already alluded to under DIFFUSION, and shall be again noticed under OSMOTIC FORCE. Connected also with this subject, are Faraday's curious determinations of the velocities with which different gases—condensed artificially by the same degree of pressure—make their escape through a capillary tube.—IV. Gases likewise vary in their *soniferous* properties, and in the *colour* they give to the *electric spark* when transmitted through them. No physical law has yet been discerned connecting these special phenomena with other qualities of the several gases.—We again refer to the important article VAPOURS. Other special facts and laws regarding the physical attributes of the gases are detailed under COMPRESSIBILITY, CONDUCTION, ELASTICITY, EXPANSION, and HEAT.

Gemini. The twins—Castor and Pollux. The third constellation of the zodiac. It is named from its two brightest stars, α Geminorum or *Castor*, of the first magnitude, and β Geminorum or *Pollux*, of the second. The constellation is near Regulus and Aldebaran. It is marked by the form Π, and extends from 60° to 90° longitude.

Geocentric. The place of a body seen from the earth is termed its *geocentric* place. Since the earth moves, its geocentric place is evidently not an *absolute* place.

Geodesy. The science which measures the Earth itself and great portions of its surface. For the results of the geodetical measurements of the Earth itself, we refer the reader to the article EARTH. We shall only mention here, some of the physical difficulties with which Geodetic science has to contend.—The first is in actual measurements of particular lengths. If rods be employed, they must be evidently all laid in the exact direction of the linear or *base* measure, and must be all exactly together, leaving no interval whatever between. This last difficulty would cause so much risk of error that chains are generally employed. Here, again, each link must be carefully stretched—none catching. But a more material error arises from the dilatation by heat (*Expansion*) or contraction by cold of the measures in question. This causes a difference in the actual lengths of the standard to which we refer quantities, and which, though not very great, must be carefully allowed for. But especially difficult is it, in such investigations, to succeed in getting a perfectly level plane along which the straight line is to be measured. Valleys, and undulations, and streams the most inconsiderable, may prevent this. Hence, measurers take simply one line at first in the most convenient position for its level, and calculate others by it, by trigonometric observations and processes. But there remains the great difficulty of measuring any sufficient base line. And, in taking trigonometrical observations of the bearing of distant objects on the Earth's surface, it will be found that the three angles of any triangle we may form will be greater than 180°. In fact, the triangle is not a *plane* one, which might, without so much difficulty, be solved, as a *spherical* one; and, if we reduce it to the methods of the plane triangle, we have to subtract one-third of their spherical excess, from each of the angles. Then, our observations of angles must be made on objects somewhat elevated. This gives an angle very slightly different from what we should have had, if the object viewed had been in our plane; but so different that the correction corresponding must be calculated and applied. Our observations, though made on the horizon, or reduced to it, in each case, however, are not all, perhaps, made at the same level. Great care must be taken, therefore, in applying the necessary corrections for this error also. Another correction is of still greater importance. Horizontal refraction is a cause of very considerable error, for which due allowance must be made in all observations. The air is denser at the horizon, and therefore there the refraction is of greater amount. Average corrections can be, and are applied for this, but no corrections, in our present position as to meteorological science, can be quite satisfactory, more especially if the horizontal atmosphere is unsettled at the time. It will be most advisable, therefore, always to make observations when the atmosphere is quite undisturbed and has been so for a considerable period. —These remarks will give an idea of the principal difficulties in conducting geodetic measurements. It will be at once evident that the more difficult trigonometry and the delicate applications of the calculus of probabilities will be required for them. —The reader is referred to the great work of Puissant.

Geometry. A science which, as the name implies, originated in efforts to measure portions of the surface of the Earth. It obtained its foundation in Greece; but it was not long ere the singular power of abstraction, which formed so characteristic a feature of the genius of the people of that country, raised it into a general Science, concerned about determining the relations of all portions of *Figurate Magnitude.* Geometry, in this extended sense, is a science of the purest kind. Resting on a few elementary conceptions, whose truth is confirmed either by elementary intuitions or universal experience, it proceeds, by way of *pure deduction*, never again postulating any fact—except the *possibility* of the figures it supposes constituted, and whose properties it undertakes to determine. If the elementary conceptions, at the root of this system of deduction, should be sufficiently comprehensive or exhaustive, it is evident that no flaw need exist in the deductive process; and that the Geometer by the mere exercise of logic must succeed in determining the relations of all the parts of any conceivable *form* or *magnitude* in space. Some critical questions regarding the sufficiency of the con-

ceptions ordinarily received as elementary will be found discussed under PARALLEL LINES.—Geometry has many subdivisions, some originating in the kinds of magnitudes treated, others in peculiarities of the mode in which they are treated. As to the former ground of division, the separation of *Plane* from *Solid* geometry, has obtained universal currency, and will be understood without definition: the distinctions depending on the latter cause, however, require certain elucidations, and are briefly explained in order, in the following articles:—

Geometrical Analysis and Synthesis. The mode in which geometrical demonstration was usually conducted by the ancients, and all pure geometers, is this,—a new truth is deduced or shown to follow necessarily, from combinations of truths already established. Euclid, for instance, makes every proposition depend strictly on preceding propositions; and the truth of that new proposition comes out as the Q. E. D. at the close of every demonstration. This process is the formal *Geometrical Synthesis.* But, although powerful for *demonstration*, this Synthesis was not found fertile as to *invention;* or well calculated to enable the Geometer to find the *mode* of demonstration. And he called to his aid, the much less arduous process of *Analysis.* In this process, the truth to be proved is *assumed as true*, or the construction to be established imagined as *already completed.* Reasoning backwards, from either assumption, any one skilled in such analysis will very soon arrive at some elementary truth, or some simple construction on which his first assumption depends, or which it clearly involves; and thus he obtains a ready guide to the erection of his *Synthesis.* The Greeks made extensive use of the method of discovery by Analysis; and we have in the fragments they have left, many fine examples of it. The student of elementary geometry, ought, on every account, to familiarize himself with this most elegant practice; and he will find no better instructor than in a volume expressly devoted to its exposition by the late Professor Leslie of Edinburgh,—a work, probably the most permanently advantageous—of all the contributions to science—offered by this very ingenious Inquirer.

Geometry, Analytical or Algebraical. The entire structure of this most important mode of contemplating *Figure*, and deducing the properties of Figurate Magnitude, may be said to have been completed by the illustrious Descartes. To define Analytical Geometry in the most general manner, we may say that it is the art of reducing the *quality* of *Figure* within the category of the *quantity* or *Number;* a curve, of whatever complicacy, or any solid—whether with plane or curved surfaces,—may be represented and perfectly defined, in the method of Descartes, by pure Algebraical or Transcendental *Equations.* It is impossible to overrate the impulse given to geometrical research, or the amount of unlooked for power placed in the hands of geometers, by this one stroke of genius: it changed the entire aspect, and indefinitely enlarged the boundaries of Inquiry, whether in Geometry, or in Dynamical Science; and most of the new methods in the Calculus—most of those extensions which, since then, have marked the onward course of pure Analysis, must, in simple justice, be attributed to it, as their exciting cause. The leading features of this remarkable method have been already popularly explained under CO-ORDINATES; and we shall further speak of it, in the way of critical appreciation, under QUATERNIONS. Excellent treatises on Analytical Geometry and every portion of it, are so numerous, that specification were needless. Some of the steps by which it has been advanced will be signalized, in the historical article immediately subjoined.

Geometry, Descriptive. A method in geometry due to the illustrious Monge,—one of the most brilliant of those geniuses which, like a galaxy, adorned the sky of France, in the times of the Revolution and the early Empire. The object of this method is, to represent on two plane surfaces—two rectangular or other planes—the elements and character of any body of three dimensions; in other words, to represent by a plane figure all the elements necessary to define and enable us to describe any figure of three dimensions. Evidently, such a Geometry is only an extension or general application of the principle of *Projections;* and, as such, it must be said to have always existed. We find traces of it among the Ancients; we find it approached almost in due generality by Desargues; and every great architect must, however imperfectly and unconscientiously, have employed it: nevertheless, it was reserved for Monge, to connect this whole order of questions, and base them on a few abstract and elementary operations, as well as to present them in a special treatise, which bestowed on them the character of a *theory* or *doctrine* wholly independent of the specialties that gave rise to former diverse and isolated practices. The practical value of this great step in Science was at once recognized by the French *Convention:* and it cannot be doubted that the great *theoretical* superiority of civil and military engineering across the channel, even at this moment, is owing to an early familiarity with procedures that have yet scarcely found a resting place among the schools of England. Descriptive Geometry, simple and accessible in all its processes, presents a very throng of practical applications. Let us specify—perspective; the construction of reliefs; the determination of shadows; gnomonics; stone-cutting; carpentry; the planning of roads and canals in countries of varied contour; naval constructions; the direction of mines; and much of the science of fortification.—The student can yet obtain no better guide than the work of Monge himself: but we refer him also to the volumes by Le Roy and Olivier. A treatise has recently been published

in this country with special reference to ship-building, by Professor Wooley.

Geometry, the Higher. *Geometrie Superieure.* A series of modern researches, is fully entitled to this name, which—partly by introducing new *formal methods*, partly by realizing the geometrical use and application of *Imaginaries*, partly by discerning the great importance and wide application of a few propositions connected with *Diverging Lines*—has really bestowed on the ordinary geometry, an efficacy in research, which it did not obtain even from Descartes. This Higher Geometry has not hitherto been much cultivated in Great Britain: the genius of Cambridge is still purely analytical. But the successful culture of it, is a distinguishing feature of that remarkable School, of which Trinity College Dublin, is the centre and *Alma Mater;* nor have we, personally, the slightest doubt that the labours of this school, in this and even more arduous ways, is destined to impress a most beneficial influence on all future geometrical inquiry. The student is referred to the Elementary Treatise by Professor Mulcahy, and especially to Mr. Salmon's works on *Conic Sections* and the *Higher Curves.* But the model and satisfactory book, in the meantime, is the volume by Chasles, entitled *Geometrie Superieure.* Brief descriptions of a few of the methods and conceptions at the basis of this new geometry, will be found under INVOLUTION; PENCILS HARMONIC; POLARS; PROJECTIONS; PROPORTION HARMONIC; RATIO ANHARMONIC; and TRANSVERSALS. The explanations given under these notices are of course supposed to be known by the reader of our subsequent slight sketch of the History of Geometry.

Geometry, Symbolical. Under ALGEBRA it was stated that the signification of the usual signs of operation, viz., $+$, $-$, $\times$, and $\div$, although originally representing mere arithmetical action on numbers, need not be limited within the narrow circle placed around them by the circumstances of their origin. On the contrary, they may be taken as the signs of any general set of relations which stand towards each other according to the fundamental or abstract conditions, which, under a special and very limited form, connect these arithmetical signs. Hence the important idea of a *calculus of operations:* hence also the farther and eminently prolific idea that, if the fundamental relations of any science can be placed within the category of relations represented by $+$, $-$, $\times$, and $\div$, there is at once accessible for the advancement of that science or branch of inquiry, the entire mass of subtle methods hitherto elaborated for the service of pure analysis alone. Symbolical Geometry means, at its root, the geometrical interpretation and application of those originally arithmetical signs. The student will easiest be introduced to its nature, by the study of parts of Professor De Morgan's work on Trigonometry: but it were gross injustice to withhold the tribute from Sir William Hamilton of Dublin, that in a succession of papers, in various scientific journals, he first of all, and most thoroughly of all, has shown the resources of this method of applying algebraical signs. Sir William Hamilton has other and far higher claims on the respect of this age; he is sure of the respect of the future. The Founder of a new Geometry, the originator of a mode of reducing Form under the category of Quantity, simpler in reality than what is contained in the conception of Descartes, and far more fertile and powerful—may not consider the subject of Symbolical Geometry as cursorily sketched by him, worthy of that continuous attention which all other Geometers desire it should obtain at his hands: nevertheless, no intelligent student of the Essays alluded to, will refrain from offering him his earnest thanks. They belong to those few things in the mathematics of our present, rather formal, day, which indicate the certain advances and potency of the future.

Geometry, History of. The History of *Geometrical Science*—using that term in its widest sense—having a much greater *positive* value than the history of almost any other branch of *Physics* or *Mathematics*, it has been considered expedient to depart, to some extent, from the general plan of this volume, and to make it the substance of a separate and detailed article. The subject naturally divides itself into *three* heads:—the History of Geometry among the Ancients; the History of Geometry during the Middle Ages, and up to the commencement of the Nineteenth Century; and its History during what has passed of the Nineteenth Century. This arrangement is commended by the distinctive nature of the progress, which has signalized these three important periods.

(1.) *History of Geometry among the Ancients.*—As already briefly intimated, the Greeks had perfected both methods of geometrical investigation (see GEOMETRICAL ANALYSIS AND SYNTHESIS)—that method by Synthesis, or the way of advancing from known to unknown truths which is so finely exemplified throughout the Elements of Euclid; and the method of Geometrical Analysis—illustrated also by Euclid in the artifice of the *reductio ad absurdum*—which proceeds by assuming as true, the proposition about to be demonstrated, or the proposed problem as already resolved, and descending from the hypothesis, by clear logical steps, until some known truth is reached, or some elementary and possible construction obtained.—The Greeks, in full possession of these two powerful methods, and aided by that singular subtlety of intellect and fondness for speculation, which so eminently belonged to them, appear to have succeeded in reaching a grand and symmetrical fabric of Geometrical truth, which certainly may contest the palm with the achievements of any age, and whose positive value has only been surpassed by the acquisitions of our own. Unfortunately—through the loss of many

of their most important treatises—we are reduced, in frequent cases, to guess merely as to the contents of these elaborate works; and our only clue is found in the fragmentary and often enigmatical *Collections* of Pappus,—an author whose writings assuredly demand from Scholars a complete and critical edition—the more especially, as invaluable materials for it exist, under the hand of Robert Simson, within the Library of the University of Glasgow. There were three divisions of geometrical science in those days:—the *first*, that of the *Elements*, now represented by the well known Euclid; the *second*, occupied with Practical Geometry or *Geodesy;* and the *third*, that virtual parent of our own *Higher Geometry*, wherein all investigations were mainly conducted by Analysis. According to Pappus, the treatises forming this Higher Geometry, were as follows: —I. The *Data* of Euclid. II. The *Section of Ratios* by Apollonius. III. Two books of the *Section of Space;* two of the *Determinate Section;* and two concerning *Contacts* or *Tangencies*, also by Apollonius. IV. Three books of *Porisms* by *Euclid.* V. Two books on *Inclination;* two on *Loci Plani*, and eight on *Conic Sections*, again by Apollonius. VI. Two books on *Solid Loci*, by the elder Aristæus. VII. Two books on *Loci on Surfaces* (probably *curves of double curvature*), by the fertile Euclid. And, VIII. Two books on *Mean Ratios*, by Eratosthenes.—But of all these treatises, three only have reached us; viz., the *Data* of Euclid, the seven first books of the *Conics* of Apollonius, and an Arabic translation of the *Section of Ratios*, by the same great geometer. Several other treatises have been attempted to be restored, in accordance with the hints of Pappus; none more ingeniously than that of *Porisms*, by Robert Simson, to which, because of its rare importance, we have devoted a separate article (Porism). Among those works which have escaped the moths of ages, we must, of course, reckon among the greatest, the works of Archimedes, rich as they are in geometrical truths, and in the suggestion of methods; nor ought the historian to overlook the *Almagest* of Ptolemy, a treatise evincing ingenuity, and offering instruction in every chapter, but, unfortunately, occupied with the exposition of a false and cumbrous astronomy, and, therefore, seldom studied, unless by the Antiquarian.—If one would estimate aright the permanent scientific value of this Ancient or Greek Geometry, a distinction must be carefully made. That Geometry had *two* purposes:—One, to Measure magnitudes, linear, plane, or solid; *i.e.* to compute how often a certain *unit* of linear, superficial, or solid magnitude is repeated or contained in any *line* right or curved; in any *superficies*, rectilineal, or curvilineal; or in any *solid*, whatever the character of the surfaces by which it is bounded. Within the sphere of this portion of geometrical science, the usefulness of the labours of the ancients may be said to have long terminated; only, however, because the methods invented and employed by Euclid and Archimedes have been perfected. The transition from the measurement of *rectilineal* quantities to the measurement of *curvilineal*, is, in a purely geometrical sense, the object of the Infinitesimal Calculus; but, although we have so far improved our algorithm, that a reversion to the ancient methods would, in any case be considered only a wasteful retrograding, it must never be forgotten, that the true logical foundations of the Modern Calculus; viz., the method of *Limits*, or of *Prime and Ultimate Ratios*, must be traced to the Greek method of *Exhaustions.* In this important respect, while the ingenious Cavalieri preceded Leibnitz, the far more powerful spirit of Archimedes was the worthy precursor of Newton. But it is surely something, that the practical value of these early logical methods, terminated only at an epoch so recent, as that of the most illustrious of our modern geometers.—The Second great division of the Greek Geometry, consisted in the effort to discover the *relations* of the parts of figurate magnitude,—to determine, in fact, and define, the various attributes of Form. Success in the former effort necessarily involved great advancement and success in this one; but the Greek Higher Geometry had evidently advanced so far in the latter class of investigations, that the most accomplished modern inquirer in this special walk, would assuredly welcome, as a great probable aid, the recovery of any of the lost treatises whose titles have been transmitted by Pappus. In truth, the significance of many of the enunciations in Pappus, have only been divined, through aid of geometrical discoveries that contribute to the scientific glory of the nineteenth century;—the germs of speculations are met there, that might have emanated from the schools of Carnot and Monge. That auspicious re-opening of the East, which may fairly be looked for, as one sure result of our recent struggle with Muscovy, will probably reveal some of the riches of these ancient times, through medium of Arabic manuscripts, now rotting in what they term 'libraries,' in those useless and already rotted monasteries of the Eastern Church.

(2.) *History of Mediæval Geometry, and of Discovery down to the close of the Eighteenth Century.*—The period to which reference is now to be made, was distinguished by three grand achievements. Multitudes of potent geometers illustrated these centuries, and every one of the steps now to be commemorated, required the co-operation of many minds to consummate it; but we cannot, in this place, refer to special labours, nor attempt to determine the amount of desert belonging to the concurrents, in each of these momentous discoveries. The points, on which the student of history will always rest, are the following.—I. In the hands of Vieta, first arose the invaluable science of *Symbols. Arithmetical* Algebra existed, previous to the labours of the

illustrious Frenchman: to *him* is unquestionably owing the idea of a science of abstract symbols, and the conception that such symbols might be subjected to all the operations, by which numbers alone had been previously combined, or to operations similar to these. From the moment of this happy discovery, the possibilities of a new method of treatment in geometry arose. Linear and angular functions might, like every other species of quantity, be represented by abstract symbols, and their relations laid down in equations. Hence, too, the modern vicious meaning imposed on the term *analysis*. Analytic reasoning being conducted most easily by aid of this general symbolic language, mere *algebraic* geometry came to be termed *Analysis*, as if by special right; while the former Greek method retained the name of *Synthesis*. A mistake of far larger reach than a mere misapplication of words; and which it behoves the accurate student very carefully to correct. The term *analysis* ought ever to be applied to that mental operation which resolves or decomposes a proposition into its elementary and constituent parts—an operation implying an attentive scrutiny of the *thing* considered—*in itself*, and in all *its properties*. Or to render the point perfectly clear, let us add the words of Poinsot:—" True *analysis* consists in the attentive examination of the problem to be resolved; and in those first reasonings by which we enable ourselves to express it in the form of equations. To transform these equations afterwards; that is to say, to combine them with each other, or with other evident ones, is, at the root, a simple *Synthesis;* unless, indeed, the idea of any transformation be suggested by some new mental act, or some new and original reasoning, which, of course, would be a new act of Analysis. Apart from this light-giving procedure, there is no Analysis; but only an obscure Synthesis of algebraic formulæ, which—so to speak—we lay, the one over the other, without seeing very clearly what may come out of the constitution." It has been deemed the more needful to make this distinction, because the discovery by Vieta, and his firm establishment of a *special logistic*, has been represented as the discovery of a substitute for those fine and subtle plays of the spirit, that so delight every intellect versant with the labours of the illustrious ancient geometers.—II. By the Greeks, for the most part, problems were viewed as *individuals*, and treated as such. As time proceeded, advances were made, with the important aim of treating them in *species*, or even *genera;* that is to say, of detecting general solutions which should be applicable to large masses of geometrical *magnitude and forms*. It were heavy injustice to omit the name of Desargues, a geometer of Lyons, who laboured in this direction, with success so remarkable, that, in the opinion of Poncelet, he merits the appellation of the Monge of his age; —unquestionably the germs of much that distinguishes the Higher Geometry of our own time, are found in his work on Conics. But the glory of establishing a truly universal method, is due to Descartes, who, grasping the algorithm of Vieta, founded that Analytical Geometry which, ever since his period, has been so powerful an engine in the hands of every inquirer. Already we have explained his method of Co-ordinates, and briefly appreciated its resources; nor shall we here resume the subject, or more than indicate the amount, which it owed for the promotion of its success and acceptance, to the subsequent labours of Clairaut and Cramer. One remark, however, is essential. By the Cartesian method, problems regarding form are immediately thrown into *equations;* and the subsequent treatment is mainly that *Synthesis*, which Poinsot, in the words already quoted, has aptly termed a mere superposition of equations. Notwithstanding the amazing facility afforded by the use of this method, it becomes therefore a question, whether its constant and inordinate use, has not deprived the study of Mathematical Science of one of its inestimable benefits—its power to encourage original thought—to exercise the best of our mental powers? History, it is to be feared, will not pronounce favourably concerning it, in this respect; nor is better proof needed than the frequent incapacity of mere analysts, to grasp thoroughly a solid geometrical or dynamical truth, or the mass of merely fantastic writing on real Physics which one finds among the volumes of a Euler, or if one's reverence would permit one to say it, even in some pages of Laplace. Happily, a reaction is occurring. Men of the age of which we are writing—men like Desargues, like Halley, like our Scottish Maclaurin—had scattered through their writings good general principles of another order, by which geometry could be treated generally without ceasing to be Geometry; and these, fertilized by the genius of Carnot, Monge, Poncelet, Hamilton, and several more, have sprung up into what is already the goodly and more healthful growth of our Modern Geometry.—III. The third grand achievement of the age, whose services we are recording, has already been discussed and explained in this Dictionary (see CALCULUS, &c.) It consisted in the perfecting, by Newton and Leibnitz, of the ancient method of Exhaustions, and the Method of Indivisibles, by Cavalieri. There is no need, again, to speak of the origin of that Calculus, or of its vast efficacy and range. All difficulties in the way of the transition from consideration of rectilineal to that of curvilineal forms, it has extinguished for ever; and it grasps, with every ease, the whole extent of the problem as to the measure of magnitudes. But the student who is not a mere tradesman, should not be satisfied with knowing this Calculus, as it now exists, or using it as an Art. Let him study it as it grew,—from its first and obscure vitality, in the writings of Fermat, of Pascal, and Barrow, to its living form, as first wielded by Newton, by Leibnitz, by the Ber-

nouillis, and their illustrious companions. This Calculus, in its applications to Geometry (with which alone we have at present to do), has had much of its form and specific methods impressed on it, by the universal use of the Geometry of Descartes: but its foundations are firm and independent, and new modes of applying it will arise, should the requirements of another Geometry demand them.

(3.) *History of the Geometry of the Nineteenth Century.*—The works that led the way towards those researches which now constitute our Higher Geometry, are unquestionably those of Monge and Carnot. Of the volume by Monge, entitled *Application de l'Analyse a la Geometrie*, we shall not speak. It is a virtual continuation of the Geometry of Descartes—reposing on the analysis of Infinitesimals, as the work of the former does on that of finite quantities. Monge, as usual with him, handles his subject originally—bestowing upon it every conceivable extension; and the relations he establishes, between equations of partial differences and certain classes of surfaces, offer fine examples of the reciprocal aid and illustration which Geometry and Analysis always afford to each other. Already full reference has been made to the far more distinguished achievement of this great Frenchman, as constituted by his *Geometrie Descriptive* (see GEOMETRY DESCRIPTIVE); but it ought to be especially mentioned, that, besides its merit as the Elements of a new form of Geometrical Science, it contains, richly strewn among its pages, hints and speculations, which have amply fulfilled the purpose of their Author, by stirring up inquirers interested in the large domain of general Geometry. To Carnot we must refer in greater detail. His labours had for their special object the extension of the Geometry of the Ancients. Their elements lie in the labours of Robert Simson, of Maclaurin, of M. Stewart; but they are stamped with that spirit of generality which one finds previously, in the writings of Desargues and Pascal alone. Carnot's first contribution to general geometry is what he termed the *Principle of the Correlation of Figures;* and which has since been farther generalized under the name of the *Principle of Continuity.* At all former times, it was considered necessary by pure Geometers, to offer as many demonstrations of a proposition as the *form* to which it related, could take on different aspects; Carnot proved that one demonstration applied to a sufficiently general state or condition of the figure, was universally applicable; and could be realized for every specific state, by a right use and interpretation of the positive and negative symbol, in Geometry. It would scarcely be conceived, how vast an amount of generality, ease, and vigour, this simple, but energetic conception, has infused into Geometry! Carnot's second achievement was his *Theory of Transversals*—also, a *method*, much more than a collection of Propositions. The tract in which he exposed it, consists of a set of theorems, expressing the relations among the segments of a system of lines, cut by any *transversal* straight line or curve. He shows that these relations, which are usually expressed by equations of two terms—are the same in *projections* of the figure, whether perspective or orthogonal; and that the same relations hold, among the sines of the angles of the projecting straight lines. Traces of the Theory of Transversals are found in Pappus; but among the moderns, Carnot had the honour of reviving it, and of showing its immense power in simplifying extensive classes of demonstrations. As given by this great Geometer, the Theory of Transversals is the foundation of the remarkable *Traite des Proprietes Projectives*, by M. Poncelet, in which we find, for the first time, that marvellous *Theorie des Polaires*—a method which, by affording the means of inventing at will, and in a way almost mechanical, theorems exceedingly diverse, yet all springing from one root, constitutes one of the most fertile resources of the Modern Geometry. It were vain to endeavour here, to characterize, or even enumerate, the many labourers who have recently wrought with effect in this comparatively new and fertile field. In Germany, in France, in Italy, men of highest ability have been attracted by its promise: nor must we forget our own Dublin, or the contributions of a Maccullagh—above all, of our Hamilton, and the admirable writings of Mr. Salmon. What has been done, however, is yet only a promise, or a bud; nevertheless, the nature of the fruit is clear: what Poinsot has done for Statics, is being accomplished for Geometry;—we shall no longer be satisfied to reach results by a sort of legerdemain, but through processes, of which every step has a clear significance, in which *things* can never be forgotten or obliterated by their symbols, and whose development therefore, must keep distinct before the eye, that chain of real *cause* and *effect* which exists in Nature itself. It is not to be doubted, that, under such conditions, Geometry might become, as it was of old, a high mental discipline. Emancipated alike from its ancient elements and mediæval symbolism, no study could be a greater favourite with the young and aspiring; nor may it be presumptuous to hope, that the great Universities of England, in their efforts to regenerate their intellectual discipline, will soon inquire, whether, in *this* matter of their teaching, there is not much to regenerate.—The student is earnestly recommended to the recent most important volume by M. Chasles, entitled *Geometrie Superieure.* Separate articles in the Dictionary have already been referred to.

Georgium Sidus. See URANUS.

Geysers. Hot springs in Iceland, which burst up from the Earth, carrying masses of stone along with them. Their origin is evidently volcanic. The water at the surface of the great spring in Iceland is 85° Cent., and at the bottom

as high as 127°·5. The water at the bottom is hottest *before*, and coldest *after* a great eruption, but that at the top it seems very little, if at all, affected by it. A geological explanation of this is given by Lyell in his *Principles of Geology*, to which we refer the reader. Herschel presents the phenomenon in a sufficiently suggestive way. Take a tobacco-pipe and heat the shank red hot; then fill the bowl with cold water—inclining the pipe a little. The water will flow into the shank, a series of violent explosions will result, in which, as in the actual case of the Geysers, a gush of vapour is always projected before the jet of water. The interval between the explosions seems to depend on the degree of heat and the amount of inclination of the tube;—their duration, on its thickness, and its conducting power.

Gibbous. Convex on both sides, as the moon appears when more than half advanced.

Glaciers. Immense masses of ice which fill the slopes of Mount Blanc, and the Alpine Range of the Pyrenees, the Balkan, and every mountain chain over the world, which rise so high, as to pierce the limit of perpetual snows. They are thick heavy layers, frequently very irregular, and sometimes extending far down into the valleys below the limit of snows. The explanations which geologists offer as to their causes are of great interest; but we refer the reader to Agassiz and Charpentier's description of them, and Forbes' *Travels in Norway and Sweden.* No natural phenomenon is more grand than the spectacle of these enormous masses. In the Alpine chain, from Mount Blanc to the Tyrol, about 400 of them are counted, and these are on the average from 10 to 15 miles long, from one to two broad, and from 100 to 600 feet thick. This will give some idea of their enormous bulk, and of the great part which they must play in the physical history of the Earth.*

Glaisher's Factors. Mr. Glaisher, the eminent observer at Greenwich, has investigated with much care the corrections to be applied to the monthly means of the Barometer, Thermometer, and Hygrometer, for *any hour* of the day, so that the monthly mean *of the day* be therefrom obtained. These corrections are called Glaisher's Factors. They are of great value to the meteorologist, and are presented among the tables appended to this volume.

Gnomon. A geometrical term. When a rectangle is divided into four parts by cross lines parallel to its sides, the sum of any three is called the *gnomon.* It has also a meaning in DIALLING (*q. v.*)

Golden Number. See CYCLE—where the Metonic Cycle is described.

Goniometer. An instrument employed to measure the angles of crystals. The simplest form consists merely of a semicircular graduated scale of degrees with a moveable and a fixed radius. The one edge of the crystal is laid along the fixed line, and the other brought into contact with the moveable one, and the number of degrees indicated read off. This instrument does not admit of much accuracy. In Wollaston's goniometer, which is used for all purposes of accuracy, the angle of the crystal is measured by determining through what angular space the crystal must be turned, so that two rays, reflected from the two surfaces successively, shall have exactly the same direction.

Governor. An ingenious mechanical arrangement by which regularity in the motion of a steam engine is secured. When new fire has just been put on, more steam is apt to be generated than the engine, in its ordinary state, can use; and if free communication between the boiler and cylinder be permitted, more will be generated. To prevent this, two balls are set upon a cylinder which revolves with the engine, and these tend to revolve faster, the faster the engine goes. When it is going very slowly, they exert a certain action on a moveable part to which they are attached, so as to keep open a valve between the boiler and cylinder; when it is going very quick, the balls fly fast, and, being connected with the valve, tend to close it, proportionally as they have diverged from the spindle. The steam has thus less outlet from the boiler, and is held in, until the engine's requirements and the supply become equalized: the balls then fall to an average position.

Graduation. See DIVISION.

Graphic Methods. It is often of the utmost importance to represent tables of *related numbers*, by means of curve lines, or other figures that show *to the eye* the nature of the relations or laws expressed or rather concealed within the mass of numbers constituting the Tables. Not only does such a mode of representation at once manifest these laws—almost rendering them palpable—but it further points out, by the irregularity of its curves, in what cases natural laws are not represented, and therefore what the cases are that require a greater amount of observation. We shall state briefly in what manner this graphic mode of representation may, in the most usual classes of circumstances, be made effective.

(1.) *When the tables contain the relations of two co-variable quantities.*—It is easy enough to see, that in such a case, the end may be effected by making one of the sets of quantities the *obscissæ*, while the others are corresponding *ordinates* of a curve. Thus, if the *hours* of the day be made *obscissæ*, and the *temperatures* corresponding with them, *ordinates*, the line joining the tops of the ordinates will give the curve of diurnal temperature. And, according as this curve—depending, in the main, on the course of the sun—is regular or not, we shall learn whether, and at what hours, our collected observations have been sufficient or insufficient for the determination of the mean temperature of every hour of the day, at the place. Nay, the curve being continuous, we

might then find by simple construction—provided the figure had been drawn on a sufficiently large scale—what is the mean temperature for any intermediate time: if, for the half-hours for instance, simply by erecting perpendiculars or *ordinates* half way between each former ordinate, and accurately measuring them. In the same way could be treated the relation between the epoch of the year and the temperature of any given time of the day; the relation between the heights of the barometer, and the time of the day or of the year, or any other two interdependent variables. The method has especial application to statistical inquiries; and the great facility it furnishes for the determination of intermediate and mean values, gives it in this case remarkable value. There is no simpler or surer mode of extracting from tables of mortality such elements as the probable life, the mean life, and the mean age of the population. Innumerable examples of such happy applications of it will be found in the writings of Quetelet.—There is, however, a second mode of executing similar representations, which may be termed the *polar mode.* This is the preferable one, when one of the quantities has to do with *direction;*—for instance, should one of them be the direction of the wind, and the other, the corresponding mean height of the thermometer, barometer, or hygrometer. Let us take as an example, from the work of Mahlmann, the variations of temperatures during the four seasons, according to the different winds at Paris: it gives, what may be termed the thermometric card of the winds for the year, and for each season—A being the curve of the mean of the year, A′ that of autumn, S that of spring, S′ that of summer, and W that of winter. The student will be at no loss to discover how fertile such a representation must be of

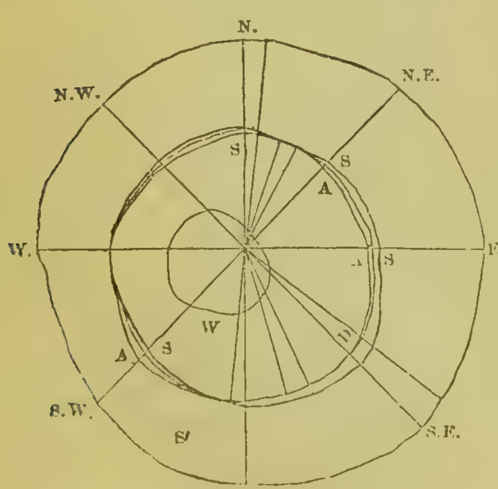


Fig. 1.

matter for thought and speculation. The contrast between the summer and winter curves is especially instructive. See HYGROMETRY. This polar method has manifold advantages in all such cases.—As already hinted, both methods afford ready means for *interpolation;* and even where irregularities exist, the mean or true curve may be had, by approximation, by judiciously drawing it through the sphere of the conflicting observations, so that it represent the centre of gravity of the irregular system they form. By this latter means Sir John Herschel first approximated to the orbit of one of the *Double Stars.*

(2.) *When relations are expressed among three variables.*— The mode of representation is, of course, much more complicated than the former; nevertheless it has also great value. It will best be understood by an example. Suppose it were required to represent to the eye, the curved surfaces expressing the mean temperature of any hour of the day on any month of the year. Set off on the axis of abscissæ twelve equal divisions representing the months, starting, let us say, with March: set off next, on the axis of the ordinates, twenty-four equal divisions representing the hours of the day; and through the several divisions of these two sets draw perpendiculars, as in the subjoined figure. The plane of the

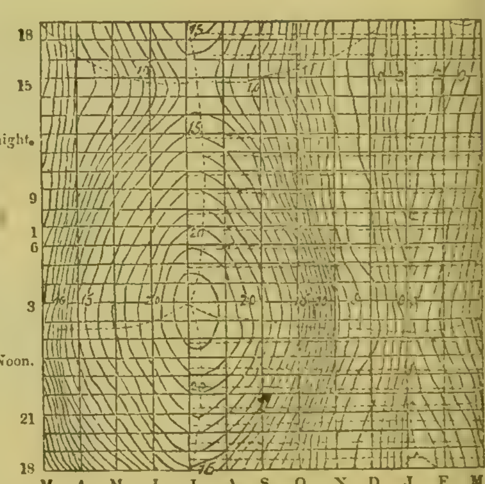


Fig. 2.

figure thus prepared, we proceed to selec[t] any temperature—say 20° Centigrade; mar[k] the points corresponding to it in tempera[-]ture, at the intersection of the abscissæ and ordi[-]nates of every month that contains a temperatur[e] of 20°; join these points, and you have the curv[e] of equal temperature for 20°, represented on th[e] face of the plane. All other temperatures withi[n] the range of the year may thus also be repre[-]sented, as in the diagram, by their fitting curve[s]. We have no space to dilate on the results tha[t] may be deduced at once from such a picture.—The method now exemplified, is precisely that b[y] which lines of equal elevation, or *contour line[s]* are represented on all good engineering maps and its applications have a wide range. Fo[r] further and very minute information on this mo[de]

important practical subject, the reader is referred to a dissertation by M. Lalaune, printed as an appendix to Mr. Walker's excellent translation of Kæmtz's *Meteorology*. And if he desires to pursue the subject farther, we commend next the valuable *Calcul par le Trait*, by M. Cousinery.

Gravitation. The great theme of Gravitation naturally arranges itself under three divisions:—the discovery of the Law; the sphere it dominates,—this latter subdividing itself into consideration of planetary and extra planetary spheres; and the nature of the Law. We shall discuss the subject, as briefly as possible, under these heads.

(1.) *Discovery of the Law of Gravitation.*—The essential preliminaries to the establishment of any Law of the Celestial Motions was, of course, the discovery of the character of these motions. The plan of our Solar System was laid down by Copernicus; but its definition is, unquestionably, due to John Kepler. The three grand Laws or principles of Order discovered by him, had the merit, beyond all doubt, of stating, for the first time, the rules according to which the planetary bodies move. He informed us *first*, that every planet revolves around the Sun in an *Ellipse*,—the Sun being in the focus of that Ellipse: *secondly*, that the velocities of these planets, at different parts of their orbits, are regulated by the principle of Areas; or, that the *radius vector* sweeps over equal Areas in Equal Times: and *thirdly*, he connected the distances of the different planets from the Sun with their times of revolution, by his last great Law—that the *squares* of the times occupied by the several planets in their revolutions in their elliptic orbits, are proportional to the *cubes* of their mean distances from the common focus of these ellipses, or from the Sun.—To reduce these Laws of Kepler into one simple expression, could not be other than a grand step in philosophy: and, such a reduction, would seem the grander an advance, if, when accomplished, it was found to comprehend and explain phenomena never known to Kepler. Previous to Newton's period, the best cultivators of physics had gradually been growing accustomed to the fundamental idea of Gravity. Whatever that force is, they saw it acting; and Galileo had determined the laws of its action on falling bodies, or on bodies near the surface of the Earth. Long afterwards, the more distinct idea of *central forces* arose, or of forces drawing bodies towards certain immovable points, or towards the centres of other masses. General propositions, of incalculable value, had even been enunciated on this subject; with regard to the more general conditions of circular motion, by the acute and fertile Dutchman, Huyghens; and our own Robert Hooke,—whose rare ingenuity has been undervalued, partly because of his restless activity—unquestionably shadowed out the probability that, through such considerations, the world would find a comparatively plain solution of all the celestial motions. But, to accomplish and perfect such a solution, was reserved for Sir Isaac Newton. Earnest and most clear in judgment, he rarely gave way to mere hypotheses; and he ended, by consolidating Kepler's theorems into that general Law of the Solar System, which succeeding experience has almost entitled us to designate a fundamental Law of the whole material changes of the Universe. The steps of Newton's first deduction were as follows:—From Kepler's second Law; viz., the Law of the Areas, he drew the conclusion, that every planet is retained in its orbit by a central or attractive force, directed towards the centre of the Sun, or rather, towards the focus of the Ellipse:—from the fact that the planetary orbits are *elliptical*, he inferred, that, with regard to each orb, this central force varies in intensity according to the inverse square of the body's distance from the focal point: and Kepler's third Law, led him to the important conclusion, that this Central Force is homogeneous throughout our whole system; in other words, that the force which retains Jupiter in its orbit, is precisely that which retains the Earth, only diminished in energy, in the ratio of the squares of their respective distances from the Sun.—It is easy to see with how great a light, a discovery of this kind illuminated the whole Order of the Solar System: nevertheless, in so far as we have now stated it, the Law of Gravitation was but a combination, or contraction into one, of the three general principles of Kepler. But Newton now advanced alone. Having discerned that the Satellites must be retained around their Primaries by a principle exactly similar, he put the novel and startling question, Is not the force with which the Moon gravitates to the Earth, precisely that with which a heavy body falls towards the Earth, at its own surface? Exact calculation confirmed the conjecture; and our great Inquirer instantly discerned that the Planetary force, whose character he had been tracing, was not merely a *planetary* one; but only a manifestation within planetary regions, and with regard to the planetary masses, of an energy which every atom of matter in the Universe most probably obeys. So that Gravitation must now be described, even with this grand generality:—*Every two portions of matter within the Universe attract each other, with a force proportional, directly to the quantity of matter they contain, and to the inverse square of their distances.*—Such the *Law* of Universal Gravitation.

(2.) *The Sphere of the Dominion of Gravitation.*—As we have now explained it, that sphere is universal; and the agency of Gravitation has been already traced as far as accurate observation has carried us.—I. The conception of a mutual action among the *planetary* bodies, necessarily involved the fallacy of Kepler's Laws—as *perfect* representations of the truth. Pure elliptic motion could consist only with the action of a central body over the orbs environing it,—not with the

superadded fact, that these orbs act likewise on each other. Now, in the case of the Moon, it was known before, that neither pure elliptic motion, nor the Law of the Areas, could be predicated. That Body is subject to many *inequalities*, or *equations*, as they are termed. Newton's genius quickly traced the origin of these: his work on the *Lunar Theory* (see LUNAR THEORY) was one of his most remarkable achievements; nor did any part of his immortal *Principia* contribute more conclusively to the establishment of its great doctrine. These investigations form the basis of the celebrated *problem* of the *Three Bodies*. But perturbations or inequalities, are not confined to the Moon. Every planet must be drawn from its rigorous elliptic path by the sympathies of the others. To trace these perturbations, to detect their sources and nature, to determine their amount, and to estimate their import as concerns the stability of the Solar System, has, indeed, been the great task of Physical Astronomy from Newton's time until now. Some of the features of this arduous enterprise, and something of the measure of our success will be found by the Student under SOLAR SYSTEM and NEPTUNE. Nor have the triumphs of Gravitation been all gained on so wide a field. The little, as well as the great, is comprehended and determined by it. It explains the *Tides;* it determines the *Figure* of the Planets; it originates and governs all such motions as those of the PRECESSION OF THE EQUINOXES, and NUTATION OF THE AXIS OF THE EARTH (*q.v.*)—II. But we follow this grand Law as our guide through spaces transcending infinitely the limits of the Solar System, and the fates and nature of its small orbs. On—towards distances next to inconceivable—the eye of Reason can, by its aid, pursue the wanderings of the Comets: we detect the same controlling Law in the revolutions of the DOUBLE STARS, and likewise, in all probability, in those more massive arrangements of myriads of nocturnal luminaries, which are now engaging the contemplation, and arousing the hopes of a new age. Not unreasonable, then, is it to conclude, that the illustrious Englishman had the rare, if not the absolutely singular felicity, to reach an ultimate and universal principle of material Nature.

(3.) *The Nature of the Law of Gravitation.*—Speculation could not fail to arise, and to expend itself on impracticable inquiries, as to the intimate nature of this force of Gravitation: even the acute Laplace, deemed that he could show the *necessity* of the rule of the inverse squares of the distances. Light, he said, and everything radiating from a point, diminishes in intensity—*through an absolute geometrical necessity*—according to this Law. He forgot that he had present to his mind, Light, as a material emanation or radiation, proceeding along straight lines; and that this subsumption had no visible or conceivable connection with the law of the increase or diminution of the Energy of a Force. That the rule of the inverse squares has some explanation, is unquestionably true; but we shall not discover that, by *à priori* reasons: it may become clear, so soon as inductive Inquiry has determined the exact correlation of Gravity and other Material Forces. This correlation is the problem of our highest science: it cannot be detected by hypotheses, nor shall we advance one step towards its solution, through the assumption of infinities of *Ethers*. Neither let it be forgotten, that—when our loftiest attainable generalization shall have been reached, —LAW—in its highest majesty, its ultimate purity and nakedness—can only be apprehended by the human mind, as the immediate will or direct ordinance of the Infinite Spirit, whose essence is partially revealed by that brilliant creation—"the work not made with hands."

Gravity. A term usually applied to the terrestrial action of Gravitation. See ACCELERATED MOTION and EARTH.

Gravity, Centre of. See CENTRE OF GRAVITY.

Gravity, Specific. See SPECIFIC GRAVITY.

Gregorian Calendar. See CALENDAR and BISSEXTILE.

Gunnery. In this article, we shall notice such of the *practical* details of the science of gunnery, as our space can enable us to find room for:—the laws of PROJECTILES, upon which the science founds, being purely mathematical and mechanical, will be treated separately under that head.—And first, of the *powder;* that which gives it its power, is its capacity for decomposition under combustion. Gaseous products are suddenly evolved from the solid grains, and these taking up space much larger than that allowed them in the chamber where the powder is contained, tear away or drive out that part of it, the ball, where there is least resistance to expansion. Powder may be inflamed either by an electric spark, an ignited body, or a sudden heating up to a temperature of about 600° Fahr. We say a sudden heating, because a slow heating would not ignite it; it would, instead, decompose it gradually, first subliming the sulphur, and leaving what is not, in fact, powder at all. A sudden stroke is quite capable of producing such a heat: thus, iron struck against iron, copper against copper, copper against iron, lead against lead, and even lead against wood, will generate heat enough to kindle powder. It is deserving of notice that the heat of ignition of the powder communicates to the ball and expands it. For this reason, the bullet must always be something less in diameter than the gun barrel. The care with which it should be handled, is the most immediate consequence of this. At first, the powder was used in the state of fine dust, but it was found that this produced, on the side of the gun-barrel a moist scum, which prevented it from falling down readily to the bottom. It has been since used in grains; and it is found that two parts of

in that state, produce as much effect, as three in the state of dust. In fact, the grains leave interstices into which the inflamed gases immediately pass, so as to communicate the heat freely and at once; and in this way, the whole charge explodes nearly at one and the same time. In considering of the rapidity of explosion, then, we have to think of the *velocity of inflammation*, that is, the speed with which, when one grain is inflamed, the combustion spreads; and the *velocity of combustion*, that is, the velocity with which a single grain burns from surface to centre. The first is, with grain powder, very great, at least eleven or twelve yards per second. It is noticeable that this diminishes, if the magnitude of the grains increases—if the charcoal be red instead of black, or if the grains be round instead of angular. The cause of the latter of these results is, that angular grains may get gathered or wedged into a mass, just as if the powder were in the form of dust. The method which M. Probert suggests to prevent explosions, depends on the same principle. It consists merely in mixing up the powder with fine charcoal, from which it may be again separated by sifting, when wanted for use. The *velocity of combustion* is such, that a single grain consumes away from surface to centre at the rate of about ·49 of an inch per second. Now, the initial velocity of a bullet, in any case may be known, and the time which it will take to traverse the gun-barrel, in consequence; it is manifest, then, that in order that the powder should produce its total effect, it should all be consumed when the bullet is leaving the gun, seeing that all gases developed afterwards, cannot affect it. It never is perfectly so. It is presumed that eight ounces of ordinary powder may be consumed in about $\frac{1}{250}$th part of a second. The length of the charge which produces the maximum velocity may be stated, in general, as about $\frac{1}{3}$ that of the bore. There is little advantage, however, in increasing the charge to that which will produce maximum velocity, because the resistance of the air increases in a much larger ratio than the velocity does. It is this atmospheric resistance which prevents the projectile from moving in a parabola, or in any curve equally inclined at its points of section, to one of its chords. Its amount is in proportion to the density and radius of the bullet, and, on an average, to the square of the velocity.—As we have already noted, the greater the grains are, the longer time will the powder take to consume. Thus, powder which will not drive the bullet out at all, will do so, when it is divided into grains, seven or eight times less. There are other combustibles, again, which burn so fast, that the gun would be likely to explode altogether. This prevents the use of gun-cotton, and of the chlorites and nitrates in general. Some explosive materials, again, which are not so violent, but explode more rapidly than powder, would leave slight residues, which would attack the gun-barrel and slowly corrode it, thus rendering the gun only safe for a limited period, depending for its strength on the care with which the barrel may be cleaned after firing. In powder itself the proportions of the ingredients exercise considerable effect on the speed of combustion. Thus, in order that this be the greatest possible, the proportion of sulphur should be between 8·5 and ·12·5 per cent. The less there is—the sulphur remaining the same—the slower is the combustion. Powder, when burnt, gives two different products, solid and gaseous. The former consists of sulphate of potash, subcarbonate of potash, and charcoal and sulphur, which come off solid, undecomposed, along with these. The gaseous products are—carbonic acid, nitrogen, carburetted hydrogen, sulphuretted hydrogen, and nitrous gas. The solid products are also volatilized at the moment of explosion by the great heat, so that good powder leaves no stain on paper on which it is fired. It is impossible, theoretically, to discover the expansive force of a given powder, because we have not merely to deal with permanent gases but with vapours, on which the effects of high temperatures are very marked,—the slightest difference being indicated by great differences of expansive force. Various experiments indicate the expansive force to be between 25,000 and 32,000 atmospheres. As for the proportion of the work spent by the powder upon the bullet and on the recoil of the musket, we have, neglecting the allowance for the friction of the ball, and in the case of ordnance, of the gun-carriage, and supposing the bullet to fit tight (that is, technically, supposing the *windage*—namely, the excess of the diameter of the bore over that of the bullet—to be nothing), from the principle of the equality of action and reaction, $\text{W}\,v = \text{W}'\,v'$, if W, W', v and v' be the weight and velocity of the gun and bullet respectively. Now, let the bullet weigh $\frac{1}{300}$th part of the weight of the gun, then $v' = 300\,v$. But, the total work done by the powder is $\dfrac{\text{W}\,v^2 + \text{W}'\,v'^2}{2}$. The former part of this, the work on the gun, will be only about the three-hundredth of that on the bullet. We might even say that the charges are proportional to the *vires vivæ* of the bullets,—so small is the proportion of effect which is wasted upon the recoil. The methods adopted in the manufacture of powder do not belong to our subject.—The next point of interest is the *bullet* or *projectile*, itself. There are three circumstances on which the effectiveness of the projectile depends. The first is its *density*. The quantity of movement is in proportion to the mass. Lead, which alone of the metals is satisfactory in this respect, is very largely used for projectiles. The second is its *hardness*. Lead is too soft to fire against such obstacles as stone walls. It would merely be *flattened*, or even, if the velocity were great, *melted* against them. The density of cast iron, which is chiefly employed instead, is, how-

ever, only 7·8, while that of lead is 11·8. Balls of the same size are therefore very much lighter than balls of lead. Compound shot is used for the sake of combining the two advantages. It consists of an iron envelope with a case of lead—the envelope being so thick as not to be broken by the impact. The third is the *form*. The projectiles in artillery are generally spherical, so that they may fit into the gun in all positions. It would be naturally expected that the solid of least resistance should be chosen for the form of the bullet. But, for it, the resistance is only less than that of the sphere, in the direction of its axis, in the opposite direction, indeed, it is much greater. As the barrel, however, is never exactly fitted by the bullet, there is almost always some irregular motion of rotation given in the passage through it which would make the resistance greater than for the spherical projectile. Cylindroconical balls, when fitted accurately, have in more recent forms of musket, been of the greatest service, as we shall afterwards notice. We have different forms of projectiles. There is first, the solid bullet, which, for cannon, is usually made of cast iron, of a spherical shape, and so as to fit pretty accurately into the barrel. Then there are hollow projectiles, made of a hollow spherical cast iron envelope, filled with a sufficient quantity of powder to explode it. Usually, too, in order that nearly the full explosive force of the powder may be obtained, this envelope is so thick as to contain at least $\frac{1}{3}$ of the total volume. The greater the quantity of powder contained is, the more complete will be the rupture of this envelope. The projectile of this sort is fired off, like an ordinary projectile, from the cannon, but it is fitted with a wedge of wood running into the mass, and through the centre of which, some rapidly combustible matter—a fusee or the like—passes down to the powder. At the explosion of the charge, the high temperature to which the liberated gases are raised, causes the fusee to light, and it ought to continue burning until the projectile falls. Those which are shot from cannons nearly horizontal are called shells—those which are shot high, at a larger angle than 45°, are bombs. There is also what is called oblong shot, consisting of a cylindrical bar with hemispherical ends. These may be used with advantage where the guns at command, as in a ship, are small, and where the destructive effect of large ones is required. It is manifest, however, that, from the want of perfect symmetry in the figure, the oblong shot must be liable to much greater irregularity of motion—especially at high ranges—than the spherical. Within about 5° from the horizon this is not, however, very material. An oblong shot, twice the weight of a round shot of equal diameter, fired with the same charge, will only produce half its effect, but the charges must be raised correspondingly. Sometimes two or three shot of the same size are fired off from a gun at once. The only advantage gained by this is, in close action, the excessive rapidity of fire. When the ships are near—for instance, in a naval engagement—almost every shot is certain to strike, and in consequence, the different velocities, and therefore different ranges of the shot will not matter seriously. Two shot, of the same size and density, for instance, will be fired off, the one nearest the mouth of the gun with $\frac{5}{9}$, and the other with $\frac{4}{9}$ of the velocity of one of them alone with the same charge. There is the additional disadvantage of danger to the gun, and greater irregularity in the motion from collision within the barrel. There are also projectiles in use, made up of combinations of smaller ones, such as the chain-shot, sometimes employed in naval actions, and sometimes boxes or canisters filled with smaller shot. For short ranges, this canister shot is exceedingly effective—scattering when it strikes against an object, and being then as if fired off near at hand: for long distances it is not much used. What are called *rockets*, consist of an inflammable composition contained in a cylindrical case of stout paper or iron, the head or front par having the form of a coul, occasionally of a hemisphere or paraboloid. The composition for the cylindrical part is in the proportion of—nitre 4 lbs. 4 oz.; sulphur, 12 oz.; charcoal, 2 lbs When the rocket is merely intended for signals the head is filled with a composition for producing, at the time of explosion, the stars of ligh which constitute the signal. Several small apertures are made in the posterior part of the aperture, where a straight rod in the direction of th axis is placed. The rocket is then set in a tub and a fuse inserted. The liberated gases pre against all parts of the enclosing case, but as the are let out slightly at the back part, they pre strongest against the front. There is then a resultant force, continually accelerating, directe along the axis of the rocket to the posterior par It is clear that the rod or stick will serve t steady the vibrations. At first, the rod was attached to one side of the rocket. Sir Willia Congreve placed it where it now is. It is easy to see that rockets may be used as the means projecting a shell, or similar projectile accompanying them, which may act as an ordina shell does. The irregularities of motion are, however, very considerable; so much so, indeed, th rockets of this sort have never been found effecti against the actual defences of a place, thoug very much so against private dwellings, whi they set on fire. Their moral effect, upon bodi of cavalry, makes them sometimes also valual weapons. It is very certain that the enthusias belief, entertained by Sir William Congreve, th his rockets would supersede artillery in the la service, has not been verified. Mr. Hale's ne kind of rockets dispenses with the stick, by ma ing the rocket rotate. The rotation is produc in a very ingenious way. Instead of permitti the rush of flame to escape from *the bottom* o

fice, in a line with the axis of the tube, by which the flame acts directly against the air, the burning material issues from *five orifices made near the neck*, obliquely to the axis of the tube; the effect of which is, that the body of the rocket is made to rotate, while it is also propelled. This is certainly a very ingenious contrivance; but the stick-rocket continues its flight, directed by the stick after the inflammable composition has burnt out; while the Hale rocket loses all directing power. The Hale rocket has also been found to fail at low elevations, and is therefore useless against troops in plane battle-fields. Unless this defect can be remedied, the invention will fail.—The depth which a given bullet will penetrate into a wall or ship's side is a point of great practical interest. It varies in general as the radius and density of the shot and the square of the velocity at the time of penetration. It is important, especially in naval battles, that the wall penetrated should throw off splinters as numerous as possible: these act exactly like canister shot. If the velocity be just sufficient for the ball to penetrate the ship's side, there will be a great many splinters flung all round—if higher, the hole will be more nearly a mere round hole, without jagged edges or splinters; just as we may see in firing a bullet through glass, from the same gun with different charges. If the velocity be considerable, there will be no rays on the glass round the hole at all.—We pass to consider the different descriptions of fire-arms and projectile weapons commonly in use. In *fire-arms*, we have, in the first place, the iron work and the wood work distinct, the latter consisting of mere junctures and adaptations, and entirely technical in its nature. The barrel of the gun is an iron tube exactly cylindrical in shape, and rather wider at the bottom than at the mouth. The ordinary dimensions of a musket, as used in France and England, are given below:—

	French.	English.
Diameter of the barrel or calibre	·0578 feet.	·0623 feet.
Diameter of the ball	·0544 —	·0561 —
Weight of the ball grains troy	428	465
Length of the barrel	3·74 —	3·51
Length of the gun	1·246 —	1·528 —
Total length of gun and bayonet	4·986 —	5·038 —
Weight of the gun and bayonet	11 lbs. avoird.	12·2 lbs. avoird.

The hammer of the gun causes the percussion which ignites the powder. The mechanism, in its simplest form, consists of a central piece, called the *tumbler*, of the same piece as the *cock*, which a great spring turns, bringing down the cock, unless when that is stopped by a catch, which is of one piece with the tumbler. There is a slight danger, in the use of this mechanism. Little particles of fulminating powder from the percussion cap are apt to collect and remain, so that, it is possible, even when there is no cap, that the weapon should be fired off. To prevent this, a very simple contrivance, invented by M. Fontenau, is made use of. He inserts a screw into the top of the cock. When we wish to get rid of this powder—perhaps after using the gun for a day or so—we unscrew this and clean it out easily. It can be left with the screw off, when it is entirely harmless, and may be handled by children with perfect safety. There are numerous other important advantages connected with this appliance which we cannot recount here.—The most effective method of increasing the military resources of a country, open to the gunsmith, is to increase the accuracy and the range of fire-arms. The *rifle* is used for this. There is, however, very considerable difference, in the rapidity with which rifles, and ordinary muskets can usually be fired. This will be intelligible on a short description of them. The rifle barrel has seven indentations inside, running up in a spiral form. A spherical bullet, just a little larger than the bore is driven into the barrel by blows from a hammer, so that it fits very accurately, and when fired, it follows the direction of the rifle barrel, twisting along with the spiral twists, and leaving with a decided rotation on its axis. Mr. Jovell, director of the royal manufactory at Ensfield, has suggested a form of rifle barrel and of ball, that are still more convenient. There are only two spiral grooves, and the ball being accurately fitted to them can be *slid* in easily, as in the ordinary musket ball. It is possible, also, to give in this way, a much greater velocity of rotation to the bullet than in any other, because it does not need to follow so many grooves. Hence the spiral twist can in this case be much more decided. In the ordinary rifle, it ought never to make more than from $\frac{1}{4}$ to $\frac{1}{2}$ of a turn for the whole length of the barrel. Experiments made with this rifle show that it produces more effect than the old musket, at double the distance, and that it can be loaded with greater ease and speed.—Still, the obstacle, which has prevented the universal use of rifles, has been the difficulty of loading, where the ball fully fills up the barrel, as in the case of the first described. The invention of M. Delvigne removes this. In his rifle, the ball, which falls freely down into the rifle, rests at the end of a chamber wrought in the breech, and a single stroke of an iron ramrod, makes it change its dimensions, when it has got there, so as accurately to fill up the whole space of the grooves and the barrel. The perfect certainty of effect which this gives is very remarkable. The *Chasseurs de Vincennes*, can with this, aim with great accuracy at a distance of 600 to 700 yards.—Further attempts have secured still further progress; in the way, however, of giving a new form to the projectile, rather than a new shape to the gun barrel. We have spoken already of the solid of least resistance, and seen that what prevented the use of bullets of its shape, was the irregular motion as to rotation, which, in ordinary weapons, they were certain to leave with. But in

rifles, we have found that the regular movement along the grooves gives a perfectly regular motion of rotation round the axis, which, like all motions of rotation, destroys and renders insensible, whatever lesser irregularities might exist. It will have been noticed, that, in order to this, the ball, in the most approved construction, has to be crushed, so that a considerable part of the impulsive force is spent in undoing this. Hence, there is a limit in the Delvigne rifle, to its capacity as a weapon. The most improved form of the Delvigne musket, with Thouvenin's and all the more recent improvements, is here shown. The upper circle represents a cross section of the rifle barrel, A is the projectile, and E the ramrod, the end of which fits the cylindro-conical bullet. The bullet is ·657 inches in diameter, and weighs 728 grains. The barrel of the musket is 34 inches long. The *tige* or pillar C, is screwed into the face of the breast-pin. The cartridge contains 2½ drachms of powder, and is made of strong paper, tied round the projectile at D. The soldier breaks the cartridge in loading, and the powder falls down into FF, the paper of the upper part of the cartridge being thrown away. Then the ball is struck smartly with the ramrod E, and being supported on D, it is shortened and widened, so that the lead and paper round it are forced into the grooves of the rifle, the point being held in the axis by the point of the rammer. The cartridge paper protects the rifle grooves and the rest of the barrel from being leaded. This rifle (the *pillar breech* rifle) however, was found inconvenient in cleaning, as the chamber FF, gets foul and the pillar is liable to be broken. The operation of loading is also fatiguing after a few rounds. M. Minié removes this by making his projectile as in the figure, *b* being a part which is forced up into the hollow space *a*, when the explosion takes place, so that, in this way, the lead of the bullet is forced into the grooves of the rifle, and the diminution of windage, and consequent accuracy of aim of the rifle, takes place, as before. It will be clear that, unless this is accomplished by the first action of the explosion of the charge, so as to force the lead into the grooves before the shot moves at all, the advantages of a rifle would not be secured, as the paper (the cartridge being applied as in the above case), would be insufficient to make the

Fig. 1.

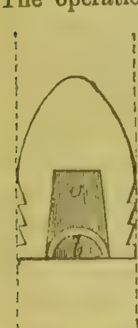


Fig. 2.

ball fit accurately. In the experiments made in 1850 with the Minié shot, the hollow part was sometimes entirely separated from the conical part, and sometimes driven violently into some part of the barrel. More careful construction of the bullet entirely prevents this. We give the

Fig. 3.

form of the Regulation Minié bullet, used in the British service. Below is also that used in the Russian service. They weigh 767 grains—being much heavier than the English. They seem much inferior to the English Regulation Minié ball In all these muskets the loading is by the muzzle of the barrel. There are considerable disadvantages in this. The gun is rendered useless if the ramrod, for instance, be lost, till a new one be obtained. To remedy this, loading at the breech has been often suggested.

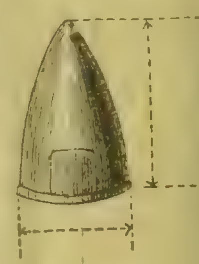


Fig. 4.

It seems that the Venetians had, before 1650, many guns, carrying 4 lb. balls, so loaded. They were known also in England in 1545. The most interesting of this description is the Prussian *zundnadelgewehr*, or needle-prime musket. We give a figure of it (fig. 5). The shot is of the form of the dotted lines in the upper part of (fig. 3), and weighs one ounce avoirdupois exactly. Its diameter at the shoulder is ·632 inches. Underneath it is the *spiegel*, or bottom, of wood with paper rolled hard round it, and with a hollow at the upper end for the end of the ball. Beneath it is a small cup P (2), containing an igniting composition. The cartridge below contains the powder, as in (2) and (3). The barrel A A is like that of the *pillar breech* musket and of the same length. It i screwed into a strong open guide or channel, B B. The chamber, properly so called, is bored ou from behind, slightly conical in shape, so tha when the cartridge is placed in it, the should of the ball C D, is stopped by the rifle groove Inside is an iron tube E E, with a strong handl F, and having at front a space G G, of about 1 inch long. In the middle of this there is pillar H, pierced for the needle N, which is t ignite the charge. It is screwed into a plate J behind which is a tube (not shown) with a doub catch spring attached, which carries withi it a small inner tube K K (1), with projection

L L, on one-half its length, and a spiral spring M, on the other. Through this tube the needle—a steel wire about ·03 inch diameter, bluntly pointed at the end, passes. At its other end the needle screws into a brass lead O, which again screws into the inner tube to which the spiral spring is attached. The trigger could not be intelligibly shown in the diagram. It has a bolt movement in firing—striking forward the needle on the fulminating matter in P. It admits of the whole mechanism of the tube E E, being drawn

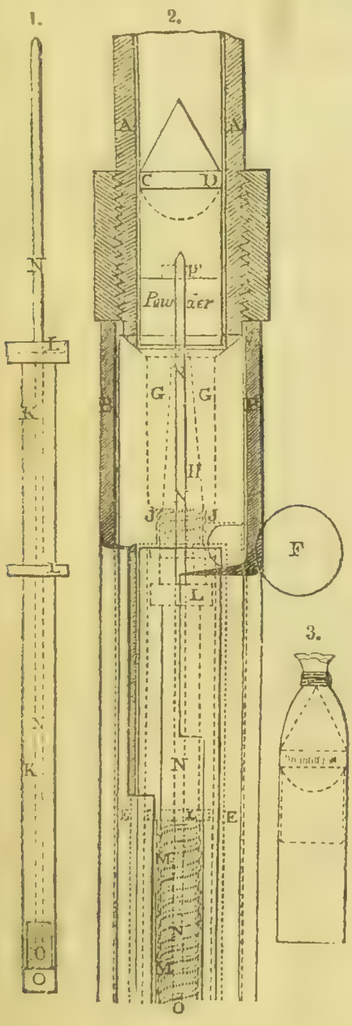


Fig. 5.

out from behind, cleaned, and put together again, which can be done in a few minutes. The sliding tube is capable of being moved backwards or forwards in the barrel, near the breech, by means of a pin or handle in its side. When it is drawn as far back as is allowed, there is an open chamber left where the charge is introduced. The tube is then pressed forward, and, by a very simple contrivance, locked into the gun as before. In this state the needle is in connection with the trigger, and the musket is ready to be fired.—The evident advantage of the needle-prime musket is the very great rapidity of fire. In an engagement, however, this is perhaps not so material. The delicacy of the whole mechanism is greatly against the general employment of this weapon. But as the Prussian government is fully convinced of the great superiority of this rifle, it has armed their soldiers with it, to a very large extent. The principle of loading at the breech has been applied to the large ordnance by Cavalli and Wahrendorff in 1846. This permits *rifled cannon.* It appears doubtful whether the Cavalli guns are sufficiently strong to endure long continued use. The Wahrendorff guns have hitherto answered better under actual trial. The principle of rotating shot has been recently applied to Cannon, by Mr. Lancaster, in his guns of elliptical bore. These make the shot rotate before passing out. and proceed like a ball sent from a rifle. They have been used for shell, but the violent motion against the side, in many cases, broke the shell. In using heavier shot, again, it has been found that the same cause has burst the gun. The Lancaster guns must, therefore, be considered, as yet, only an experiment. Pistols are only small guns especially portable; and their construction does not involve or depend upon principles in any degree differing from those already explained. The *revolver* has only one barrel; the breech carries six cavities for six charges, and makes one-sixth of a rotation each time that the weapon is fired off, putting itself immediately so as to be ready to be fired off again. There are very considerable mechanical difficulties to overcome from the niceness of the mechanism; but in such weapons as Colt's revolvers, these are, in effect, very sufficiently overcome.—As for Cannons, and the larger pieces of ordnance employed, there are three things to be considered: the accuracy of aim, the manœuvring for which they are intended, and the means of transport. In view of all these, we require different arrangements for almost every different purpose—so that we have mountain artillery, field artillery, naval and siege artillery, and the like. For field artillery, the first essential is mobility. The carriage consists of two principal parts, the carriage properly so called, and the waggon, which accompanies the gun for the sake of carrying various supplies of shot, powder, &c.. The violence of recoil is carefully to be guarded against; and this may be done in two ways: either by increasing the weight of the cannon in proportion to the ball, or by making the beam of the gun carriage, by which it fits into the waggon that goes before, so long that it lie at a very acute angle to the ground. Siege artillery is much heavier than ordinary field artillery, and the condition of mobility is not so much attended to. Mortars and Lancaster guns, having great weight, it is necessary that their recoil should be as slight as is possible. Naval artillery, and fortress artillery, again, are subject

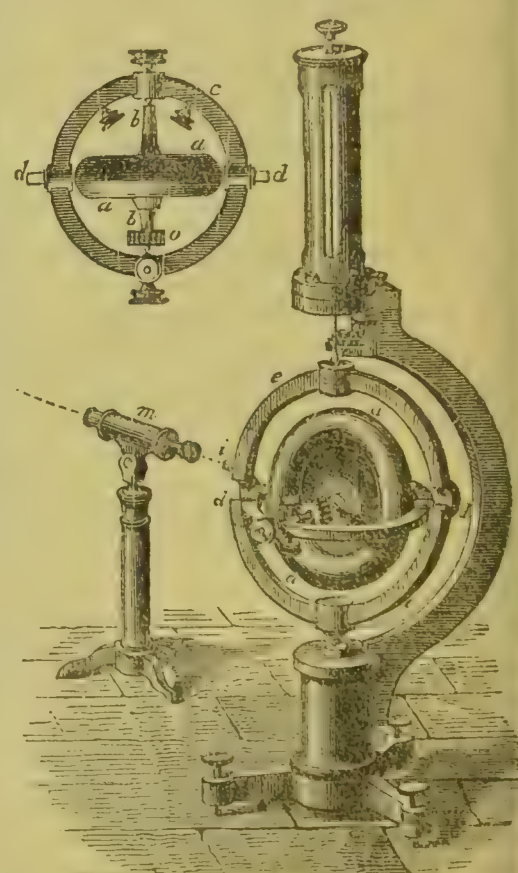
to conditions easily conceived, which cause certain technical modifications in their construction. The carriages are usually made of wood; sometimes—for the sake of lasting longer—of bronze. The cannon themselves are made of bronze, and malleable or cast iron. The qualities desirable in their construction—which neither of these metals fully procures—are sufficient *tenacity*, in which respect they stand in the following relations:—Malleable iron, 21. Bronze, 13. Cast iron, 6. (The corresponding densities of the three metals are 7·78, 8·826, and 7·03 respectively.)—Next their hardness. Then, there is also required, that if the cannon should burst, the explosion should be as little dangerous as possible. This is the greatest objection to cast metal, which would be scattered asunder in a thousand fragments in such a case, while bronze and malleable iron would probably crack before they broke. And there are further economical considerations of price, and of a supply that may answer any demand. In these latter, lies the greatest objection to the employment of bronze. Cannons are now frequently made of a sort of composite of cast and malleable iron, the inner part being malleable, the outer cast metal, and in the interval, the two metals shading down one into the other. It must be remembered that the difference of the two metals, is only a little more or less of carbon in their composition; and that metals —of qualities approaching nearer and nearer continually—can be used for the different layers. There have been attempts to use bronze and cast iron, but the different dilatability of the two metals interposes an almost insuperable barrier. Even the outermost layer of cast iron, has very nearly the same dilatability as the innermost of malleable iron. The succession of metals is something like this. Iron, steeled iron, steel, white cast iron, grey cast iron. The iron has itself no trace of carbon in it, the steeled iron has traces, the steel from ·6 to 1·5 per cent., the white iron from 2 to 4, and the grey from 3 to 7 per cent.—Very ample details on the processes of manufacture adopted, will be found in the French *Dictionnaire des Arts et Manufactures*, articles *Affut*, *Armes a feu*, *Bouches a feu*, *Poudre a Canon*, and *Projectiles*, and Sir Howard Douglas's admirable work on *Naval Gunnery:* to all of which, the present article is indebted.

Gunter's Line. A scale upon which numbers are laid down opposite their logarithms. Suppose the scale to be in inches, and to be 3 inches long—then 1000 might be marked at the end (10^3, 3 being the logarithm of 1000), 100 at two inches from the beginning (2 being the logarithm of 100), and 10 at one inch from it, the intermediate intervals of an inch being filled up by the numbers whose various logarithms correspond to the respective lengths. Its use is just like that of a logarithmic table. If we want to multiply 4×8, take the length marked 4, extend it from the point marked 8, and we shall come to 32. The reason is evident, we have been adding the logarithms. So, to take the square or cube root of a number, bisect or trisect its length on Gunter's line, and to take its square or cube, double or triple it. And so on.

Gunter's Scale. A scale of great use to sailors in working simple questions of navigation. Various trigonometrical quantities, such as sines, chords, tangents, &c., of different angles are laid down on it; and it is frequently of considerable advantage in practice. Its use depends on trigonometric principles, just as the use of the Line of logarithms.—The *Gunter's Chain*, used by surveyors, is also worthy of notice. Its length is 66 feet, and 10 square chains give an acre. It is divided into 100 links, so that 100,000 square links make up an acre accurately. It is very useful when measuring land for agricultural purposes to have so ready a method of turning the results into acres. Thus, suppose a rectangular field, 7·95 chains long (*i.e.* 7 chains, 95 links), and 4·80 chains broad, then $\frac{7 \cdot 95 \times 4 \cdot 80}{10} = 3 \cdot 816$ is the number of acres contained in it.

Gyration, Centre of. See CENTRE OF GYRATION.

Gyroscope. An instrument recently invented



by M. Foucault, which has acquired notoriety from its efficiency in rendering *visible*—through

its direct dynamic effects—the diurnal rotation of the Earth. The rationale of the instrument has been already adverted to in page 202; the following are a few details regarding its structure. Its object being to enable a heavy disc, *a a*, in rapid rotation, to preserve what plane of rotation its dynamic conditions may require it to maintain, the uses of the various parts of the Gyroscope will readily appear. The tall figure shows the gyroscope in supposed action, and a simple inspection will evince how very free is the frame *e e* to shift its plane,—being suspended at its top by a simple thread passing through the upright cylinder, and resting at its bottom on a very fine point placed in an agate cup. Within the frame *e*, the detached frame *c c* may be laid, at any time, so that the knife edges *d d* rest on hard plates; and as that frame is so constructed that the heavy rim, *a a*, which constitutes its chief part, may be put into rapid rotation by a separate machine, it is evident that the said ring may easily be placed in that state of rapid motion within *e e*, and so left that it experience no appreciable obstruction in its efforts to preserve its natural plane. The form of the instrument has since been simplified: that, just described, is Foucault's. In reference to its important object, it is most successful. See EARTH, page 202.

H

Hadley's Sextant and Quadrant. See SEXTANT.

Haidinger's Dichroscopic Microscope. See MICROSCOPE.

Haidinger's Fringes or Tufts; a very curious set of phenomena, first discovered by the eminent mineralogist of Vienna, by means of which *polarized light* may be detected by simple vision. These tufts require—in order that they be easily seen—the Observer to have a pretty distinct conception of them, and also that he have an eye naturally sensitive to colour. Neither the word *fringe* nor *tuft*, adequately explains the appearances discerned by Haidinger: the following will better define them:—Take a set of small twigs of willow—of course of yellowish colour—and tie them tightly by the middle. The ends of the twigs will of course spread out somewhat, and form yellowish *tufts* or *brushes.* At the *waist*, or narrow part of this bundle, conceive a slight amount of *violet* light: *that* is the complete picture of what every good and careful eye may detect when receiving a beam of polarized light. The phenomenon is not special but universal. Light, if simply polarized—from whatever quarter or in whatever way it comes—whether polarized by artificial modes, or natural modes, such as by the atmosphere, whether polarized by reflection or refraction, uniformly exhibits it; so that Haidinger's *tufts* are an infallible criterion *to a good eye*, whether the light it discerns is polarized or not. Haidinger considers further, that the disposition of these tufts clearly indicates the *plane* of polarization—the axis of the yellow light always being in that plane.—The theoretical explanation of these remarkable colours is perhaps still to find. But it probably holds by this law, —*no medium can polarize all the coloured rays under the same incidence;* so that the *disappearance of the reflected ray never can be entire.* The intermediate colours alone, would, under such circumstances, be quite lost to the eye; and the remaining complementary ones, viz.:—the *yellow* and *violet*—surviving, so to speak, the comparative extinction of the others, would arrange themselves, in some form, before the attentive operator. They must also, if this be true, arrange themselves in tufts, with axes *at right angles to each other.*—The subject, however, remains obscure.

Hail; Hail-Storm. One of those meteorological phenomena, of which the explanation continues exceedingly difficult and doubtful.—The circumstances of a Hail-storm are generally these. It is preceded by a rustling noise in the air, and always accompanied by electrical effects: thunder is sometimes its precursor; it is often heard during the storm, and at the approach of that storm, the electrometer frequently changes its sign,—indicating, at the same time, great differences of electric *intensity.* In our climates, these showers usually occur in spring and summer, and almost always about those hours when the daily temperature is highest. Hail often precedes and accompanies heavy rain-showers; it rarely follows them. The clouds that scatter this meteor are extremely dense, and generally they exhibit a sort of bronze colour; their edges are irregular; they manifest great irregularities in their solid contour, and, in the main, they are not much elevated. Like all great storms, these Hail-storms are local; but their devastation extends, nevertheless, over considerable spaces. By one single storm in France in 1788, the damage done was computed at *one million of pounds sterling*,—this storm ravaged upwards of a thousand parishes.—The Hail-drop is usually composed of several distinct and very distinguishable layers of transparent ice, surrounding a white and opaque nucleus, which appears a mere flocule of snow. The weight of the hail-drop often reaches that of a hundred or three hundred grains.—How is it to be conceived, then, that solid pieces of ice of such weight—the assemblage of which, constitutes those dark bronze-looking clouds—can sustain themselves in the air, or develop themselves within those clouds? A problem, whose solution is not achieved. The explanation, on the ground of electrical operations, first given by Volta, has recently been modified by M. Peltier, and presented in the following shape. When

two clouds, in opposite electric states, are placed one above the other, their mutual attraction is considerable: the two strata approach without any signal electric discharge—only they act on each other, through *influence*—that is, as one pointed or corrugated electric body acts on another of the same kind. The electricities are thus exchanged; but, according to Peltier, no such exchange can take place, without producing *vaporization* of the water of those drops or vesicles of which the clouds are composed: hence, a lowering of temperature—rapid in proportion to the electric tension of the two clouds. If the temperature of the two clouds is considerable, no noticeable effect results: but if one of them is near *zero*, this diminution of temperature must produce congelation of the portions of it not *vaporized*—transfusing them into flakes of snow. Peltier follows the course of these flakes, showing how, in his opinion, they must be quickly surrounded by strata of solid ice: but as the entire views of this Inquirer are simply under consideration; and—be it frankly confessed—as the whole subject appears at present, much more a *talk* about a thing, than a careful deduction of necessary results, we shall not follow him into details. Kaemtz, on the contrary, is inclined to attribute all known effects to the low temperatures of those upper atmospherical strata, in which the particles of water solidify. Neither, however, does his theory at all satisfy one. There is, indeed, at present, no sufficient explanation of the phenomena of a *Hail-storm;* and it is abundantly necessary that Physicists betake themselves to closer and more numerous verifications of all the facts connected with these extraordinary and formidable *Hydrometeors.*

Halley's Comet. One of the best known and earliest discovered of the comets of our system. Halley, the celebrated English astronomer, compared the various comets which had appeared before his time, with the recorded elements of their orbits at each time, and found that three comets had appeared in the years 1531, 1607, and 1682, at intervals, and with elements differing only as any one knowing gravitation would expect them to do. He ventured to predict that it would return in the year 1758. It did appear accordingly on Christmas day of that year, and was afterwards repeatedly observed at various points in Europe, though not at all by the naked eye. The prediction of its return in 1835 was easily made; but there remained the enormous labour of making the corrections which the perturbing forces of the planets would require. It was at last, however, undertaken, and the comet returned as predicted. It had been predicted by Rosenberger on the 11th November, and by Sehmann on the 26th, and it actually passed its perihelion on the 16th. It is not difficult now to predict its return about the year 1911, but the labour of calculating its perturbations, and the exact date of its reappearance, has yet to be gone through. With all the increased power of our mathematical analysis, we should be able to predict its return within a day, or even a few hours of the truth. The following are the elements as observed in the Perihelion passage, November 15·93546 of Greenwich mean astronomical time:

Place of Perihelion on the orbit	304° 32′ 9″·2
Long. ascending node	55 8 21·2
Inclination of orbit	17 45 56 7
Eccentricity	·9675509
Semi-axis major	18·0779386

See Comets.

Haloes; Parhelia. Haloes are extremely complex optical phenomena, due to the refraction of light through small frozen particles floating in the atmosphere. Haloes, properly so called, are coloured circles around the luminary whose rays give rise to them: the radius of the first circle subtends an angle of 22°; that of the second, 46°; and there is sometimes a third, the visual angle of whose radius is 90°. All three are coloured prismatically; in the two first, the red is within, and the violet without; while, in the third circle, the opposite arrangement takes place. The atmospheric modifications that give rise to these concentric circles around the Sun, may also produce a white circle parallel to the horizon, as broad as the Sun, and passing through his disc. Sometimes another white band is visible, also passing through the Sun, but perpendicular to the former, giving rise to a white cross within the circles of the halo: which cross, however, often extends beyond these circles,—the horizontal one, as just remarked, going round the entire horizon.—*Parhelia*, or images of the Sun, are formed upon these white bands, near their intersection with the circles of the haloes,—their distance from the centre of the luminary being greater, the higher that luminary is in the sky. Parhelia are coloured like haloes, and have frequently a sort of tail, lying along the parhelic circle on which they are formed.—Finally, quite independent of haloes properly so called, or o[f] circles concentric with the Sun, and also of parhelia—*tangent circles* are sometimes discerned, an[d] portions of elliptic arcs, of an extremely comple[x] nature; evidently connected, however, with th[e] concentric and parhelic system.—Marriotte, lon[g] ago, attributed haloes to the refraction of ligh[t] through small crystals of ice; and all observa[tion] appears to confirm his view. Calculatio[n] also confirms it, for the angles subtended by th[e] radii of the concentric circles, are deducible fro[m] the natural shape of these crystals of ice, whic[h] are always referable to hexagonal prisms. Th[e] third circle, with colours in the inverse order, [is] probably due to the refraction of the rays th[at] have undergone a first reflection within th[e] prisms. The *rationale* of this process may b[e] comprehended by the student who reads artic[le] Rainbow. Parhelic, horizontal, and vertic[al] circles, being, on the other hand, white, or un[-]coloured, must originate in *reflections* of light

and it is in acts of *reflection* by the small flakes of snow, that they find their explanation. See for details, any extended optical treatise, or the excellent work on meteorology by Kaemtz.—These phenomena are never seen, unless the *cirrus*, or *cirro-stratus* cloud, intervenes between the luminary and the observer;—a fact decisively establishing the nature of the structure of these clouds.

Hardness of substances, is a quality by which we are frequently enabled to draw clear distinctions between them. It is probable that the hardness of all the ultimate atoms is the same, though no experimental investigation throws light on such a point.—We form tables of the hardness of bodies, placing them in the order of their capacity for scratching one another. If one body, drawn along another, leaves a scratch on its surface, it may be set down as harder than it. It would be improper here, however, to make the scratching body very sharp pointed, as such a point would make us believe the scratched body softer than it really is, as glass is sometimes scratched by a pretty sharp pin point. Mohl has taken the following bodies as his units of hardness:—

Talc has a hardness marked by	1	Felspar, or adularia	6
Rock salt, gypsum	2	Rock crystal	7
Calcareous spar	3	Topaz	8
Fluor spar	4	Corundum	9
Phosphate of lime, or apatite	5	Diamond	10

Those which are scratched, *e.g.* by topaz, and not by rock crystal, have a hardness somewhere between 7 and 8, and it is quite conceivable how, arranging a series of such substances, we may, by mutual comparisons, come to be able to fix approximately the numbers representing their hardnesses. There is only this danger to be guarded against, that we may get the substances in their non-natural state, *e.g.* hammered metals. A change of hardness might evidently result from such a process as hammering, and we should then thus obtain false indications. We give a table of the hardnesses of various of the more common substances. It is a general rule, though not unvarying, that simple bodies are hardest:—

Plumbago	1	Sal ammoniac	1·5—2
Common salt	2	Amber	2—25
Native sulphur	2·5	Native copper	2·75
Native gold	3·25	Native silver	4·25
Native iron	4·5	Native arsenic	.5
Opal	6·75	Quartz	7
Chalcedony	7	Flint	7—7·25
Emerald	7·5—8	Topaz	8
Tourmaline	8	Chrysoberyl	8·5
Sapphire	9	Diamond	10

Harmonics. We have seen (Acoustics) that when a string is set to vibrate, so as to evolve musical tones, its halves, its thirds, its fourths, and so on, vibrate also; producing, according to the fundamental laws of vibration, notes with twice, three times, four times, &c., the number of vibrations which the original had. These notes are called *harmonics* to the original one. According to this method, it is evident that every note will have its just harmonic, at the same point in the succeeding octave, as it occupies in its own. Thus C produces, for example, C′; if C′ be the C of the octave immediately above; and as C′ has 2 a vibrations (a being the number of the original note), the next, which has 3 a, or $\frac{3}{2} \times 2\,a$ vibrations, will be G′, *i.e.* G of the same octave. The next with 4 a vibrations, or $2 \times 2\,a$, will be C″, and 5 a, or $\frac{5}{4} \times 4\,a$, will be E″. The series of harmonics, when the fundamental note is, *e.g.* C, will therefore be thus,—

C, C′, G′, C″, E″, G″ (B‴ flat) C‴ D‴ E‴ F‴ G‴ A‴, &c.

When we employ an instrument consisting of a single tube open or closed, the foregoing must be the series of notes that can be obtained from it. The exact series will depend upon the fundamental note. (See Acoustics, *Tables.*) The bugle, the French horn, and the trumpet, are examples of such instruments.—In no instrument is it possible to give the whole series of harmonics. We have already pointed out (Acoustics), that there is a limit at once to the number and the fewness of the vibrations which are audible to the human ear. Several of these, it is not possible for ordinary people to sound. Hence, the range of such instruments is sufficiently limited, and yet, remembering that the fundamental, without being either incapable of being heard or sounded, may have a pretty small fraction of the highest number of audible or soundable vibrations, we may make the range very extensive.

Hearing. The act of hearing is, of course, that accomplishment of sensation for which provision is made by the organism of the Ear. It may be used in a wider sense to denote the accomplishment of that sensation by aid of any mechanism. To determine the circumstances under which Hearing is the easiest—to determine, for instance, the structure of a large room or hall, so that a speaker be heard with facility, is, unfortunately, as yet, one of the great *desiderata*, and *desideranda* of practical science. The first necessity,—the only one, indeed, clearly recognized—is the prevention of echoes; and, this is accomplished best, by such irregularity of interior, as shall prevent any one predominant reflection of sound—an irregularity that may break up the reflected waves into a great number of small ones, many of which may be supposed to interfere with and destroy each other. When the speaking for which a room is to be used, takes place invariably at one spot—such as a Church, or a public assembly with a Tribune, the voice of the speaker may be greatly aided by reflectors placed behind him: but when the speaking occurs, as in our House of Commons, at all parts of the building, no such aid is admissible. The infancy, or rather total absence of applicable science on this subject, has lately been sufficiently proved by Sir Charles

Barry's efforts at Westminster.—The practised speaker learns that there is an art, in this matter, which he must acquire for himself. Every room has its own peculiar or fundamental note; and if the voice be pitched, in conformity with that note or any of its concords, speaking and hearing will be alike comparatively easy.—See farther, article SOUND, *Transmission of.*

Heat, or **Caloric,** the cause of our sensations of Heat, and the source of many of the most important and manifest actions and changes that occur in the material world. The phenomena connected with this powerful physical agent, are described and discussed under various separate articles in our dictionary. For *Heat*, as modifying the condition of bodies, see articles EXPANSION, CONGELATION, VAPORIZATION, and the one immediately following this notice. For the laws of the diffusion of Heat, the student is referred to ABSORPTION, CONDUCTION, SPECIFIC HEAT; and, with respect to free Caloric, to our long article on RADIANT HEAT, as well as to REFRACTION and POLARIZATION. The whole modern theory, termed the MECHANICAL THEORY OF HEAT, has been discussed in a very elaborate article from the pen of Professor Rankine of Glasgow University, to whose researches its development owes so much. The applications of Heat to practical purposes, are treated under HEATING OF BUILDINGS, VENTILATION, AIR-ENGINE, STEAM-ENGINE, and VAPOURS. For the modes of measuring Heat, see PYROMETER and THERMOMETER. And every important current speculation regarding its cosmical action, will be noticed under SUN and TEMPERATURE.

Heat, *as the Cause of Change of State.*—Until quite recently, it would have been enough to state in this place, that under the influence of Heat or of its abstraction, all bodies may be made to assume the *solid*, *liquid*, or *gaseous* form; but the remarkable investigations of M. Boutigny d'Evreux relative to the phenomena and conditions of change of state, demand a special notice. The primary fact, in this curious inquiry, was noticed by Leidenfrost in 1790. It is this—if a drop of water be let fall on a *red hot dry* plate, instead of distributing itself over the surface of the plate, or immediately evaporating, it rolls about over that surface, and remains rolling for a considerable time. The observations and researches of Boutigny have given this curious phenomenon full definiteness, and seem to lead to practical results of greatest importance. It appears that whenever water or other liquid is dropped on a plate or capsule, at a temperature considerably higher than the boiling point of that liquid, it immediately assumes the form of an oblate spheroid, becomes subject to the aforesaid undulatory motion, and requires a very much longer time for its evaporation, than had the plate been merely at the temperature of its boiling point. Boutigny calls this the *spheroidal state* of bodies subject to the action of Caloric, or the *spheroidal effects of Caloric.* It also appears that if the bulb of a thermometer be plunged into the spheroid, that thermometer always indicates a lower temperature than the point of ebullition: let the metallic capsule, however, be allowed to cool, the temperature of the water will immediately rise, and ebullition of a most violent and convulsive nature takes place on the instant. It is easy to see how, on the ground of these most singular facts, the moistened hand may safely be brought into contact with red hot metals; although the astonishment is not yet forgotten, with which the members of a recent meeting of the British Association, saw M. Boutigny handling innocuously, and without fear, masses of boiling lead; thus apparently realizing the tales—usually deemed legendary—of escapes in ancient times from the *ordeal* of *Fire.* It will be recollected by the historical student, that, in the year 241, one of the High Priests of the Magi, proposed—under the orders of *Sapor*—that large quantities of molten lead should be poured on his naked body, and that if he came forth unscathed, the truth of the orthodox faith should be held established. The experiment was made before an immense assembly; the High Priest was unhurt, and the doubts of sceptical Persians thus banished for ever!—These remarkable researches have thrown much light on the formerly very puzzling phenomena of the explosion of Steam Boilers. We shall discuss this subject at length under the appropriate article (STEAM BOILERS); it may simply be remarked here, that, on the very contrary to what was formerly believed, these catastrophes must often have been the result of acts in any way *lowering the temperature of* the boiler.—More pertinent it is to notice in this place, the *theoretical* inquiries suggested, and only yet in course of solution. For instance, what is the lowest temperature at which this spheroidal state can be produced? What the rate or law of evaporation when the body has entered the spheroidal state? What the temperature of bodies in this state, and what the temperature of their vapour? Does radiant heat traverse these spheroids, or is it reflected from their surfaces? Are all bodies capable of assuming this condition, or only a certain class; and if the latter, what the characteristics of this class? Is there contact between liquids in the spheroidal state, and the surfaces that gave rise to that state?—These points, and many others, are still *sub judice.*—The reader is referred especially to the original and laborious researches of M. Boutigny.

Heat, *Capacity of Bodies for;* or SPECIFIC HEAT.—If two equal masses of different substances, at the same temperature, are required to be raised in temperature the same number of degrees, it is found that a greater positive quantity of Heat is required to accomplish the change in the one body than the other. On this account bodies are said to vary in their *Capacities*, or *Specific Heats.* See SPECIFIC HEAT.

Heat, Theory of the Mechanical Action of, or Thermo-dynamics.

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36. Conclusion.—Science of Energetics.

1. *Historical Sketch, and References to Authorities.*—It is a matter of ordinary observation, that Heat, by expanding bodies, is a source of motive power; and, conversely, that motive power, being expended either in compressing bodies, or in producing friction, is a source of heat. The reduction of the laws according to which these phenomena take place, to a physical theory, or connected system of principles, called the SCIENCE OF THERMO-DYNAMICS, is of recent date, and, in many respects, may be considered to be still in progress. The steps in reasoning, and in experimental knowledge, which have gradually led to the formation of this system of principles, are difficult to trace, and more difficult to separate from the history of the two kinds of mechanical hypotheses, which have been proposed as means of deducing the laws of heat from those of motion and force; for one of those hypotheses, —that which supposes the phenomena of heat to be caused by the presence, in greater or less quantity, of a substance called "*caloric*," has been the chief impediment to the progress of the accurate knowledge of the laws of the relations between heat and motive power; while the other hypothesis, which supposes the phenomena of heat to be caused by molecular vibrations and revolutions, has been the means, in some instances, of anticipating laws, and predicting numerical results, which have since been confirmed by experiment, and in others, of suggesting experiments whereby important laws have been discovered.—In the stage which our knowledge has now attained, it is possible to express the laws of Thermo-dynamics in the form of independent principles, deduced by induction from the facts of observation and experiment, without reference to any hypothesis as to the occult molecular operations with which the sensible phenomena may be conceived to be connected; and this course will be followed in the body of the present article, reserving to the conclusion a general account of the nature and results of those hypothetical ideas which have been, and may be found useful. But, in giving a brief historical sketch of the progress of Thermo-dynamics, the progress of the hypothesis of thermic molecular motions cannot be wholly separated from that of the purely inductive theory.—The Aristotelian Hot Element, as well as the other *στοιχεῖα*, appears, so far as we can judge, to have been understood by Aristotle himself, not as a *substance*, but as one of the *states* of which substances are susceptible.—In the *scholastic* sense of the term "*Elementum Ignis*," viz., the supposed substance, afterwards called "Phlogiston" and "Caloric," Galileo disputes the real existence of anything corresponding to it, and Bacon declares it to be one of those "*nomina nihilorum*" which are amongst "*Idola fori molestissima*."—The hypothesis of thermic molecular motions was maintained by Galileo, Bacon, Boyle, and Newton, Montgolfier, Seguin, Rumford, Davy, Leslie, and Young. Rumford and Davy supported it by most remarkable experiments on the production of heat by friction,—a phenomenon which is the key to the whole science of Thermo-dynamics:—Davy endeavoured to put the mechanical hypothesis into a definite form: Young, in his Lectures, stating the whole question in the clear and forcible manner peculiar to him, showed, that the facts of experiment, as known in his time, were conclusive against the hypothesis of substantial caloric. That hypothesis, however, continued to hold its ground, and to a considerable extent does so still; a fact which is probably in a great measure owing to the employment of its language in works of reference, and to the popular tendency to ascribe substantive existence to the subject of a name.—The adoption of the hypothesis of thermic molecular motions, and, what is of more importance, the abandonment of the hypothesis of substantial caloric, have been much promoted by the series of discoveries which have shown, that the communication of light and heat by radiation, if not actually consisting in the propagation of molecular vibratory movements, takes

place according to laws analogous to those of the propagation of such movements, and wholly at variance with those of the diffusion of any conceivable substance.—A most important step towards the formation of a true physical theory of the relations, not only between heat and motive power, but between heat and every other kind of physical energy, was made by Black's great discovery of Latent Heat, and by Watt's applications of that discovery to the improvements of the Steam Engine.—The term "*Latent Heat*," when freed from hypothetical notions, means, an amount of that condition of matter called *Heat*, which has disappeared in producing physical effects different from heat,—such as expansion, fusion, evaporation, and chemical changes,—and which may be made to reappear by reversing the changes in which such physical effects consisted,—that is, by compression, congelation, liquefaction of vapours, and inverse chemical changes.—The progress in the true theory of Thermo-dynamics, to which this discovery might have led, was for a long time retarded by a fallacious principle, arising from the hypothesis of substantial caloric in the following manner:—Let a substance change from a less bulky to a more bulky condition, or from the liquid to the gaseous state, or generally, from the state A to the state B, this change being of such a nature, that, according to Black's discovery, heat disappears, and some physical effect different from heat is produced. Let this operation be called (A, B), and let H_1 be the amount of heat which disappears. Next, let the substance change back from the state B to the original state A: let this change be called (B, A). It will cause a certain quantity of heat H_0 to reappear. If the series of intermediate changes undergone by the substance during the process (B, A), be exactly the reverse, step for step, with those undergone during the process (A, B), everything done by the first process will be exactly undone by the second; no permanent physical effect will ensue from the combined processes; and the amount of heat which reappears, H_0, must necessarily be equal to the amount of heat H_1, which formerly disappeared. This was understood from the time of the first discovery of Latent Heat; and so far there is no fallacy, but an important truth. But it was further assumed, that heat has a substantial existence, and that, consequently, $H_0 = H_1$, under all circumstances, even although the processes (A, B) and (B, A) should differ in their intermediate steps. This assumption leads to the following paradoxical result, which shows it to be fallacious. It is known that the process (B, A) may be made to differ from (A, B) in its intermediate steps in such a manner, that a permanent mechanical effect shall be produced by the combined processes. Now, if under such circumstances, H_0 is assumed to be still $= H_1$, it follows, that by employing the mechanical effect of the combined processes in *developing heat by friction*, we may *increase the amount of heat in the universe*, or *create caloric*;—a consequence opposed to the original assumption of the substantiality of caloric, and proving that assumption to be self-contradictory.—That fallacious assumption unfortunately pervaded the reasonings of Carnôt, in his *Réflexions sur la Puissance Motrice du Feu* (Paris, 1824):—a work which, notwithstanding this fallacy, contains the first discovery of an important law:—*that the ratio of the greatest possible work performed by a heat engine, to the whole heat expended, is a function of the two limits of temperature between which the engine works, and not of the nature of the substance employed.*—(*Thomson's Account of Carnôt's Theory*, *Edinb. Trans.*, 1849, vol. xvi.)—The fallacy referred to prevented Carnôt from discovering what this function of the limits of temperature is.—The phenomenon of the development of heat by the friction of a fluid possesses peculiar advantages as a means of ascertaining the relations between heat and mechanical power, owing to the simplicity of the action which takes place; for, at the end of the process, the fluid is left exactly in the same condition as it was at the beginning; so that the evolution of a certain amount of heat is the sole effect produced; and this being compared with the mechanical power expended in agitating the fluid, exhibits in the most simple, direct, accurate, and satisfactory manner possible, the relation between heat and mechanical power.—The idea of subjecting this phenomenon to experimental measurement, appears to have been first put in practice independently by M. Mayer about 1842, and by Mr. Joule about 1843. The numerical results at first obtained, were, as was to be expected in a new kind of experiment, somewhat rough and inexact; but by long perseverance, Mr. Joule increased the exactitude of his methods of experimenting, until he succeeded in ascertaining by experiments on the friction of water, oil, mercury, air, and other substances, to the accuracy of $\frac{1}{300}$ of its amount, if not more closely still, the *Mechanical Equivalent of an Unit of Heat*; that is, *the number of foot-pounds of mechanical work which must be expended, in order to raise the temperature of one pound of water by one degree.* For Fahrenheit's degree, this quantity is 772 foot-pounds: for the Centigrade degree, $\frac{9}{5} \times 772 = 1389{\cdot}6$ foot-pounds (*Phil. Trans.*, 1850).—This, the most important numerical constant in molecular physics, has been styled by other writers on the subject "JOULE'S EQUIVALENT," in order that the name of its discoverer may be perpetuated by connection with the most imperishable of memorials—a TRUTH.—Mr. Joule at the same time proved by experiment the law which had previously been only a matter of speculative theory with others: that not only heat and motive power, but all other kinds of physical energy, such as chemical action, electricity, and magnetism, are convertible and equivalent; that is to say, that any one of these kinds

energy may, by its expenditure, be made the ans of developing any other, in certain definite ›portions.—Meanwhile, partly through a theo- ical anticipation of this law, and partly through ; influence of the hypothesis of *molecular mo- ns* as applied to heat, the formation of a sys- natic theory of the relations between heat and ›tive power advanced. MM. Helmholtz, Holtz- ınn, and Waterston, may be referred to as ving aided this progress.—The investigations the Count de Pambour, on the theory of the ›am Engine, although not involving the dis- ;ery of any new principle in Thermo-dynamics, ›perly speaking, were conducive to the progress that science by pointing out the proper mode . applying mechanical principles to the expan- e action of an elastic fluid.—We shall now ıclude this historic sketch of the Science of ıermo-dynamics, by referring to those recent pers in Scientific Transactions and periodicals, which its laws and their consequences are set th by different authors in a systematic and tailed form, and investigated by processes dif- ing considerably in detail, but agreeing in eir fundamental principles and their results.— LAUSIUS: *Poggendorff's Annalen*, 1850-1856, *ssim.* RANKINE: *Edin. Trans.*, 1850-52 (vol. x.); *Phil. Trans.*, 1854, 1859; *Phil. Mag.*, 351; *Edin. Phil. Jour.*, 1849, 1855; *On the eam Engine and other Prime Movers*, 1859; HOMSON: *Edin. Trans.*, 1851-53 (vol. xx.); *hilos. Magazine*, 1851-5, *passim.*—Numerical ıta, without which these theoretical researches ould have been fruitless, were furnished by the ;periments of Dulong, and of MM. Bravais, artins, Moll, Van Beek, and others, on the ›locity of sound; by those of M. Rudberg, on .e expansion of gases; by the experiments, al- ost unparalleled for extent and precision, of . Regnault, on the properties of gases and ıpours, made at the expense of the French overnment, and published in the *Proceedings ıd Memoirs of the Academy of Sciences*, from 347 to 1854; and by the joint experiments of iessrs. Joule and Thomson, on the Thermic ffects of currents of elastic fluids, made at the ;pense of the Royal Society, and published in ıe *Philosophical Transactions* for 1854.

2. FIRST LAW OF THERMO-DYNAMICS.— *'eat and Motive Power are mutually convert- le; and heat requires for its production, and 'oduces by its disappearance, motive power in e proportion of* 772 *foot-pounds for each Fah- ›nheit Unit of Heat:*—the said unit being the nount of heat required to raise the temperature one pound of liquid water by one degree of ahrenheit, near the temperature of the maxi- um density of water. This law may be con- dered as a particular case of the application of vo more general laws, viz:—1. All forms of ıergy are convertible:—2. The total energy of ıy substance or system cannot be altered by the utual actions of its parts.

3. *Dynamical Specific and Latent Heat.*—All quantities of heat, such as the *specific heat* of any substance, or the *latent heat* correspond- ing to any physical effect, may be expressed *Dynamically*, that is, in units of work, by multi- plying their values in ordinary units of heat by Joule's equivalent.

4. *Total Actual Heat.*—Let a substance, by the expenditure of motive power in friction, be brought from a condition of total privation of heat to any particular condition as to heat. Then, if from the total motive power so expended, there be subtracted—First, the mechanical work performed by the action of the substance on external bodies, through changes of its volume and figure, during such heating,—Secondly, the mechanical work due to mutual actions between the particles of the substance itself during such heating,—the remainder will represent the motive power which is employed in *making the substance hot*, and which might be made to reappear as ordinary motive power, if it were possible to reduce the substance to a state of total privation of heat. This remainder is the quantity called the *Total Actual Heat* of the substance: being the Total Energy, or capacity for performing work, which the substance possesses *in virtue of being hot.* It is not directly measurable; but its value may be computed from known quanti- ties, by means to be afterwards explained. When a homogeneous substance is uniformly hot, every particle of it is equally hot; and every particle is hot in virtue of a condition of its own, and in- dependently of its relation to other particles. These are facts known by experience; and they lead to the following consequence:—that when the total actual heat of a homogeneous and uni- formly hot substance is considered as a quantity made up of any number of equal parts, all those equal parts are similarly circumstanced; and hence follows—

5. THE SECOND LAW OF THERMO-DYNAMICS. —*If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided into any number of equal parts, the effects of those parts in causing work to be performed will be equal.*—This law may be considered as a particu- lar case of a general law applicable to every kind of *Actual Energy:* that is, capacity for perform- ing work constituted by a certain condition of each particle of a substance, how small soever, independently of the presence of other particles (such as the vis-viva of motion). The mathe- matical expression of the Second Law of Thermo- dynamics is as follows:—Let unity of weight of a homogeneous substance, possessing the actual heat Q, undergo any indefinitely small change, so as to perform the indefinitely small amount of work dW. It is required to find how much of this work is performed by the disappearance of heat. Conceive Q to be divided into an indefinite number of indefinitely small equal parts, each of which is δQ. Each of those parts will cause to

be performed the quantity of work represented by

$$\delta Q \, . \, \frac{d}{dQ} \, dW$$

consequently the quantity of work performed by the disappearance of heat will be

$$Q \, . \, \frac{d}{dQ} \, dW \text{..................(1.)}$$

which quantity is known when Q, and the law of variation of dW with Q, are known.

6. *Absolute Temperature: Specific Heat, Real and Apparent.*—*Temperature* is a function depending on the tendency of bodies to communicate the condition of heat to each other Two bodies are at *equal temperatures*, when the tendencies of each to make the other hotter are equal. All substances absolutely devoid of heat are at the same temperature. Let this be called the *Absolute Zero of Heat;* and let the scale of temperature be so graduated that for a given homogeneous substance, each degree shall correspond to an equal increment of actual heat. This mode of graduation necessarily leads to the same scale of temperature for all substances. For if two substances A and B be at equal temperatures when they possess respectively two certain quantities of actual heat Q_A and Q^B, then if each of those quantities of actual heat be divided into the same number of equal parts n, the tendency of the substance A to communicate heat to B, arising from any one of the nth parts of Q_A, must, from the property of actual heat already mentioned, be equal to the tendency of B to communicate heat to A, arising from any one of the nth parts of Q_B; from which it is easily seen, that so long as the quantities of actual heat possessed by the two substances are in the *ratio* $Q_A : Q_B$, their temperatures will be equal, independently of the *absolute amounts* of those quantities. The amount of actual heat, expressed in units of work, which corresponds, in a given substance, to *one degree of absolute temperature*, is the *Real Dynamical Specific Heat* of that substance, and is a constant quantity for all temperatures. The total quantity of mechanical work required to raise the temperature of unity of weight of a substance by one degree, generally includes, besides the real specific heat, work employed in overcoming molecular forces and external pressures. This is the *Apparent Dynamical Specific Heat;* and may be constant or variable.—Joule's Equivalent is the Apparent Dynamical Specific Heat of liquid water at and near its maximum density; and it is probably sensibly equal to the real specific heat of that substance. The real specific heat of each substance is constant at all densities, so long as the substance retains the same condition, solid, liquid, or gaseous; but a change of real specific heat, sometimes considerable, often accompanies the change between any two of those conditions. From the mutual proportionality of Actual Heat and Absolute Temperature, there follows—

7. THE SECOND LAW OF THERMO-DYNAMICS, *expressed with reference to* ABSOLUTE TEMPERATURE.—*If the Absolute Temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed will be equal.*—This law is expressed algebraically as follows:—from the relation between absolute temperature (t), and actual heat (Q), it follows that

$$t \frac{d}{dt} = Q \frac{d}{dQ}$$

consequently the expression 1, for the work performed by the disappearance of heat, is transformed into

$$t \, . \, \frac{d}{dt} \, dW \text{..................(2.)}$$

This expression is applicable, not merely to homogeneous substances, but to heterogeneous aggregates.—When the expressions 1 and 2 are negative they represent heat which appears in consequence of the expenditure of mechanical work in altering the condition of a substance.—The first and second laws virtually comprise the whole theory of Thermo-dynamics.

8. *Of Heat-Potentials and Thermo-dynamic Functions.*—The second law of Thermo-dynamics may also be expressed in the following form:—*The work performed by the disappearance of heat during any indefinitely small variation in the state of a substance, is expressed by the product of the absolute temperature by the variation of a certain function, which function is the rate of variation of the effective work performed with temperature.* That is to say, make

$$\frac{dW}{dt} = F;$$

then the work performed by the disappearance of heat is

$$t \, dF \text{......................(3.)}$$

This function F has been called the *Heat-Potential* of the given substance for the kind of work under consideration.—Now let the substance both perform work and undergo a variation of absolute temperature dt, and let K denote its real dynamical specific heat. The whole heat which it must receive from an external source of heat, to produce these two effects simultaneously, is

$$dH = K \, dt + t \, dF = t \, d\phi \text{ ;.........(4.)}$$

in which

$$\phi = K \, . \, \log_e t + \frac{dW}{dt} \text{ ....(5.)}$$

ϕ is called the *Thermo-dynamic Function* of the substance for the kind of work in question; and in some papers the *Heat-Factor.*—The equation (4) is the *general equation of Thermo-dynamics*

which we shall proceed, in the sequel, to apply, by determining the Thermo-dynamic Function for each particular case.—In determining this function, it is to be observed, that the function W, representing the work performed by the kind of change under contemplation, is first to be investigated as if the temperature were constant, and then the law of its variation with absolute temperature found.

9. *Perfect Gas, and other Thermometers.*—According to the customary mode of measuring temperatures, standard temperatures are fixed by phenomena which occur at them, such as the melting of ice, and the boiling of water under the mean atmospheric pressure; and the standard scale of temperatures is graduated according to the product of the pressure and volume of a given mass of a perfect gas. A perfect gas is a substance in such a condition, that the total pressure exerted by any number of portions of it, at a given temperature, against the sides of a vessel in which they are enclosed, is the sum of the pressures which each such portion would exert if enclosed in the vessel separately at the same temperature; in other words, a substance in which the elasticity of each appreciable particle, how small soever, is a property independent of the presence of other particles. Absolutely perfect gases are not found in nature; but air is sufficiently near to the condition of a perfect gas for thermometric purposes.—The ordinary zero of thermometric scales is the temperature of melting ice (as in the Centigrade and Reaumur's scales), or a point at an arbitrary number of degrees below that temperature, (for example, 32° in Fahrenheit's scale).—The *absolute zero of gaseous tension* is the temperature at which a perfect gas would exert no pressure, if it were possible to obtain a perfect gas at a temperature so low. This point, according to the most recent determination, is 274° Centigrade, or 493°·2 Fahrenheit, below the temperature of melting ice; that is, 461°·2 below the ordinary zero of Fahrenheit's scale.—Temperatures, as measured from the zero of gaseous tension, are expressed as follows:—Let t_0 be the temperature of melting ice, as so measured; t, any other temperature, also measured from the same point; $P_0 V_0$, the product of the pressure and volume of a given mass of a sensibly perfect gas at the temperature of melting ice; $P V$, the corresponding product at the temperature t, then

$$\frac{t}{t_0} = \frac{P V}{P_0 V_0} \quad \text{...............(6.)}$$

For one pound of air $P_0 V_0 = 26214$ foot-pounds nearly.—It was anticipated some years since, by certain theoretical and hypothetical investigations, that the scale of the perfect gas thermometer would be found to agree with the absolute thermometric scale, as to the length of its degrees; and also that the zeros of those scales would be found to be near each other, if not coincident. Throughout many of the papers referred to, the formulæ were so framed as to contain unknown terms, suited to provide for the possibility of a sensible difference between those zeros. But as, according to the latest and best experiments, no such appreciable difference has been found, the zero and scale of the perfect gas thermometer may be treated as sensibly, if not exactly, coincident with the absolute zero and absolute thermometric scale.—For temperatures not exceeding 300° Centigrade = 572° Fahrenheit, the apparent dilatation of mercury in glass is so nearly uniform as to be sufficient for the practical measurement of temperature. (See Regnault, *Mem. of the Acad. of Sciences*, 1847.)

10. *Dilatation of Gases.*—The coefficient of dilatation of a perfect gas, being the increase of volume under constant pressure, for one degree of rise of temperature, of so much of the gas as fills unity of space at the temperature of melting ice, is the reciprocal of the absolute temperature of melting ice, or

$$\frac{1}{493 \cdot 2} = 0 \cdot 0020276 \text{ per degree of Fahrenheit.}$$

This is a theoretical limit to which the coefficients of dilatation of gases approximate as their densities diminish and temperatures increase. Their actual coefficients of dilatation exceed this limit by small quantities depending on the nature, density, and temperature of the gas.

11. *Expansive Action of Heat in Fluids.*—Let V denote the volume in cubic feet occupied by a given mass of any fluid, whether liquid or gaseous, enclosed in a vessel of variable capacity, (such as a cylinder with a piston); P, the pressure, or effort to expand, which the fluid exerts against the interior of the vessel, in pounds per square foot; then will $P\,dV$ denote the effective or external work in foot-pounds, performed by the fluid during an indefinitely small expansion dV, and $\int P\,dV$ the effective work performed during any finite expansion, the relation between P and V being fixed by the circumstances of the case.—To find the Heat-Potential and the Thermo-dynamic Function for the expansion of a fluid, the pressure P is to be expressed in the form of a function of the volume V and absolute temperature t, and the general value of the integral

$$W = \int P\,dV$$

found, on the supposition that t is constant; then will

$$F = \frac{dW}{dt} = \int \frac{dP}{dt}\,dV$$

be the Heat-Potential, and the Thermo-dynamic Function will be

$$\varphi = K \,.\, \log_e t + \int \frac{dP}{dt}\,dV \quad \text{.....(7.)}$$

Applying this function to the determination, in foot-pounds, of the whole quantity of heat $d\,\text{H}$, which must be communicated to unity of weight of the fluid in order to produce simultaneously the indefinitely small variation of temperature $d\,t$ and the indefinitely small variation of volume $d\,\text{V}$, we find,

$$d\,\text{H} = t\left(\frac{d\,\phi}{d\,t}\,d\,t + \frac{d\,\phi}{d\,\text{V}}\,d\,\text{V}\right)$$

$$= \left(\text{K} + t\int_{\propto}^{\text{V}} \frac{d^2\,\text{P}}{d\,t^2}\,d\,\text{V}\right)d\,t + t\,\frac{d\,\text{P}}{d\,t}\,d\,\text{V} \quad (8.)$$

which is the general equation of the expansive action of heat in a fluid.—If this expression be analyzed, it is found to consist of the following parts:—I. The variation of the actual heat of unity of weight of the fluid $\text{K}\;d\,t$. —II. The heat which disappears in producing work by mutual molecular actions depending on change of temperature and not on change of volume $t\int_{\propto}^{\text{V}} \frac{d^2\,\text{P}}{d\,t^2}\,d\,\text{V}\,.\,d\,t$. The lower limit of this integral is made to correspond to the state of indefinite rarefaction, that is, of perfect gas, in which these actions are null. Let $\text{D} = \frac{1}{\text{V}}$ be the density, or weight of unity of volume of the fluid; then we have, as a more convenient form of the integral

$$\int_{\propto}^{\text{V}} \frac{d^2\,\text{P}}{d\,t^2}\,d\,\text{V} = \int_{0}^{\text{D}} -\frac{\frac{d^2\,\text{P}}{d\,t^2}}{\text{D}^2}\,.\,d\,\text{D} \quad (8\text{ A.})$$

III. The Latent Heat of Expansion, heat which disappears in performing work, partly by the forcible enlargement of the vessel containing the fluid, partly by mutual molecular actions depending on expansion, $t\,\frac{d\,\text{P}}{d\,t}\,d\,\text{V}$.

The heat, expressed in units of work, which must be communicated to unity of weight of a fluid to produce any given finite changes of temperature and volume, is found by integrating the expression 8. Now that expression is not the exact differential of any function of the temperature and volume; consequently its integral does not depend solely on the initial and final condition of the fluid as to temperature and volume, but also upon the mode of intermediate variation of those quantities.

NOTE.—The nature of the functions mentioned in this section is much elucidated by representing them geometrically. Let O X, O Y, be a pair of rectangular axes. Let abscissæ, measured from O Y parallel to O X, such as O a, O c, O d, O b, represent volumes assumed at different instants by a fluid mass, and ordinates, measured from O X parallel to O Y, such as a A, c C, d D, b B, the corresponding pressures. Then the co-ordinates of a number of points, A, C, D, B, will indicate a number of pressures and volumes assumed at different instants, and a curve, such as A C D B, will indicate by its co-ordinates the gradual changes of pressure and volume of the fluid mass during some given process. Let the process under consideration be expansion against a piston at a given absolute temperature t, from the volume O $a = \text{V}_\text{A}$, to the volume O $b = \text{V}_\text{B}$. The absolute temperature t being given, fixes the pressure P corresponding t any given volume V, and consequently determines the form of the curve A B.

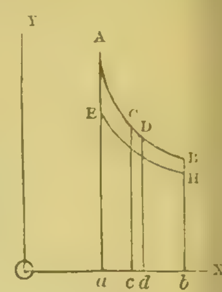

Fig. 1 A.

Let $c\,d = \Delta\,\text{V}$ represent the increase of volume during a given small portion of the expansion; and let the pressure at the beginning of this portion of the expansion be C c = P. The work performed by the action of the fluid against the piston during this small increase of volume will be *approximately* equal to the rectangle $\text{C}\,c \times c\,d = \text{P}\cdot\Delta\,\text{V}$, and the smaller $c\,d$, the closer will be the approximation; and if the whole expansion be subdivided into a number of small parts such as $c\,d$, the whole work performed by the action of the fluid against the piston will be *approximately* equal to the sum of all the rectangles such as $\text{P}\cdot\Delta\,\text{V}$, a sum denoted by the symbol

$$\sum_{\text{V}_\text{A}}^{\text{V}_\text{B}}.\left(\text{P}\,.\,\Delta\,\text{V}\right) \;\ldots\ldots\ldots\ldots\ldots\ldots\text{(a.)}$$

The smaller and the more numerous the parts such as $c\,d$ into which the expansion is divided, the more nearly does the sum of the rectangles expressed above approach to the *exact* value of the work performed, which is the area a A B b a, denoted by the symbol;

$$\text{W} = \int_{\text{V}_\text{A}}^{\text{V}_\text{B}} \text{P}\,d\,\text{V};\;\ldots\ldots\ldots\ldots\ldots\text{(b.)}$$

a symbol expressing the fact, that this area is the *limit* to which the sum of rectangles (a) approximates the more closely, the more numerous and the narrower the rectangles of which that sum is composed.—Now let the same expansion be undergone by the fluid mass at an absolute temperature

$$t - \Delta\,t$$

where $\Delta\,t$ is a small difference of temperature; and let the co-ordinates of the curve E H express the relation between the pressures and volumes successively assumed under these circumstances. The area a E H b a will represent the work performed during the expansion from V_A to V_B at this new temperature; and the area A B H E =

A B $b\,a$ — a E H $b\,a = \Delta$ W will be the *variation* the *work performed*, arising from the *variation* *temperature* $\Delta\,t$. Now the more the variation temperature $\Delta\,t$ is diminished, the more nearly ill the ratio

$$\frac{\Delta\, W}{\Delta\, t}$$

proximate to a certain *limiting ratio* denoted by

$$\frac{d\, W}{d\, t},$$

aich is the *rate of variation with temperature* the work

$$W = \int_{V_A}^{V_B} P\, d\, V$$

rformed by the fluid against a piston in expanding from the volume V_A to the volume V_B at e absolute temperature t. This rate of variation is also the difference $\phi_B - \phi_A$ between the dues of the Thermo-dynamic Function for the ro states of the fluid denoted by the points A id B on the diagram.

12. *Intrinsic Energy of a Fluid.*—Another ode of analyzing the expression (8) is as folws:—

I. The variation of actual heat, as before, K $d\,t$.

II. The *external work* performed, P d V.

III. The *internal work* performed, in overcomg molecular forces, viz:—

$$\int_{\infty}^{V} \frac{d^2\, P}{d\, t^2}\, d\, V\, .\, d\, t + \left(t\, \frac{d\, P}{d\, t} - P\right) d\, V.$$

ow this last quantity is the exact differential a function of the temperature and volume, iz:—

$$\int_{\infty}^{V} \left(t\, \frac{d\, P}{d\, t} - P\right) d\, V$$

$$= -\int_{V}^{\infty} \left(t\, \frac{d\, P}{d\, t} - P\right) d\, V = -\, U\, ; \quad (9.)$$

given value of U expresses the work required overcome molecular forces, in expanding unity weight of a fluid from a given state, to that perfect gas; and the excess of the actual eat of the fluid above this quantity, or

$$K\, t - U,$$

the *Intrinsic Energy* of the fluid, or the work hich it is capable of giving out, in changing om a given state as to temperature and volume, a state of total privation of heat and indefinite cpansion. Let the suffixes A, B, denote the ates of the fluid at the beginning and end any given series of changes of temperature id volume, and $H_{A,B}$, the supply of heat from an external source necessary to produce those changes; then

$$H_{A,\,B} - \int_{V_A}^{V_B} P\, d\, V = (K\,t - U)_B - (K\,t - U)_A \quad (10.)$$

that is to say:—*the excess of the heat absorbed above the external work performed is equal to the increase of the intrinsic energy;* so that this *excess* depends on the initial and final states only.

13. *Expression of the Thermo-dynamic Function in terms of the Temperature and Pressure.* The volume of unity of weight of a fluid V, its expansive pressure P, and its absolute temperature t, form a system of three quantities, of which, when any two are given, the third is determined. In the preceding sections, the *volume* and temperature are taken as independent variables. In some investigations it is convenient to take the *pressure* and temperature as independent variables, the volume being expressed as their function. The following expression of the Thermo-dynamic function in terms of this pair of independent variables is taken from an unpublished continuation, now in the hands of the Royal Society of Edinburgh, of one of the series of papers already referred to. Let t_0, as before, be the absolute temperature of melting ice; $P_0\, V_0$ the product of the pressure and volume of unity of weight of the fluid, in the *perfectly gaseous* state, at that temperature; then

$$\phi = \left(K + \frac{P_0\, V_0}{t_0}\right) \log_{\epsilon} t - \int_{0}^{P} \frac{d\, V}{d\, t}\, d P \quad (11.)$$

By the aid of the above equation, and of the following well known theorem,

$$\int_{V_A}^{V_B} P\, d\, V = \int_{P_B}^{P_A} V\, d P + P_B\, V_B - P_A\, V_A \quad (12.)$$

all the equations of the preceding sections are easily transformed.

14. *Principal Applications of the Laws of the Expansive Action of Heat.*—The relation between the temperature, pressure, and density of any particular substance being known by experiment, the principles of the preceding sections serve to compute the quantity of heat which will be absorbed or emitted under given circumstances; and conversely, in some cases when the quantities of heat absorbed or emitted under given circumstances are known by experiment, the same principles serve to determine relations between the temperature, pressure, and density of the substance. The chief subjects to which the principles of the expansive action of heat are applicable, are the following:—Real and Apparent Specific Heat:—The Heating and Cooling of Gases and Vapours by Compression and Expansion:—The Velocity of Sound in Gases:—

The Free Expansion of Gases:—The Latent and Total Heat of Evaporation of Fluids:—The Efficiency of Thermo-dynamic Engines:—The Latent Heat of Fusion. Most of these subjects have reference to substances wholly or partially in the gaseous condition; for it is in that condition only that we have power to regulate artificially the mode of expansion and contraction of substances to an extent appreciable in practice.

15. *Real and Apparent Specific Heat.*—These terms have been explained in a previous section. The symbolical expression for the apparent specific heat of a given substance, stated in units of work per degree of temperature in unity of weight, is as follows:—

$$\text{K}' = \frac{d\,\text{H}}{d\,t} = t\,.\frac{d\,.\,\phi}{d\,t} = \text{K} + t\,\frac{d\,.\,\text{F}}{d\,t}\ \ldots(13.)$$

In which the term K is the real specific heat, or that which actually makes the substance hotter, being a constant quantity; while the other term represents the heat which disappears in performing work, internal and external, for each degree of rise of temperature. The coefficients $\frac{d\,.\,\phi}{d\,t}$ and $\frac{d\,.\,\text{F}}{d\,t}$, represent respectively the rates of variation with temperature of the Thermo-dynamic function and heat-potential, under the circumstances of the particular case. With respect to liquids and solids, it is impossible to regulate artificially the mode of variation of the Thermo-dynamic function to an extent appreciable in practice. For substances in these states, the apparent specific heat increases with rise of temperature at a rate which is slow, but which appears, as theory would lead us to expect, to be connected with the rate of expansion. For Gases, the mode of variation of the Thermo-dynamic function with temperature may be regulated artificially in an arbitrary manner, so as to vary the apparent specific heat in an indefinite number of ways. It is customary, however, to restrict the term "Specific heat" in speaking of gases, to two particular cases; that in which the volume is maintained constant during the variation of temperature, and that in which the pressure is maintained constant. The specific heat at *constant volume*, is thus expressed in units of work per degree, being deduced from the expression 7, for the Thermo-dynamic function.

$$\text{K}_\text{v} = \text{K} + t\,.\int_{\infty}^{\text{V}} \frac{d^2\,\text{P}}{d\,t^2}\,.\,d\,\text{V}\ \ldots\ldots\ldots(14.)$$

For a theoretically perfect gas, $\text{K}_\text{v} = \text{K}$ ...(14 A.)

The specific heat under *constant pressure*, deduced from the expression 11 for the thermo-dynamic function, is as follows:—

$$\text{K}_\text{P} = \text{K} + \frac{\text{P}_0\,\text{V}_0}{t_0} - t\int_{0}^{\text{P}} \frac{d^2\,\text{V}}{d\,t^2}\,.\,d\,\text{P}\ldots(15.)$$

For a theoretically perfect gas,

$$\text{K}_\text{P} = \text{K} + \frac{\text{P}_0\,\text{V}_0}{t_0},\ \ldots\ldots\ldots(15\text{ A.})$$

being simply the real specific heat, increased by the work performed by unity of weight of the gas in undergoing, at any constant pressure, the expansion corresponding to one degree of rise of temperature; a quantity of work which is constant for a given perfect gas under all circumstances.—The quantities $\frac{d^2\,\text{P}}{d\,t^2}$ and $\frac{d^2\,\text{V}}{d\,t^2}$, representing the deviation of the laws of the elasticity of actual gases from those of the ideal condition of perfect gas, are so small, that their effects on apparent specific heat, though *calculable*, fall within the probable limits of errors of observation in the direct experiments hitherto made on the specific heat of the more common gases, such as air and carbonic acid. Referring, therefore, to the detailed papers already cited, for computations of the effects of such deviations, it will be sufficient for practical purposes to consider the specific heats of gases as represented by the formulæ 14A and 15A. The specific heats of gases, as expressed in the customary way, by their ratios to that of water, are found by dividing the quantities in these formulæ by Joule's equivalent (J), and may be thus expressed:—

$$c = \frac{\text{K}_\text{v}}{\text{J}};\quad c' = \frac{\text{K}_\text{P}}{\text{J}}.\ \ldots\ldots\ldots(16.)$$

Before the period of M. Regnault's experiments on a great variety of gases and vapours, published in the *Comptes Rendus* for 1853, no trustworthy direct experimental determination of the specific heat of any gas or vapour existed, except an approximate determination by Mr. Joule, made in 1852, of the specific heat of air. In one of the papers referred to in the preceding sections, however, (*Edinburgh Transactions*, 1850) the dynamical specific heats of air had been computed from the following data:—

$\text{P}_0\,\text{V}_0$, from M. Regnault's experiments, 26214 foot-pounds. $t_0 = 493°\cdot2$ Fahrenheit.

$\therefore\ \text{K}_\text{P} - \text{K}_\text{v} = \frac{\text{P}_0\,\text{V}_0}{t_0} = 53\cdot15$ foot-pounds per degree of Fahrenheit.

$\gamma = \frac{\text{K}_\text{P}}{\text{K}_\text{v}}$, as deduced from the velocity of sound in air, assumed in the paper referred to as approximately $= 1\cdot4$; but a more exact value is 1·408. Consequently,

$$\text{K}_\text{v} = \frac{\text{P}_0\,\text{V}_0}{t_0}\,.\,\frac{1}{\gamma - 1} = \frac{53\cdot15}{0\cdot408} = 130\ 3$$

foot-pounds per degree of Fahrenheit.

$$\text{K}_\text{P} = \frac{\text{P}_0\,\text{V}_0}{t_0}\,.\,\frac{\gamma}{\gamma - 1} = 53\cdot15 \times \frac{1\cdot408}{0\cdot408} =$$

183·45 foot-pounds per degree of Fahrenheit.

Hence is deduced the following ratio of the specific heat of air under constant pressure to that of water,

$$c' = \frac{\mathrm{K_P}}{\mathrm{J}} = \frac{183{\cdot}45}{772} = 0{\cdot}2377.$$

c' according to M. Regnault's experiments pub. 1853,	0·2379
Difference,	0·0002

M. Joule's approximate determination in 1832 was 0·23. According to the dynamical theory of heat, the apparent specific heat of a gas under constant pressure is *sensibly the same at all pressures and temperatures*, if the gas is nearly perfect.—According to the hypothesis of *substantial caloric*, that specific heat *diminishes as the pressure increases*, according to a law which is stated in most treatises on physics, even of the most recent dates (in some, indeed, as confidently as if it were an observed fact.)—The experiments of M. Regnault, by which the specific heat of air under constant pressure was determined at various temperatures from—22° Fahr. up to 437° Fahr., and at various pressures of from *one* to *ten* atmospheres, and found to be sensibly the same under all these circumstances, constitute "experimenta crucis" conclusive against that "idolon fori," the hypothesis of caloric. Those experiments also afford evidence of the fact, that the scale of the air thermometer sensibly agrees with that of absolute temperatures.

16. *Heating and Cooling of Gases and Vapours by Compression and Expansion.*—If a substance wholly or partially in the state of gas or vapour be enclosed in a vessel which does not conduct any appreciable amount of heat to or from the substance, then the compression and expansion of the substance through variations of the volume of the vessel will produce respectively heating and cooling, according to a law expressed by the condition, that the *Thermo-dynamic function is constant.*—The following equation contains two modes of expressing this condition, deduced from the expressions 7 and 11 respectively :—

$$\mathrm{K}\,.\log_e t + \int_{\infty}^{\mathrm{V}} \frac{d\,\mathrm{P}}{dt}\,d\,\mathrm{V} = \left(\mathrm{K} + \frac{\mathrm{P_0\,V_0}}{t_0}\right).$$

$$\log_e t - \int_0^{\mathrm{P}} \frac{d\,\mathrm{V}}{dt}\,d\,\mathrm{P} = \text{constant} \quad \ldots\ldots\ldots(16\ \mathrm{A})$$

For a perfect gas, we have

$$\frac{d\,\mathrm{P}}{d\,t} = \frac{\mathrm{P_0\,V_0}}{t_0\,\mathrm{V}}, \text{ and } \frac{d\,\mathrm{V}}{d\,t} = \frac{\mathrm{P_0\,V_0}}{t_0\,\mathrm{P}}; \quad \text{hence,}$$

let $\mathrm{P_1}$, $\mathrm{V_1}$ correspond to one given absolute temperature t_1, and $\mathrm{P_2}$, $\mathrm{V_2}$, to another given absolute temperature t_2; then for a perfect gas, or a gas sensibly perfect,

$$\left.\begin{aligned} &\log.\frac{t_2}{t_1} = (\gamma - 1)\log.\frac{\mathrm{V_1}}{\mathrm{V_2}} = \frac{\gamma - 1}{\gamma}\,.\log.\frac{\mathrm{P_2}}{\mathrm{P_1}} \\ &\text{or, } \frac{t_2}{t_1} = \left(\frac{\mathrm{V_1}}{\mathrm{V_2}}\right)^{\gamma - 1} = \left(\frac{\mathrm{P_2}}{\mathrm{P_1}}\right)^{\frac{\gamma - 1}{\gamma}} \end{aligned}\right\} (17.)$$

From equation 16 is easily deduced the law of the variation of the pressure with the volume of any fluid enclosed in a nonconducting vessel, viz. :—*the rate of variation of the pressure with the volume, when the fluid is enclosed in a nonconducting vessel, exceeds the rate of variation when the temperature is constant, in the ratio of the apparent specific heat of the fluid at constant pressure to its apparent specific heat at constant volume :*—a law expressed symbolically as follows :—

$$\frac{d\,.\,\mathrm{P}}{d\,.\,\mathrm{V}} = -\,\gamma\,.\,\frac{\frac{d\,\mathrm{P}}{d\,t}}{\frac{d\,\mathrm{V}}{d\,t}} \quad \ldots\ldots\ldots\ldots(18.)$$

For a perfect gas this becomes,

$\frac{d\,.\,\mathrm{P}}{d\,.\,\mathrm{V}} = -\,\gamma\,.\,\frac{\mathrm{P}}{\mathrm{V}}$, as equation 17 also shows.

The cooling of air by expansion has been applied by Dr. Gorrie to the manufacture of ice, and by Professor Piazzi Smyth and Mr. Rankine to ventilation.

17. *Velocity of Sound in Gases.*—The velocity of sound in any fluid is well known to be equal to that acquired by a heavy body in falling through one-half of the height which represents the variation of the pressure of the fluid with its density during a sudden change of density. That is to say, let v be the velocity of sound in feet per second, g the accelerating force of gravity in a second $= 32{\cdot}2$ feet per second, D the weight of one cubic foot of the fluid in pounds $= \frac{1}{\mathrm{V}}$, and P its elastic pressure in pounds per square foot; then

$$v = \sqrt{g\,.\,\frac{d\,\mathrm{P}}{d\,\mathrm{D}}}\,.$$

During the transmission of a wave of sound, the compression and expansion of the particles of a fluid take place so rapidly, that there is not time for any appreciable transmission of heat between different particles,* and the variations of the pressure and density are related to each other as they would be in a nonconducting vessel; consequently, if h represent the rate of variation of pressure with density at a constant temperature, then it follows from the principle of equation 18,

that $\frac{d\,\mathrm{P}}{d\,\mathrm{D}} = \gamma\,h$, and

$$v = \sqrt{g\,\gamma\,h} \quad \ldots\ldots\ldots\ldots\ldots(19.)$$

This equation was long since proved by Laplace,

* Proved by Prof. G. G. Stokes.

and Poisson for perfect gases, for which

$$h = \text{P V} = \frac{\text{P}_0 \text{V}_0}{t_0} t, \text{ ....(19 A.)}$$

but it is true, as we have seen, for all fluids whatsoever.—Applying the formula to air, considered as a sensibly perfect gas, with the following data:—

$$\gamma = 1{\cdot}408; \quad \text{P}_0 \text{V}_0 = 26214; \quad t = t_0;$$

	Feet per second.
The following is found to be the velocity of sound in pure dry air at the temperature of melting ice............	1090·2
The velocity by experiment is:—	
According to MM. Bravais and Martins	1090·5
According to MM. Moll and Van Beek	1090·1

Experiments on the velocity of sound serve to determine the ratio γ of the specific heats of a gas at constant pressure and at constant volume. For Oxygen, Hydrogen, and Carbonic Oxide, it is sensibly the same as for air; for Carbonic Acid, considerably less. (*Edinburgh Transactions*, vol. xx.)

18. *Free Expansion of Gases and Vapours.*—When the expansion of a gas takes effect, not by enlarging the vessel in which it is contained, and so performing work on external bodies, but by propelling the gas itself from a space in which it is at a higher pressure P_A into a space in which it is at a lower pressure P_B, a portion of work represented by

$$\int_{\text{P}_\text{B}}^{\text{P}_\text{A}} \text{V}\, d\,\text{P},$$

is employed wholly in agitating the particles of the gas; and when the agitation so produced has entirely subsided through the mutual friction of those particles, an equivalent quantity of heat is developed, which neutralizes the previous cooling, wholly if the gas is perfect, partially if it is imperfect. The equation representing the result of this process is the following:—

$$\int_{\phi_\text{A}}^{\phi_\text{B}} t\, d\,\phi = \int_{\text{P}_\text{B}}^{\text{P}_\text{A}} \text{V}\, d\,\text{P} \text{ ....(20.)}$$

In this equation, let the Thermo-dynamic function be expressed in terms of the temperature and pressure, as in equation 11, and let K_P be put for its value, according to equation 15; then we have

$$\int_{t_\text{B}}^{t_\text{A}} \text{K}_\text{P} \,.\, d\,t = \int_{\text{P}_\text{B}}^{\text{P}_\text{A}} \left(t \frac{d\,\text{V}}{d\,t} - \text{V} \right) d\,\text{P} \text{(20 A.)}$$

This quantity represents the amount whereby the heat reproduced by friction falls short of that which disappears during the expansion, and for a perfect gas is null. The phenomenon here in question, was first observed by Mr. Joule, and Professor William Thomson, jointly, to determine experimentally the relation between the absolute scale of temperature, and that of the air thermometer, which had previously been to a considerable extent a matter of conjecture and hypothesis. In such experiments the variation of temperature which takes place is very small, hence we may put approximately

$$\text{K}_\text{P}\, \Delta\, t = \left(t \frac{d}{d\,t} - 1 \right) \int_{\text{P}_\text{B}}^{\text{P}_\text{A}} \text{V}\, d\,\text{P} \text{ ...(21.)}$$

where t is the mean of t_A and t_B, and

$$\Delta\, t = t_\text{A} - t_\text{B}$$

is the final cooling effect.—Let T represent temperature measured by the air thermometer on the ordinary scale, and k the dynamical specific heat of the gas under constant pressure as referred to this scale, which is formed by multiplying the specific heat as given by M. Regnault, by Joule's equivalent. Let the absolute temperature t be regarded as a function of T,

$$t = f(\text{T})$$

whose form is to be ascertained. Then for equation 21 we may put

$$k\, \Delta\, \text{T} = \left(\frac{f(\text{T})}{f'(\text{T})} \cdot \frac{d}{d\,\text{T}} - 1 \right) \int_{\text{P}_\text{B}}^{\text{P}_\text{A}} \text{V}\, d\,\text{P} \text{ (22.)}$$

Each experiment on cooling by free expansion, gives a value of the cooling effect $\Delta\,\text{T}$, corresponding to a particular pair of pressures P_A, P_B. The relations between P, V, and T, are given by formulæ, founded on M. Regnault's experiments on the elasticity of gases* (see a paper on the Centrifugal Theory of Elasticity, *Edinburgh Transactions*, vol. xx; also *Philosophical Magazine* for December 1851). Consequently, from each experiment on free expansion, there can be calculated the value of $\frac{f'(\text{T})}{f(\text{T})} = \frac{d \,.\, \log_e t}{d\,\text{T}}$, for a particular temperature T on the air thermometer. This function, when multiplied by Joule's equivalent, is called Carnôt's function, being a function of which Carnôt pointed out the existence, but failed, from reasons already stated, to discover the form. Those experiments are still in progress; and so far as they have yet been carried (having been made on Air and Carbonic Acid), they indicate, that the absolute zero of heat does not appreciably differ from that of gaseous tension, and that the scale of absolute temperature sensibly coincides with that of the perfect gas thermometer. *Philos. Trans.* 1854). This fact having been established, experiments on free expansion become an easy and accurate means of ascertaining the relations between the pressures, temperatures, and densities of various

* These formulæ will be described generally in the sequel of this article.

elastic fluids. Experiments on the free expansion of steam have been made by Mr. C. W. Siemens, and show (as theory leads us to expect), that steam, after having been freely expanded, is *superheated*, or above the temperature of saturation corresponding to its pressure.

19. *Latent Heat of Evaporation.*—It is known by experiment, that the pressure under which a fluid boils at a given temperature, (being the least pressure under which it can exist in the liquid state, and the greatest under which it can exist in the gaseous state, at the given temperature), is a function of the temperature only. Let v be the volume occupied by unity of weight of a fluid, when in the liquid state, at the absolute temperature t, and under the corresponding pressure of ebullition P, and V the volume of the same weight when in the state of saturated vapour at the same pressure and temperature. Then on applying equation 8 to this case, we find that because the temperature is constant, the first term is $= 0$; and because the pressure is constant, the factor $t \frac{dP}{dt}$ of the second term is constant; so that the integral is

$$H = t \frac{dP}{dt} (V - v) \text{ ....(23.)}$$

which is the value in units of work, of the heat which disappears in evaporating unity of weight of the fluid at the given temperature. Now suppose the weight of fluid evaporated to be $\frac{1}{V - v}$; that is to say, so much of the fluid, that its increase of bulk in the act of evaporating is one cubic foot; then

$$L = \frac{H}{V - v} = t \frac{dP}{dt} \text{ ....(24.)}$$

will be the *Latent Heat of Evaporation in foot-pounds per cubic foot of space.* This law enables us to compute the expenditure of heat necessary to propel a piston through a given space, by means of a given vapour *at full pressure* and at any temperature, simply from the relation between the temperature and the pressure of ebullition, and without knowing the density of the vapour. The utility of this mode of computation is evident from these considerations:—that the weight of fluid expended in any vapour engine is of secondary importance in comparison with the space which it is capable of filling under a given pressure, and with the expenditure of heat,—and that the density of most vapours is only known approximately, and in a great measure by indirect methods of computation. The rate of increase of the pressure of ebullition with the temperature $\frac{dP}{dt}$, may be computed either from a table of such pressures for the fluid in question (such as those given by M. Regnault in the *Memoirs* and *Comptes Rendus* of the Academy of Sciences), or from formulæ of the following form:—

$$\log._{10} P = A - \frac{B}{t} - \frac{C}{t^2} \text{ ......(25.)}$$

$$L = t \frac{dP}{dt} = P \left(\frac{B}{t} + \frac{2C}{t^2} \right) . \log_e 10 \text{ (25 A.)}$$

$$(\log_e 10 = 2{\cdot}3026 \text{ nearly}).$$

The *form* of these equations was indicated by a mechanical hypothesis of which a sketch will be given in a subsequent section; the constants for certain fluids, as computed from M. Regnault's experiments, are given below, for pressures in *pounds on the square foot*, and absolute temperatures in degrees of Fahrenheit.

	A.	log.10 B.	log.10 C.
Water..........	8·25907	3·4364155	5·5987307
Alcohol..........	7·9707	3·3123335	5·7532255
Æther..........	7·5732	3·3149229	5·2170580
Bisulphuret of Carbon	7·3438	3·3072774	5.2183876
Mercury..........	7·9691	3·7228362	..

20. *Computation of the Density of Vapour from the Latent Heat.*—The densities of vapours are but imperfectly known by direct experiment. The density of a vapour at saturation at a given temperature may be computed indirectly in the following manner:—L being, as above, the latent heat per cubic foot, and H, the latent heat per pound of the fluid, ascertained by experiments (such as those of M. Regnault on water, and of Dr. Andrews on other fluids). Then

$$V - v = \frac{H}{L} \text{ ....(25 B.)}$$

is the increase of volume of one pound of the fluid in evaporating, from which the density of the vapour is easily calculated. The densities, thus computed, of the vapours of Æther and Sulphuret of Carbon, at their boiling points under the mean atmospheric pressure (2116·4 lb. per square foot) agree almost exactly with those computed from the chemical composition of these vapours, supposing them to be perfectly gaseous. The densities of the vapours of water and alcohol as computed from their latent heats of evaporation, are greater than those corresponding to the perfectly gaseous state. For steam at low pressures the difference is trifling, but increases rapidly as the pressure increases. (*Proc. Roy. Soc. Edin.* 1855).

Example. $P = 2116{\cdot}4$ (one atmosphere).

	Æther.	Bisulph. of Carbon.	Water.
Boiling points (ordinary scale)	95°	114°·8	212°
Weight of one cubic foot of vapour:—			
Calculated from latent heat	0·1853 lb.	0·1829 lb.	0·03790 lb.
Calculated as perfect gas	0·1856	0·1830	0·03679

21. *Total Heat of Evaporation.*—The total heat of evaporation of unity of weight of a fluid, *from* one temperature, *at* another tempera-

ture, is the quantity of heat required to raise the temperature of unity of weight of the fluid from the first temperature to the second, and then to evaporate it at the second temperature. Some fixed temperature, such as that of melting ice, is usually taken for the first temperature. It is deducible from equation 12, that the total heat of evaporation of unity of weight of a fluid, whose vapour is sensibly a perfect gas, and very bulky as compared with the liquid, from t_0, at t_1, is sensibly equal to

$$\text{H}_0 + \text{K}_\text{P} (t_1 - t_0) \ldots\ldots\ldots\ldots (26.)$$

Steam is not a perfect gas; and its total heat of evaporation as ascertained by experiment, is expressed by

$$\text{H}_0 + a (t_1 - t_0) \ldots\ldots\ldots (26\text{ A.})$$

in which a is a certain constant, less than the specific heat under constant pressure, K_P.—According to M. Regnault's experiments, let t_0 be the temperature of melting ice; then

$\text{H}_0 = 1091{\cdot}7$ F. units $\times 772 = 842792$ ft.-pounds.

$a = 0{\cdot}305 \times 772 = 235{\cdot}46$ foot-pounds per degree of Fahrenheit.*

22. *Thermo-dynamic Engines, and their Efficiency.*—A Thermo-dynamic Engine is a piece of mechanism in which the variations of volume and pressure of an elastic fluid, recurring periodically in a cycle, are so regulated as to produce at each cycle of variations, or *stroke*, a permanent disappearance of heat, and development of motive power; which development of motive power is necessarily equivalent to the heat which disappears. This heat, in other words, is said to be *transformed* into motive power. The *Efficiency* of a Thermo-dynamic Engine is a fraction, representing the proportion which the heat transformed into motive power at each stroke, bears to the whole heat expended at each stroke. This proportion, in an engine in which no heat or power is wasted, depends solely (as will presently be shown) upon the temperatures at which the elastic fluid receives and rejects heat.

23. *Diagrams of Energy. Isothermal and Adiabatic Lines.*—The principles of the action of Thermo-dynamic engines, as well as all other engines, are much elucidated by the aid of geometrical figures, called *indicator-diagrams* when they are described by apparatus attached to actual engines, and generally *diagrams of energy.* (See fig. 1.) In such a diagram abscissæ, measured from OY parallel to OX, such as Oa, Ob, Oc, Od, represent volumes successively assumed by the elastic fluid; ordinates, measured from OX parallel to OY, such as aA, bB, cC, dD, represent pressures exerted by the fluid. The relation between the pressures and volumes successively assumed by the fluid during a given process, is expressed by a line, straight or curved, such as AB, the co-ordinates of any point in which represent a volume and the corresponding pressure. Let the process be an expansion from the volume Oa to the volume Ob, the pressures being the ordinates of the curve AB. Then the area aABb will represent the work performed by the elastic fluid in propelling a piston during the expansion in question. If the process were a compression, from Ob to Oa, then the same area would represent work exerted by the piston on the fluid. A *stroke*, or *cycle of processes*, at the end of which the elastic fluid returns to its original condition, is represented by a closed line, such as ABCD; and when the changes take place in the direction of the letters, so that the pressure is, on the whole, less during the compression than during the expansion, the *area enclosed within this line* represents at once the motive power permanently communicated to the piston during one cycle, or stroke, and the heat which permanently disappears. The lines on a diagram of energy may be of various kinds:—for example:—An *Isothermal Line* represents the relation between the pressure and volume at a given constant temperature. Its equation is $t =$ constant. A *Line of No Transmission*, or more briefly, an *Adiabatic Line*, represents the relation between the pressure and volume during the expansion and compression of the fluid in a nonconducting vessel; consequently, for such a line, the Thermo-dynamic function is constant; and its equation is, $\phi =$ constant. The *Athermal Line* corresponds to the absolute zero of heat. It is at once an Adiabatic Line and an Isothermal Line, and is an asymptote to all other Isothermal and Adiabatic Lines. For a perfect gas, the Athermal Line sensibly coincides with the axis OX.

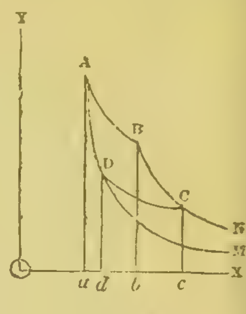

Fig. 1.

24. *Efficiency of an Elementary Thermo-dynamic Engine.*—An Elementary Thermo-dynamic Engine is one in which the reception of heat by the elastic fluid takes place wholly at one absolute temperature t_1, and its rejection wholly at another absolute temperature t_2. Consequently, in such an Engine the change between those two limiting temperatures must be made entirely by compression and expansion of the fluid.—In fig. 1, let AB be part of the isothermal line of t_1, DC part of that of t_2; and let ADM, BCN, be a pair of adiabatic lines, corresponding respectively to any two thermo-dynamic functions ϕ_A, ϕ_B, and produced indefinitely towards X.—Then will ABCD be the diagram of an elementary thermo-dynamic engine receiving heat at t_1 and rejecting heat at t_2.—The heat received from the furnace, at each stroke, during the process

* The form of equation 26 A. was hypothetically anticipated by Sir John Lubbock in 1840.

A B, is $t_1 (\phi_B - \phi_A) = H_1$, and is represented by the indefinitely-produced area M A B N.—The heat rejected at each stroke, during the process C D, and abstracted by some refrigerating substance (such as the jet of cold water in the condenser of a steam engine) is $t_2 (\phi_B - \phi_A) = H_2$, and is represented by the indefinitely-produced area M D C N. — The heat permanently transformed into motive power at each stroke is represented by the area A B C D

$$\left.\begin{array}{l} = H_1 - H_2 = (t_1 - t_2)(\phi_B - \phi_A) \ldots\ldots \\ \text{Consequently the } \textit{efficiency of the engine} \text{ is} \\ \dfrac{H_1 - H_2}{H_1} = \dfrac{t_1 - t_2}{t_1} \ \ldots\ldots\ldots\ldots \end{array}\right\} (27.)$$

The last equation expresses the *law of the efficiency of elementary thermo-dynamic engines.*

25. *Efficiency of Thermo-dynamic Engines in General.*—Let the closed line A a b B c d A be the diagram of any thermo-dynamic engine. Draw a pair of Adiabatic lines A M, B N, touching the closed line in A, B, respectively, and indefinitely produced in the direction O X. Then throughout the process represented by the part A a b B of the diagram, the fluid is receiving heat, and throughout the process is represented by the part B c d A, rejecting heat. — Cut an indefinitely narrow band from the diagram by any pair of indefinitely-close Adiabatic lines $a\,d\,m$, $b\,c\,n$, corresponding to the Thermo-dynamic functions ϕ, $\phi + d\,\phi$, respectively; and let the absolute temperatures corresponding to the elements $a\,b$, $c\,d$, be t_1, t_2, respectively. Then, treating the band $a\,b\,c\,d$ as the diagram of an elementary engine, we find—

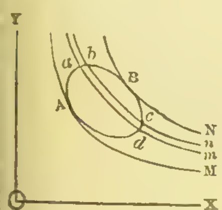

Fig. 2.

Heat received during the process $a\,b$ = indefinitely-produced area $m\,a\,b\,n = d\,H_1 = t_1\,d\,\phi$;

Heat rejected during the process $c\,d$ = indefinitely-produced area $m\,d\,c\,n = d\,H_2 = t_2\,d\,\phi$;

Heat transformed into motive power = area $a\,b\,c\,d$ $= d\,H_1 - d\,H_2 = (t_1 - t_2)\,d\,\phi$.

Consequently, whole heat received by the fluid per stroke,

$$\left.\begin{array}{l} = \text{area M A } a\,b \text{ B N} = H_1 = \displaystyle\int_{\phi_A}^{\phi_B} t_1\,d\,\phi; \\ \text{Heat rejected per stroke,} \\ = \text{area M A } d\,c \text{ B N} = H_2 = \displaystyle\int_{\phi_A}^{\phi_B} t_2\,d\,\phi; \\ \text{Heat transformed into motive power per stroke,} \\ = \text{area A } a\,b \text{ B } c\,d \text{ A} = H_1 - H_2 \\ \qquad = \displaystyle\int_{\phi_A}^{\phi_B} (t_1 - t_2)\,d\,\phi. \end{array}\right\} (28.)$$

26. *Thermo-dynamic Engines of Maximum Efficiency.—Between given limits of temperature,* the efficiency of a Thermo-dynamic engine is the greatest possible, when the whole reception of heat takes place at the higher limit, and the whole rejection of heat at the lower; that is to say, when the engine is an *elementary engine;* and the theoretical efficiency of such an engine is independent of the nature of the fluid employed.

27. *Of the Heat-Economizer, or Regenerator.* —To fulfil strictly the above condition of maximum efficiency between given limits of temperature, the elevation of the temperature of the fluid must be performed wholly by compression, and the depression of its temperature wholly by expansion: operations which are in many cases impracticable, from the great bulk of cylinders which their performance would require.—This difficulty is almost entirely avoided by the following process, for producing alternate elevation and depression of temperature with a small expenditure of heat, invented about the year 1816 by the Rev. Doctor Robert Stirling, and subsequently improved and modified by Mr. James Stirling, Captain Ericsson, and others.—The fluid whose temperature is to be lowered is passed through the interstices of an apparatus called an *Economizer* or *Regenerator*, formed by a number of thin plates of metal or other solid conducting substance, or of a network of wires, exposing a great surface within a small space. The material of the economizer becomes heated by the cooling of the fluid. When the temperature of the fluid is again to be raised, it is passed through the interstices of the economizer in the contrary direction, and the heat which it had previously given out is in part restored to it.—It is impossible to perform this process absolutely without waste of heat; but by giving a sufficient mass and surface to the economizer, the waste may be reduced to a small amount. In some experiments by Mr. Siemens, on air, the waste of heat at each stroke appears to have been about one-twentieth part of the heat alternately abstracted from and restored to the air.

27A. *Isodiabatic Lines.* — One condition of the economical working of the economizer is, that the quantity of heat given out by the fluid during any given stage of the lowering of its temperature shall be equal to the quantity received by it during the corresponding stage of the raising of its temperature. This condition is realized in the following manner.—Let E F be an arbitrary line representing the mode of variation of the pressure and volume of the fluid during the lowering of its temperature.—Let G H be the

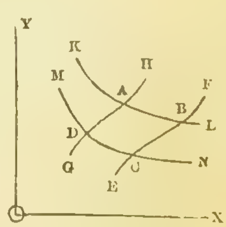

Fig. 3.

corresponding line for the raising of the temperature of the fluid.—Let K L, M N, be any pair of isothermal lines, intersecting G H in A and D, and E F in B and C, respectively. Let ϕ_A, ϕ_B, ϕ_C, ϕ_D, be the Thermo-dynamic functions for these four points. Then if, for every possible pair of isothermal lines,

$$\phi_B - \phi_A = \phi_C - \phi_D,$$

the lines E F and G H have the required property, and are said to be Isodiabatic with respect to each other.

28. *Thermic Lines for a Perfect Gas.*—Each *Isothermal* Line for a Perfect Gas is a common rectangular hyperbola, whose asymptotes are O X, O Y, its equation being,

$$P\,V = \frac{t}{t_0} P_0\,V_0 = \text{constant}\ldots\ldots\ldots(29.)$$

Each *Adiabatic* Line for a Perfect Gas is a curve of the hyperbolic kind, having O X, O Y, for asymptotes, its equation being

$$P\,.\,V^{\gamma} = \varepsilon^{\frac{\phi}{K}} = \text{constant}\ldots\ldots\ldots\ldots(30.)$$

Each *pair of Isodiabatic* Lines for a Perfect Gas are so related to each other, that if V, V′, be the abscissæ of the points of intersection of these two lines respectively with one and the same isothermal line, the ratio V: V′ is a constant quantity for all isothermal lines. The same is the case with the ratio P: P′. It follows from this, that all straight lines of constant volume, parallel to O Y, are mutually isodiabatic, (which is equivalent to saying that the specific heat at constant volume is constant), and also that all straight lines of constant pressure, parallel to O X, are mutually isodiabatic, (which is equivalent to saying that the specific heat under constant pressure is constant.)

29. *Thermic Lines for Saturated Vapour.*—When a fluid mass at a given temperature is under the pressure of ebullition for that temperature, it may be either wholly in the liquid state, wholly in the state of saturated vapour, or partially in those two states, and may occupy any volume from that of complete liquefaction to that of complete evaporation. Hence the *Isothermal* line for this condition is a limited straight line parallel to O X; and the distances of the extremities of that line from O Y represent the volumes of the mass in the states of liquid and of saturated vapour respectively, at the given temperature.—The exact equation of an *Adiabatic* curve for a mixture of liquid and saturated vapour is very complex. Examples of its application may be found in *Philosophical Transactions*, 1854, 1859, and Rankine *On Prime Movers*, 1859. This curve, however, in most of the cases which occur in practice, approaches to the form of a sort of hyperbola, and computations in which it is treated as of the hyperbolic kind are found to be sufficiently accurate for most practical purposes connected with steam engines. In actual indicator-diagrams of steam engines, only one adiabatic or nearly-adiabatic curve occurs, viz.:—that representing the cooling of the steam by expansion. To realize the theoretical maximum of efficiency, the temperature of the water supplied to the boiler ought to be raised to the temperature of ebullition by forcibly compressing a portion of the waste steam into the liquid state; but this process would be inconvenient, if not impossible, in practice, and its omission occasions but a small loss of efficiency. The nearest approach possible in practice to the maximum theoretical efficiency in a steam engine or other vapour engine, is attained when the expansion is continued until the pressure of the vapour falls so as to be just sufficient to balance the unloaded friction of the engine, added to the pressure at which the condensation takes place in condensing engines, or the rejection of the vapour in non-condensing engines.

30. *Composite Vapour Engines.*—In a Composite Vapour Engine, two fluids are employed, a less and a more volatile, such as water and æther, in the engine of M. Du Trembley. In this engine, the steam, instead of being expanded to and condensed at the lowest attainable limit of temperature, is expanded to and condensed at some intermediate temperature, and the heat rejected by it in the act of condensation is used to evaporate the æther, which works an auxiliary engine, and is condensed at the lowest limit attainable. The æther merely performs a portion of work which the steam would have performed if its expansion had been continued far enough; but as the pressure of the saturated vapour of æther at a given temperature is much greater than that of steam, the æther cylinder occupies a much smaller space than that which would have to be added to the steam cylinder to enable it to realize the same efficiency.

31. *Economy of Heat-Engines in general.*—If the number of British Fahrenheit-Units of heat produced by the combustion of one pound of a given kind of fuel, be multiplied by Joule's equivalent, 772 foot-pounds, the result is the *total heat of combustion* of the fuel in question, expressed in foot-pounds. In different kinds of coal, it varies from about 6,000,000 to 12,000,000 foot-pounds. This total heat is expended, in any given engine, in producing the following effects, whose sum is equal to the heat so expended. —1. The *waste-heat of the furnace*, being from 0·15 to 0·6 of the total heat, according to the construction of the furnace, and the skill with which the combustion is regulated. — 2. The *necessarily rejected heat of the engine*, being, as hereinbefore stated $= \frac{t_2}{t_1} \times$ the heat received by the engine, t_1 being the upper, and t_2 the lower limits of temperature.—3. The *heat wasted by the engine*, whether by conduction, or by non-fulfilment of the conditions of maximum effi-

ency.—4. The *useless work of the engine*, employed in overcoming friction and other prejudicial resistances.—5. The *useful work*. The economy of a Thermo-dynamic Engine is improved by diminishing as far as possible the first four of these effects, so as to increase the fifth. It is with the diminution of the second and third effects that the theory of Thermo-dynamics is chiefly concerned; and this is to be effected,—first, by so regulating the working of the elastic fluid, that the reception and rejection of heat shall take place as far as possible at the upper and lower limits of temperature respectively, so as to diminish the heat wasted by the engine, and secondly, by diminishing as far as possible the ratio $\frac{t_2}{t_1}$ of the lower limit of absolute temperature to the upper, to which ratio the necessarily-rejected heat is proportional. In saturated steam engines and other vapour engines, the former of these objects has already been effected perhaps to the fullest extent possible in practice:—and that, it may be observed, almost wholly by carrying out and developing the ideas of Watt. There therefore remains, as the means of increasing the economy of heat-engines, only the diminution of the ratio $\frac{t_2}{t_1}$. Now the diminution of the lower limit of absolute temperature, t_2, is restricted by the temperature of the surrounding air and water; so that the increase of economy must be accomplished chiefly by raising the upper limit of temperature t_1. This, in the case of saturated steam, has already been carried nearly as far as is possible, consistently with safety, owing to the rapid increase of the pressure of ebullition with the temperature. Hence, it is to be concluded, that in order to improve the economy of Thermo-dynamic engines to any great extent beyond what has already been accomplished, it is necessary to use elastic fluids in the gaseous state, so that it may be possible to increase the upper limit of temperature to any extent consistent with the durability of the solid materials of the engine, and at the same time to regulate the pressure at will, by regulating the density of the gas employed. The gaseous substances best suited to this purpose are simply those which are most readily obtained:—viz., superheated steam, and atmospheric air. The choice between those two substances is wholly a question of practical convenience.* The efficiency of the steam in actual steam engines ranges from 0·02 to 0·2 (see Steam Engine).

32. *Latent Heat of Fusion.*—When Freezing and Melting are accompanied by a change of volume, the *Latent Heat of Fusion* is subject to a law analogous to that expressed by the equation (23.) for the latent heat of evaporation: viz., let v be the volume of unity of weight of the substance in the liquid state, v' the volume in the solid state, t the absolute temperature of fusion, and $\frac{d\,\text{P}}{d\,t}$ the reciprocal of the rate at which that temperature varies with the external pressure under which fusion takes place; then the latent heat of fusion, in units of work, is

$$\text{H} = t\,\frac{d\,\text{P}}{d\,t}\,(v - v') \qquad \text{(31.)}$$

When the latent heat and temperature of fusion and the alteration of volume $v - v'$, are known by experiment for a given substance, the alteration of the temperature of fusion by pressure may be computed by the following formula:—

$$\frac{d\,t}{d\,\text{P}} = \frac{t\,(v - v')}{\text{H}} \qquad \text{(31 a.)}$$

When the bulk of the substance in the solid state exceeds that in the liquid state (as is the case for water, antimony, cast-iron, and a few other substances), then $\frac{d\,t}{d\,\text{P}}$ is negative, that is, the temperature of fusion is lowered by pressure; a principle first pointed out by Mr. James Thomson as a consequence of Carnôt's theory (*Edinburgh Transactions*, vol. xvi.) For water we have the following data:—

$v = 0{\cdot}016$ cubic foot per pound,
$v' = 0{\cdot}0174$ „ „
$t = 493^\circ{\cdot}2$ Fahr.
$\text{H} = 142 \times 772 = 109624$ foot-pounds;

consequently, $-\frac{d\,t}{d\,\text{P}} = 0{\cdot}0000063$ Fahrenheit, being the amount by which the melting point of ice is lowered for each pound of pressure on the square foot. An *atmosphere* of pressure being 2116 lb. per square foot, we have, for the lowering of the melting point per atmosphere of pressure,

$$2116 \times \left(-\frac{d\,t}{d\,\text{P}}\right) = 0^\circ{\cdot}0133 \text{ Fahrenheit},$$

a result verified by the experiments of Professor William Thomson.

33. *Cosmical Speculations connected with Thermo-dynamics. Solar Heat.*—Various speculations concerning the application of the principles of Thermo-dynamics to the phenomena of the universe in general have recently appeared, of which the most interesting is one originated by Mr. Waterston (*Report of the British Association* for 1853), and reduced to numerical computation by Professor Thomson (*Edinburgh Transactions*, xxi.), respecting the source of the light and heat of the sun. Mr. Waterston proposes the supposition, that the solar heat is produced by

* For investigations connected with Air Engines, besides the papers already referred to, see those of Professor Norton and Professor Barnard, in the *American Journal of Science* for 1853.

friction of a shower of meteoric or small planetary bodies, continually falling from surrounding space into the atmosphere or on the surface of the sun. Professor Thomson shows, that the annual fall of a quantity of matter into the sun, which would increase the angular diameter of that body as seen from the earth by only *one second in forty thousand years*, is sufficient, according to this supposition, to account for the quantity of light and heat annually emitted by the sun at the present time; whereas, on the supposition that the sun's light and heat are produced by chemical action, that is, by combustion or by electricity, at least *three thousand times* the above quantity of matter would require to enter annually into chemical combination at or near the sun's surface If combustible matter be supposed to fall into the sun, such matter would produce at least three thousand times more heat by its mechanical action than by its combustion. Professor Thomson considers the Zodiacal light as being probably a cloud of meteoric bodies in the act of gradually approaching and falling into the sun.

34. *Hypothesis of Molecular Vortices.*—In the preceding sections, the laws of Thermo-dynamics have been stated simply as the general expressions of facts ascertained by experiment, without adopting any hypothetical assumptions. In this, as well as in other branches of molecular physics, the laws of phenomena have to a certain extent been anticipated, and their investigation facilitated, by the aid of hypotheses as to occult molecular structures and motions with which such phenomena are assumed to be connected.—The hypothesis which has answered this purpose in the case of Thermo-dynamics, is called that of "Molecular Vortices," or otherwise, the "Centrifugal Theory of Elasticity."—The fundamental suppositions of this hypothesis are,—that bodies consist of *atoms;* this word being used to denote, not absolutely indivisible particles, but the *smallest similar parts* into which a homogeneous substance can be divided;—that each atom consists of a simple or compound nucleus surrounded by an elastic atmosphere;—that the tendency to preserve particular volumes and figures, possessed by the parts of solids, and the tendency to preserve particular volumes possessed by the parts of liquids, arise from forces exerted between the atomic nuclei and the parts of their atmospheres;—that radiant light and heat consist in the transmission of oscillations of the atomic nuclei;—and especially, that thermometric heat, which is accompanied by a tendency to perform work by indefinite expansion, and a diminution of the tendency to preserve a definite volume and figure, consists in *circulating currents*, *eddies*, or *vortices*, amongst the particles of the atomic atmospheres, giving them a tendency to recede from their nuclei, and occupy a greater space, and also to lose any particular geometrical distribution as to density and arrangement.—According to this hypothesis, the *actual heat* of a substance is the total *vis-viva* of the molecular motions (the *vis-viva* of a body being understood to mean, the product of the mass of the body by *one-half* of the square of its velocity).—From this hypothesis all the laws of Thermo-dynamics are deduced by the aid of the principles of ordinary mechanics, leaving one point to be ascertained by experiment, viz.:—the interval, if any, on the thermometric scale, between the absolute zero of heat and the zero of gaseous tension;—an interval which, as already stated, is inappreciable, according to the best existing experiments.

35. *Elasticity of Gases and Vapours.*—Besides the Laws of Thermo-dynamics, the hypothesis of Molecular Vortices leads also to certain laws of the Elasticity of Gases and Vapours, which have been confirmed by experiment, viz:—1. That the relation between the pressure, density, and absolute temperature of an elastic fluid is expressed by the following equation:—

$$\frac{\text{P V}}{\text{P}_0 \text{ V}_0} = \frac{t}{t_0} - \text{A}_0 - \frac{\text{A}_1}{t} - \frac{\text{A}_2}{t^2} -, \text{ \&c. } (32.)$$

where A_0, A_1, A_2, &c., are a series of functions of the density $\frac{1}{\text{V}}$, to be determined for each elastic fluid by experiment. For Air and Carbonic Acid Gas, it appears that each of these functions is sensibly proportional to the density simply, multiplied by a constant.—II. That the relation between the pressure and absolute temperature of ebullition of a fluid is expressed approximately by the following equation:—

$$\log. \text{P} = \text{A} - \frac{\text{B}}{t} - \frac{\text{C}}{t^2} -, \text{ \&c.} \ldots\ldots\ldots (33.)$$

A, B, C, &c., being specific constants, to be determined for each fluid by experiment. Some of these constants have been given in section 19 of this article.—(On the subject of the hypothesis of molecular vortices and its results, see the *Edinburgh Philosophical Journal*, 1849, *Edinburgh Transactions*, vol. xx., and *Philosophical Magazine*, *passim*, especially for December 1851, and November and December, 1855. The values of the constants in such formulæ as 32 and 33 have undergone revision from time to time according as more and more exact experimental data have been obtained. The latest and most exact values of the constants in the equation (32) for air and carbonic acid gas are investigated in an unpublished paper in the hands of the Royal Society of Edinburgh, for an abstract of which, see the Minutes of Proceedings of the Society, 1854-5.)

36. *Conclusion.* — *Science of Energetics.*—Although the mechanical hypothesis just described may be useful and interesting as a means of anticipating laws, and connecting the science of Thermo-dynamics with that of ordinary mechanics, still it is to be remembered that the

ence of Thermo-dynamics is by no means de-dent for its certainty on that or any other pothesis, having been now reduced to a system principles, or general facts, expressing strictly results of experiment as to the relations be-een heat and motive power. In this point of w, the laws of Thermo-dynamics may be re-ded as particular cases of more general laws, plicable to all such states of matter as consti-e *Energy*, or the capacity to perform work, ich more general laws form the basis of the ENCE OF ENERGETICS, a science compre-ding as special branches, the theories of tion, heat, light, electricity, and other phy-al phenomena.*

Heating of Buildings. The long winters his country, accompanied by so many changes, he excess of humidity at one time, and of cold cing winds—particularly from the east, in ing at another, have necessarily led, more ecially in recent years, to much consideration ng bestowed upon the means of warming both lic and private buildings so as to obtain an able temperature combined with salubrity. s unnecessary here to enter upon a description the numerous contrivances of open grates to rm rooms. Much skill has been bestowed in ir construction—and they are made to suit palace or the cottage—even the mode of iting the fire has been changed, and the law ersed by making the fuel burn downwards ead of upwards. The use of open fires is so nded with the habits and usages of the people, t it would indeed be a difficult task to change lic opinion as regards them; still this mode heating has its disadvantages and incon-iences, such as smoky chimneys, back smoke, ste of fuel, injuring furniture from dust, and unequal warming of the apartment; one t of the room being cold, forcing the inmates draw near the fire—by which they are over-ated on the one side, and chilled on the other, ile the feet and legs are exposed to a current of d air at the floor.—Various contrivances have n proposed as a substitute for open fire grates obtain uniformity of temperature, and several these with considerable advantage. When ging of such inventions, it is of paramount portance to keep the following rules in re-mbrance:—I. That by the mode of heating, temperature is not raised at the expense of oxygen, which is allotted for sustaining the lthy respiratory functions, leaving a residuum carbonic acid and other gases.—II. That the de of heating does not injure the atmospheric in the process of warming it, by the air being de to pass over metallic surfaces at or near ed heat, and that the carbonaceous matter of fuel is not mixed with the heating current.— attention to these two points the value of ious artificial means of heating may be correctly estimated; and no method, however economical in fuel, should on any account be employed which has the least tendency to deteriorate the air to be heated.—At the present time, when artificial heat is applied to buildings in this country, it is chiefly made use of in cases where open fires cannot be conveniently adopted. This has been accounted for from the deep rooted habits of the people in favour of room fires, although it is well known that on the continent of Europe no such partiality exists. Taking the custom, however, as now existing in Britain, artificial contrivances for heating are chiefly used for warming public buildings, factories, prisons, warehouses, conservatories, &c.; and when applied to houses it is usually auxiliary to open fires.—Judging of the merits of the numerous inventions for heating by the preceding standards, No. I. would include the various modes of heating by gas, which cannot be burned without deteriorating the atmospheric air; while it is a fact that no more heat can in reality be obtained from the burning of gas in a close vessel as a stove, than if the same quantity were consumed in the lighting of a room.—This mode of heating also applies to the various kinds of stoves which hold out that heat can be obtained from burning charcoal or other fuel without injury to the air of the apartment—or to stoves or grates without flues. For what would be thought of an open fire without a chimney? It should always be kept in remembrance, that combustion cannot go on without air; and unless the products from it are removed as they are generated, they become fatal to human life. The many evil results arising from breathing for any length of time corrupted air is well known. It has been supposed, that carbonic acid gas in apartments is so diluted that it cannot be so accumulated as to produce immediate serious effects. Still it was but lately that several of the congregation of a country church in the north of England were nearly suffocated by a stove; and it is almost a matter of common occurrence to read of the death of people from the fumes of charcoal in close places. What are the fumes of charcoal producing asphyxia but carbonic acid gas? But the products of combustion from a coal fire, smoke, sulphur, and carbonic acid, &c., are equally fatal to life; and the same poison, which destroys life over the embers of a fire in a ship's cabin, causes the accident from choke damp in the mine, the well, or the vat.—As regards the second mode of heating, it applies to the numerous class of cockle stoves of cast iron, or close hot air stoves, which are used to warm churches and other buildings. These stoves are not now so much in use as they once were, but they are liable to the objection of overheating the air, depriving it of its moisture and purity, passing it over metal often at a red heat, say 700° F., by which the air itself is decomposed, the oxygen is rapidly consumed, forming an oxide on the surface of the metal,

* *Edinburgh Philosophical Journal*, July, 1855; *Proceedings of the Glasgow Philosophical Society*, 1853-5.

and the vapours are converted into hydrogen, or carburetted hydrogen gas. The rarefied air, which enters the apartment from such stoves, must be chiefly composed of nitrogen—the effluvia arising from the hot air—often mixed with the products of the fuel—carbonic acid gas within the cockle—entering with the heated current; this sulphureous vapour being most injurious to health.—From the risk of the heating vessel being burnt out, or giving way, and the fumes from the fuel escaping into the warm air flues, very considerable danger from these stoves exists as to fire. Cast iron cockles of this description were, till about 1853, in use at the College Museum and Library, Edinburgh, from which much injury arose to the collection and books. They have since been removed, and these buildings have been heated with a patent hot water heating apparatus, which will be afterwards noticed.—Several kinds of heated air stoves, made of malleable iron, for warming the air have also been used. By these it is proposed to effect the object by warming a larger volume of atmospheric air to a lower temperature. It should, however, be kept in view, that whenever the iron reaches a black heat beyond 300° F. a partial decomposition of the air takes place by the burning or scorching of the dust mixed with the atmospheric air which abounds more in towns than in the country, (visible to the eye in the sunbeam), and which is chiefly composed of animal and vegetable matter. Another defect stoves of all kinds are liable to, arises from the fact that the fire is burning within the interior of the metal or brick work, and in the course of time, from constant expansion and contraction, the fumes of the fuel are liable to escape through the crevices of the stove into the hot air flues, and thence are discharged with the hot air into the apartment, thus producing effluvia or injuring the purity of the entering current.—These defects, appertaining to stoves, being long known, numerous schemes have been proposed for their removal. The attention of mill-owners, where stoves were early and chiefly used, was directed to find some better plan than the cast iron cockle, originating from the German stove; accordingly, one of the earliest improvements was what was called the Belper stove, the idea originating from the late William Strutt, Derby, in 1792. By this plan the old method of introducing small streams of highly heated air was got rid of, and the heat of the entering current reduced, in order to obtain a larger volume of slightly warmed air. It does not appear that the plan of Strutt was much adopted by mill-owners. The late Mr. Robertson Buchanan, C.E., states that he did much to introduce the Belper stove into Scotland. It was not till 1819 that it became generally known by the publication of *Sylvester's Domestic Economy*. A year or two after this, the late Sir John Robison, Secretary of the Royal Society of Edinburgh, who happened to be at Derby, was so struck with the advantages of the Belper stove over those cast iron cockle stoves then generally used, that he had one on the principle of the former applied to heat his house in Edinburgh. The attention of Mr. R. Ritchie, engineer, of the latter city, being turned to the subject, he effected various improvements to simplify the Belper stove. These he brought under the notice of the Royal S. Society of Arts in 1832, which awarded to him a premium. Stoves of this kind have been extensively erected by him, which have given as much satisfaction as stoves can give.—Notwithstanding the improvements made upon stoves, these did not effect the object required by mill-owners. From the rapid increase of factories of large extent, and with several floors, an equality of heat in different levels was required, which could not be obtained by the heated air from stoves from the rise of the warm air to the upper level. Steam heat had been proposed many years before, but it made its way at first very slowly. It seems to have been first brought under public notice by a Colonel Cook in 1745, in the *Philosophical Transactions*, London. It was introduced into England by James Watt in 1785, and into Scotland by Mr. Neil Snodgrass in 1789. The first mill heated by steam in England was by Boulton and Watt, 1799; and the same year, in Scotland, Mr. Snodgrass introduced it into a cotton mill. Mr. R. Buchanan, C.E., was one of the first writers—1807-10—in favour of warming mills by steam. The late Mr. Tredgold—1824—and others have likewise written in favour of it. This plan of warming mills may be said to have originated about the commencement of the present century, but since that period the practice has gone on increasing to the present time, and now we may remark, hardly any other method is used, so infectious is example. But although the plan of heating by steam is so general at factories, it has not been extended—though it has often been tried—to domestic buildings; and Mr. R. Ritchie of Edinburgh informs us that he applied it in country mansions more than twenty years ago. The mode at present, in general use is to place the steam pipes within work-rooms and to elevate the temperature of the room by means of the surface of the pipe. The cleanliness of the plan has its advantages, and from the general use of the steam engine at factories, it has no doubt led to steam being almost the only mode now adopted for warming them, as the steam can be led through the pipes over the different floors of a large building. The steam pipe, in order to keep clear of the machinery, is placed over-head of the workers. When this system of heating is combined with proper ventilation it answers the purpose very well. It is, however, capable of improvement; and in 184[illegible] Mr. R. Ritchie, C.E., Edinburgh, submitted a communication to the Royal Society of Arts on the *Warming and Ventilation and Sanitary*

ngements of Factories, for which a premium awarded. His paper was subsequently shed, and it is deserving of the attention of rs of factories, or others employing a number ork people confined in one apartment.—Be- the arrangement of the steam pipes as be- noticed in a few instances, as at one very work-room of one floor, at a flax mill in , steam power is made use of for inject- he air into the apartment by means of a rful fan, the air being previously warmed, ssing it through tubes in cases heated by .—The drawback against the use of steam, rming domestic buildings, arises from the ilty of management, and that it almost re- the presence of an engineer to work it with ; besides which, as it is not required mestic buildings, as in factories, it is only ry peculiar cases that steam is made use of e former class. Hot water pipes were found of easier management than steam, and they have gradually come to be made use of medium of heating when artificial heat is re- d for public or private buildings. Hot water rst used in this country to warm green-houses. as, however, previously used in France. described by M. Bonnemain in the *Diction- Technologique*, upwards of fifty years ago. onnemain also used hot water for heating ouses in France *serres chaudes*—hot water first applied in Britain by the Marquis de annes. It is now a good many years since ings in London were first heated by this with large pipes; both large and small pipes owever, now in use; Mr. R. Ritchie, C.E., burgh, first applied hot water to heat a stic dwelling in 1826.—In the year 1835, ratory to rebuilding the House of Com- , which had been destroyed by fire, (sup- to have originated from stove flues in the e of Lords)—a select committee was ap- ed to take evidence as to the best mode of ing and ventilating the new house. The stions of Dr. D. B. Reid were entertained, his plan of an experimental house was ed. From this report, a question of some rt was mooted, as regards heating, viz., er the air should be previously warmed, d, moistened, and regulated, before it en- the house? This plan, following up the of Mr. Strutt, it will at once be observed, variance with the plan generally adopted lls, where the heat is given to the air by tion. A question has consequently origin- in which there is much difference of opinion, ly, whether it is better to warm the air is to raise the temperature of an apartment given point before admission, proposing to air and heat combined,—or whether it is r to have the warming and ventilating pro- separated, and to raise the temperature of ir of the apartment with heat radiating from derately heated surface—suppose hot water pipes, and to keep the ventilating arrangements separated—or in other words, to admit fresh air as it exists without being previously heated, and thus deprived of its moisture and natural pro- perties. A great deal may be said on both these heads, but it has been urged with strong reasons that the air itself is less altered by the second plan than by the first. One writer, several years ago, has given his opinion very clearly upon this subject. He states, "It has been proposed by some to heat the external air and throw it into the apartments, in a manner similar to some kinds of stoves, supposing that thus by *combining ventilation with heating*, it would be more salu- brious. But this would be obtaining heat at a great expense of fuel, and be losing one very important advantage which attends heating by pipes, viz., that the heating may be kept per- fectly separate from the ventilating process, so that each may be managed separately, and no more heat nor ventilation given than what is just and proper. Whereas, on the other plan, there must always be nearly the same ventilation whe- ther the external air be dry or damp, warm or cold—when damp, a great quantity of moisture must be thrown into the building, which will require additional heat to suspend it, in order to prevent it from being injurious; and when the air is dry, the current of air will carry in dust along with it. I would, therefore, recommend pro- ducing the current from the *rarefaction of the air within the building*, and ventilating by the window or some other proper openings for the purpose." —Both of these methods are now in general use, for warming buildings with hot water. By the first plan cast iron pipes, of three or four or more inches internal diameter, are made use of. These pipes, as in the House of Commons, St. George's Hall, Liverpool, and elsewhere, are placed in a chamber, and the external air is made to pass over them, which, receiving heat from the pipes, enters the building at an increased temperature; another plan adopted is, to place these pipes in flues or trenches, or under passages, as may be seen in churches, and the external air is made to pass over these pipes, and thus receiving heat from them, enters the building warmed, as in the other case. The water is heated in a boiler, from which it flows and returns; the pipes are usually connected with what is technically termed spig and faucet joints, but flange joints are some- times used, although the latter are more generally used for steam joints. As this plan of heating is so well known, especially in conservatories, it is unnecessary to enter into further particulars regarding it. — Various buildings have been heated by Mr. R. Ritchie, C.E., Edinburgh, some of which may be noticed, combining both of the preceding arrangements, viz., the Commercial Bank and Police Buildings, Edinburgh; the Justiciary Court and St. Matthew's Free Church, Glasgow. In these buildings, Perkins' ingenious patent, of pipes four inches bore with screw joints,

is used. By this plan no boiler is required and a great saving of fuel is effected. Sometimes the large hot water pipes are exposed in apartments to heat them, as is done with steam pipes in factories; but this is rarely done and is very unseemly. The defects arising from the mode of heating with air, either heated by hot water or steam—even supposing the plan in principle more scientifically correct than by exposing the heated surface—have already been noticed. Practically, the plan of heating by air flues has been found to be attended with many inconveniences, such as reverse currents—the difficulty of getting fresh air from a pure source, as it is often obliged to be taken from sunk areas—smells entering through the flues into the building — while the heated air flowing into the church or apartment, through gratings trod upon (being generally placed in the floor), brings in with it dust or smoke. Another point connected with this mode of heating, is the waste of fuel which arises from the pipes being sunk in trenches or chambers—as no heat can be given out until the whole water in the pipes is heated. The application of hot water to heat domestic buildings would have made but little progress, however useful the large pipe system in some respects is—more especially for greenhouses—had it not been for the very ingenious inventions of Mr. A. M. Perkins of London, who, several years ago, brought out his patent apparatus for heating buildings of every description with hot water by means of pipes only one inch *external* diameter. These pipes are made of the finest and most ductile kind of malleable iron; they admit of being bent cold into any form required, and thus can be easily combined with architectural arrangements.—Mr. C. J. Richardson, Fellow of the Royal Institute of British Architects, London, who has written an excellent treatise on warming and ventilating buildings, and who has expressed himself very strongly in favour of Mr. Perkins' ingenious contrivance, remarks that "the pernicious system of warm air stoves, and the costly and therefore unattainable one of steam, have been superseded in a great measure by the more simple and less expensive method of a circulation of hot water through iron tubes."—It is somewhat singular that Seneca, the Roman writer, describes very accurately a contrivance, a tube called Draco (a serpent) for heating the warm water for baths—the Piscena of the Thermæ—which bears a great resemblance to the useful invention of Mr. Perkins, inasmuch as by both plans a boiler is dispensed with. It may, however, be necessary to state, that very important improvements have been made in the latter since it was first brought forward. In London, this mode of heating has been largely adopted at a variety of buildings, and applied to different purposes where heat is required. The great extent of pipe (above 60,000 feet) and the number of furnaces (above 70) erected at the British Museum, and its durability and continued use there, are the strongest proofs of its manifest and numerous advantages. Such is the improvement on this apparatus, that about a mile of pipe of this small diameter can now be heated by one fire, which might almost appear incredible if examples could not be shown. This plan of heating with hot water admits of almost endless arrangements,—it can either be made use of to warm the external air previous to its admission, as already noticed, or to give off its heat by radiation within the apartment, or fresh air may be made to pass through a coil of pipe placed in a room, as recommended by the late Dr. Combe. Mr. Perkins has recently made still further useful improvements, under a recent patent, in his invention, 1856, —by which the older systems of heated water in large pipes is combined, or connected with the small,—these improvements have been lately noticed in the last edition of Mr. Richardson's work. As respects the small pipe system, he remarks, "that the superiority of Mr. Perkins' apparatus consists in this,—that the pipes, which form a continuous or endless tube, are reduced in size, and a very small quantity of water is required, (compared with the common system), which, circulating with great rapidity, effectually takes up the heat from the furnace, and transmits it to any height in a building, or in any direction to a considerable distance; the furnace being placed in any convenient spot, either within or without the structure. The apparatus thus combines before all other the great requisites of compactness, utility, and frugality, and possesses the power of adaptation to all situations, interfering in no respect with architectural arrangements. In the hands of the architect, it is capable of being adapted to any building in consequence of the small space it requires, and from the rapid circulation it is capable of warming even the largest building very efficiently. Fig. 1 will explain the principle—but the arrangement of such an apparatus must be endless varied to suit objects and circumstances. M Richardson also mentions that such is the safe of this mode of heating, that Mr. Beaumo Managing Director of the County Fire Offi

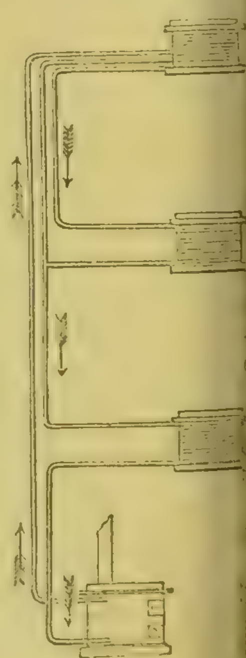

Fig. 1.

'don, states to Mr. Perkins that the Directors hat office are so satisfied with its absence of langer, that they readily accept insurance on dings at the lowest rate of premium.—The :uring a circulation of water through such ll tubes is obtained by the extreme expansi- y of water. The relative specific gravities only to be considered, which two columns water must bear to each other—one column ing been rendered lighter by the application eat, which expands it.—The air being pumped of the tubes, they are then filled with water. apparatus being now ready for use, the fire ne furnace is lighted, the effect of which is, use the particles of the water to expand, and ming lighter, they rise rapidly in the flow scending pipe, and parting with their caloric radiation as they pass onwards, they come : to the furnace by the return pipe. By ns of the heated particles ascending and their e being supplied by colder ones,—those that : already parted with their caloric in their ard progress through the pipes,—a constant ilation is maintained. Provision is made for expansion of the water, which is regulated n uniform law. The movement of the water igh the pipes has been compared with the lation of the blood in the human frame, water in the tubes having a somewhat simi- novement with the blood in the arteries and s.—The small pipe system of heating has no- e been more successful in its various applica- , than when applied by Mr. R. Ritchie, C.E., burgh, who is also Mr. Perkins' agent. R. has effected various improvements on it, to increase the utility and facilities of the ngement of the apparatus. He has proved its : capabilities as an agent for diffusing heat economizing fuel. At Stewart's Hospital, burgh, he has made one fire heat about 0 feet of pipe,—a large coil, amongst others, ing heated at 150 feet from the fire. At the ster House, he has heated a large extent ipe,—as also at the Signet Library, where utility and economy of this mode of heat- s fully shown. At the College Museum, an nsive range of pipes has been heated by him veral coils from one fire, which is made to both the upper and under Museum. Fig. 2

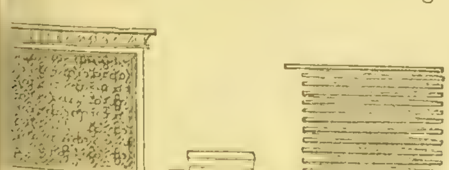

Fig. 2.

s the method by which the heat is there ed, and how the coil of pipes is concealed an ornamental case with a marble or other The pipes are also carried round rooms, as shown. A still larger application of this principle has been carried out by Mr. R. Ritchie at the College Library, where, as previously mentioned, more than one mile of pipe is heated by two furnaces. This mode of heating has given great satisfaction, as has been acknowledged not only from its increased salubrity and comfort, but by the preservation of books, keeping them free from damp, smoke, and dust. A still further application of this principle—of heating a great extent of pipe from an ordinary sized furnace, (which in ten hours will consume little more than one cwt. of fuel), has been carried out by Mr. R. Ritchie at the Lunatic Asylum, Aberdeen, where he has heated two wings of a building of 150 feet each, two storeys high, with about 4,000 feet of pipes from one fire. Other two wings at the same building are heated in a similar manner—and this method of heating has been carried by him successfully into operation, to a great extent, at this Asylum and other buildings at Aberdeen. The economy of fuel by this mode of heating must be at once apparent. The whole of the academy at Bathgate, heated in this manner, from one fire, hardly exceeds for fuel fivepence per day. It may indeed excite surprise, that these things are accomplished. Mr. R. Ritchie is also of opinion that he could heat a much greater extent than has yet been done by this invention—that almost any building could be heated by *one fire*, by which a great saving of expense for fuel and trouble in the management would be effected.—Mr. Richardson remarks of this system of heating that, "the most ignorant domestic is capable of conducting it, and the small expense of fuel used, in proportion to the effect produced, combine so many recommendations of this system, that, sooner or later, it will be universally adopted, and the more intelligent portion of the community will countenance it at a very early period."—*Ventilation.*—It is at all times important to consider that, whatever plan of heating is adopted, due regard should be paid to the ventilation as well as the hygrometrical condition of the air, and imparting moisture to it when necessary. This subject will be fully treated of under the head of VENTILATION.

Hebe. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Heliacal. See ACHRONICAL.

Heliocentric. The position of a celestial body as seen from the SUN, instead of from the EARTH, is its *Heliocentric* position, in opposition to its *Geocentric.*

Heliometer. A name given originally to a small telescope used in measuring the diameter of the Sun, but which suggested a principle that has recently been applied in the construction of several of the largest and most important instruments now belonging to Practical Astronomy. The distinguishing characteristic of the old Heliometer was this—either the small object-glass was cut into two parts through a diameter, these

two parts being relatively moveable; or, in front of the simple object-glass, a Dollard's double image micrometer could be placed. See MICROMETER. So soon as art had reached the power to execute its requirements, it became evident that the same contrivance might be taken advantage of, in the case of object-glasses of any size, and, therefore, of telescopes of the highest efficiency; and it appeared as well, that the Heliometer, if capable of measuring the diameter of the Sun, might equally measure the distances and relative positions of any two stars. The achievement demanded from art, was first accomplished by Frauenhofer, who constructed the great instrument that produced results so signal in the hands of Bessel; and his rather daring example has been most successfully followed since by Merz and Repsold. That our description of this most valuable instrument may be sufficiently palpable, we present the reader with a view of the Heliometer at Oxford, constructed by the Mechanician of Hamburg, and at present under the management of Professor Manuel Johnson.

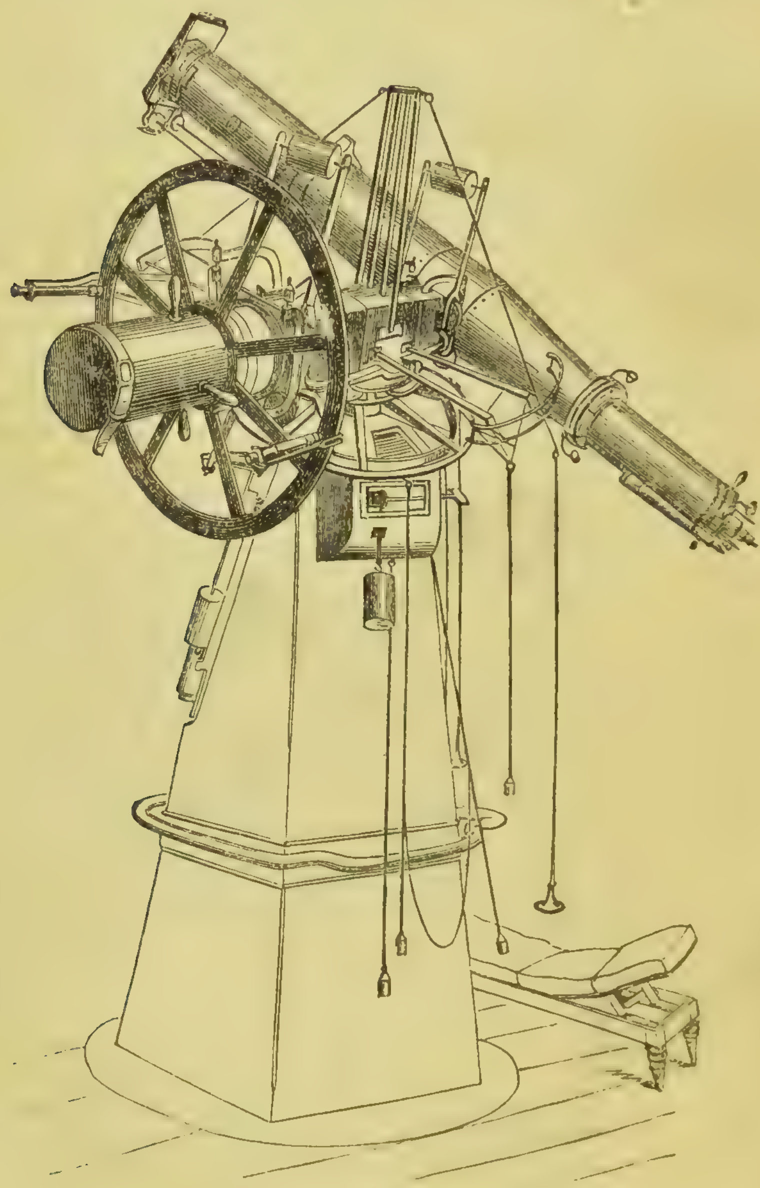

Fig. 1.

The telescope, it will be seen, is mounted as an Equatorial—the hour circle being at the top of the pillar, and the declination circle at one end of the axis—both quite within reach of the observer, whose place of course is near the eyepiece. It is evident that every adjustment requisite for the Equatorial, is requisite in the case of the Heliometer, and that no correction nece[illegible] sary for the former can be dispensed with in t[illegible] use of the latter instrument. These corrections, [illegible] we shall soon see, are considerably more comple[illegible] nor is it to be concealed, that the labour of usi[illegible] one Heliometer aright, arising from such causes [illegible] the great, if not the only practical objection [illegible]

t Instrument. The peculiarity of the Helio-
.er will be easily understood by aid of the sub-
ıed cut, which represents its divided object-
ss. In the Oxford Instrument, the object-glass

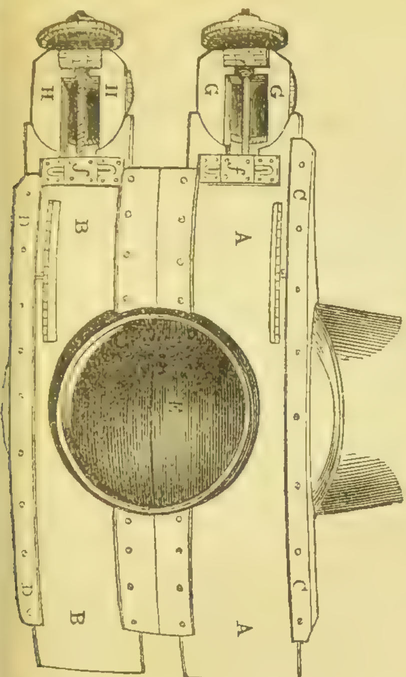

Fig. 2.

·5 inches in diameter, and has a focal length 0·5 feet. That glass, as the figure shows, is cut) two semicircles, E and F, by a section along iameter; and these two pieces can be made to e along their common section, or to alter their ition in regard to each other, by the screws 3 and H H,—screws that have micrometer or ly graduated heads, which, in connection with scales at C and D, indicate how far the centres he semicircles are at any time apart. When the semi-object-glasses are in the position shown the cut, they are virtually one object-glass, l, therefore, form one image of a star or other ernal magnitude, in the focus; but when the tres of the two pieces are apart, each semi-ect-glass has a focus of its own, and two ıges are consequently formed, whose apparent ances from each other, correspond with the ances of these centres. Conversely, when centres coincide, two stars will form two ıges in the focus, whose distance and relative ection correspond with the angular distance l relative direction of the actual bodies; and is easy to see, that by screwing the centres ay from each other, these images may be ught nearer and nearer, until they lie in the ıe vertical line. If the whole frame con-ıing the object-glass could be turned suffi-ıtly round, the two images might be made to cur as to direction also, in other words, to ıcide and form *one image* in the focus of the Heliometer. This coincidence established,—an operation easily effected by means of the handles of the Hooke's joints represented in fig. 1—the Observer has simply to examine the state of his Instrument; and this will indicate the angular distance and relative position of the two stars. The amount by which the whole object-piece has been turned round, is indicated on the circle surrounding the tube of the telescope a little way above the eye-piece. And the distance of the centres—corresponding to the angular distance of the stars, can be read off by the observer at the eye-piece also, through the two subsidiary tubes in the cut, by aid of an arrangement within the tube for illuminating the scales by the Electric Light. Nothing can exceed the ingenuity displayed by M. Repsold in the construction of this superb instrument. Only to one other peculiarity, but that a very important one, can we allude here. In all previous structures of this kind, the plate A A, B B, on whose surface the semi-object was *plane* or *flat;* Repsold felt that the foci of the two segments could not, in such an arrangement, fall at the same distance from the observer's eye, when the centres were apart; and he applied an effectual remedy by *curving* these plates—their radius of curvature being the focal length of the object-glass. It need not be intimated to the experienced observer, that the care requisite for the true use of the Heliometer must be of the highest order, and that the corrections needed to reduce its rough results are numerous. The more complex any instrument, the more do these corrections multiply; and, in the case before us, we have, in addition to the errors of the common equatorial, to verify several micrometer screws, and to determine the exact representations in angular distances, of the divisions of several scales. Further, there is the great question of flexure—not the mere flexure of the telescope in length, but which, in this case, is of much more consequence, the flexure of the declination-axis; this latter flexure must be considerable, on account of the great weight of the Telescope, and it is of the greatest importance to detect it accurately, because it directly affects the precision with which the Heliometer indicates the relative *position* of two stars. For a curious and acute elimination of this source of error by Professor Johnson, we refer to the *Radcliffe Observations* for 1851, in which volume, and the previous one, the structure and use of the Oxford Heliometer are most ably discussed at large. The student should also peruse the famous Memoir by Bessel, in the *Konigsberg Observations*; or the dissertation by Brunow in his *Späzische Astronomie.*—The Heliometer is especially applicable to investigations of the multiple stars; although its merits, compared with those of the position Micrometer, are not yet fully determined. Professor Johnson has recently shown in an elaborate Memoir its remarkable adaptability to *Photometrical* determinations. See *Radcliffe Observations.*

Heliostat. An instrument indispensable to every one engaged in higher optical research. Its object is to make a solar beam stationary, or apparently stationary; in other words, to preserve its direction invariable, notwithstanding the motion of the Sun in the heavens. It is evident that the possession of such an instrument must be of the last importance, so that an analysis of the solar beam be conducted without embarrassment. The construction of Heliostats is various; but the principle is the same in all, viz., that of the *Equatorial* Telescope. A reflecting surface attached to a polar axis, moved by clock-work at the rate of the sun's diurnal velocity: the ray obtained from this surface is necessarily constant in direction; and this ray is again turned by a second and fixed mirror, as the observer desires.

Heliotrope. A valuable instrument much used abroad in Geodetic surveys. Its objects are to enable the surveyer to transmit signals of reflected light from one station to another, and readily to discern those transmitted. Its most convenient form has been given it by M. Merz of Munich.

Hemisphere. Half of a sphere. The Earth is divided into two hemispheres, the Northern and Southern. The line of division is a great circle, the *Equator*, perpendicular to the line of the poles.—It is a remarkable fact that the Earth can be divided into two hemispheres, one of which shall contain almost all the *land*, and another, almost all the *water* on its surface. The harbour of Falmouth is very nearly in the centre of the terrene hemisphere. This goes, so far, to explain the pre-eminence attained by London and the English ports generally, in mercantile affairs. Sir John Herschel infers that the protruding half must be lighter than the other, and that, therefore, the globe cannot have equally dense hemispheres.

Hercules. An old constellation which Aratus, Hyginus, and Ptolemy, describe as ἐνγονασιν, and which the poet Aratus says is "a figure like that of a man in sorrow." It lies between Lyra, Ophiucus, Draco, and Bootes. It has no stars above the third magnitude. An extremely rich and brilliant cluster of stars, or *Nebula*, is in this constellation.

Hermetical. A word taken from the language of the old alchemists, who regarded the Egyptian Hermes, or Thaat, as the inventor of all arts, and especially of chemistry. Hermetically, means *exactly*, and is chiefly applied to the closing up of vessels which contain substances capable of dissipation by evaporation or otherwise. They are said to be hermetically sealed, when no air can enter the vessel. A glass vessel is hermetically sealed, when, by means of the blowpipe, its mouth is completely closed.

Herschel. See Uranus.

Heteroscii. (People with different shadows.) An old term applied to those whose shadows are always in different directions. Those dwelling in the torrid zone have their shadows changing in direction all round the compass. Hence, no heteroscii to each other can be found there. People in the northern hemisphere beyond the tropics, have their shadow always turned to the north, and those in the southern, beyond the tropic, always to the south. Hence, in a general way, these parts of each hemisphere may be considered "heteroscii" to each other.

Horizon. When we stand at any height above the ground, if it be level, we see a considerable portion of its surface. If we stand higher (see Dip), we see more of it, but in all cases we are stopped somewhere, by the sphericity of the Earth's mass. We cannot see round it beyond a certain point; and the rim which marks the outmost limit of the Earth visible to us,—where our line of vision no longer can strike its surface, but only stretch away into space—is called the horizon. We can understand readily that, as the Earth is regular in its spherical form, this line should, except where local irregularities break it, be circular. From our ordinary elevation, there does not appear any sphericity in the Earth, and the *horizon*, the *sensible horizon*, appears, therefore, a plane with a circular rim. It is just the plane tangent to the surface at the point where we touch it, or very nearly so. The term is used indifferently for the plane, and for its circular edge. Considering the enormous distance of the heavenly bodies, it would make very little difference in respect to them whether we suppose that plane passing through the centre of the Earth, or over its surface. The two planes are parallel, and at the constant distance of nearly 4,000 miles (a semi-diameter); but at the remoteness of the stars, 4,000 miles is imperceptible. For the moon and the nearer planets there is greater difference, but the *rational* or *celestial* horizon, as it is called, is still used in observations; and where needful, observations are reduced to it, by the geocentric and parallax corrections being made.—When—through mists, or clouds, or darkness, or whatever other cause—it is impossible to see the horizon, so that we cannot take the altitude of any object, an ingenious optical contrivance (*artificial horizon*) employed, is this:—A vessel of mercury is set down, floating horizontally (*a horizontal mirror*); and the object to be observed is looked at through a telescope with a graduated circle attached. Its *image* is next looked at with the same telescope, the circle remaining, meanwhile, fixed, and the telescope moving in a plane. The reading of the angular space through which it has moved from the position where it viewed the star, to that where its reflection was seen, is then carefully noted; and, according to the easily understood theory of mirrors (Catoptrics), the half of this gives the accurate altitude of the star above the horizon.

Horology. An explanation of the principles of the measurement of time. See Calendar, Dialling, Clepsydra, &c.

Horopter. The surface of *single vision* corresponding to any given binocular *parallax*, is called the *Horopter.* For a full explanation see PARALLAX BINOCULAR.

Horse Power. A power capable of raising 33,000 lbs. through one foot per minute. When an engine is said to be of so many horse power, it is meant that it could lift so many times 33,000 lbs. through a foot in a minute.

Hour Angle. The angle between the hour circle suited to any given body, and the meridian of the place of observation.

Hour Circle. A circle in the heavens passing through the position of any body, and the poles of the heavens.

Hyades. (The rainy.) A constellation forming the forehead and eye of Taurus. It has five stars visible to the naked eye. Some of the ancients saw seven. They are arranged in the form of the letter V, α and ϵ Tauri being at the extremities; γ at the angular point; θ Tauri between α and γ Tauri; and δ between ϵ and γ. The star α Tauri is Aldebaran.

Hydra. An old constellation, sometimes erroneously called also Hydrus. It is represented as a long snake trailing on the ground, carrying on his back a Goblet (Crater), and near his tail a Crow (Corvus). It is usually, from its great length, divided into four parts: Hydra, the head, all near to, and south of Regulus; Hydra et Crater; Hydra et Corvus; and Hydræ continuatio, containing the tail. The star α Hydræ is of the second magnitude.

Hydraulic Press; or the **Bramah Press,** from its inventor's name. The suggestion of this instrument is claimed for Pascal, but however that may be, it was not actually constructed before Mr. Bramah took out his patent in 1796. It rests upon the well-known hydrostatical principle, that when a mass of fluid is in equilibrium, pressure applied at any part of it is transmitted over its whole mass, and each particle sustains the same pressure as every other particle. It is used for producing powerful compression over a large surface, for raising weights, for drawing up trees from the roots, or raising the piles of worn out bridges from river beds: and, in fact, everywhere that we wish to apply forces readily to a work where we cannot bring them directly to bear, or desire to apply them over a large surface, when we cannot readily apply them immediately, the Bramah press may be made use of.—In its simplest form, it consists of a large piston moving in a water-tight cylinder, the bottom of which is filled with water, which also communicates by tubes filled with water, with the bottom of the cylinder of a small forcing pump. The water in this last is submitted to any pressure we may wish to apply; and that being transmitted to the large piston, lifts it up, moving along with it whatever may be connected with it.—The Bramah press is, like all other machines, no creator of effect, but a mere distributor of it. Indeed, the whole work it does, is first given to it; although, as the actual exertion at the pump does not require to rise higher than the pressure, it does not appear to have been so. The apparent gain of power is just in proportion to the difference of areas of the sections of the pump and of the cylinder. If the one, for example, have a radius of only one-fourth of an inch, and the other one of ten inches, —the area, being in the proportion of the squares of the linear dimensions, will be as 1 to 1,600, and the apparent gain will be exactly proportional to this difference.—The hydraulic press has been used, of late years, very efficiently in raising those enormous masses of iron, contained in tubular bridges, to the height at which they must be finally placed above the level of the water. It has done this, with the greatest ease,—raising them *en masse.* Its almost unlimited capacities promise to be yet more fully recognized.

Hydraulics include the study of the phenomena of fluids in motion, and of the methods of obtaining from these, useful results. We shall speak here only of incompressible fluids like water, which are called liquids. The subject is divisible into four parts; 1st, the laws of the effluence of water contained in a reservoir; 2d, of running waters; 3d, of the use of water as a motive power; 4th, of machines for raising water.

(1.) *Of Effluence or Discharge.—First.* Let the reservoir be constantly full of water. The opening by which the water flows out, is sometimes completely covered by the fluid, and is then called an orifice; sometimes it is not limited above, and is called a *weir*, or *sluice.* If the orifice be in a wall whose thickness is less than half the smallest dimension of the orifice, it is said to be in thin wall; in other cases, it is supplied with a spout, sometimes cylindrical, but generally conical and convergent. The vertical distance of the surface of the fluid, from the centre of gravity of the orifice, is the *charge of water* on the orifice, to which the efflux is due. Let H be the charge of water; V, the mean velocity of efflux; and $g = 32{\cdot}2$, the velocity acquired by falling bodies in a second. In the case of an orifice with thin wall, $V = \sqrt{2\,g\,H}$; that is, the velocities are proportional to the square roots of the charges. Let Q = waste of water per second, m a constant co-efficient, S the surface of the orifice; then $Q = m\,V\,S = m\,S\,\sqrt{2\,g\,H}$, m being equal to ·62 in orifices with thin walls, which gives $Q = 4{\cdot}98\,\sqrt{H}$.—In orifices which have a cylindrical spout, equal in length to three or four times the smallest dimension of eth orifice at least, and where the efflux is from the whole mouth, and not merely from its lower part $V = {\cdot}82\,\sqrt{2\,g\,H}$, and the waste of water $Q = {\cdot}82 \times S\,\sqrt{2\,g\,H} = 6{\cdot}58\,\sqrt{2\,H}$. The conical, convergent spouts, made use of in public works, whose inclination is about 10° or 12°,

diminish but very slightly the total waste of water and the effective velocity, through the fluid friction. They are about ·98 of the theoretical waste and velocity. Conical divergent spouts (which are, however, little used) are capable of doubling the waste of water due to orifices in thin walls. When the charge on the centre of the orifice is very feeble in proportion to the height of it, the mean velocity of efflux is a little less than that given by the formulæ above.—If, at the upper part of one of the walls of a reservoir, a rectangular opening is made, whose base is horizontal, the water of the reservoir (which, we suppose to be always kept quite full) will fall in a sheet over this base or ground. Let H be the charge of water on the base l, the width of the opening, L the width of a reservoir, Q the waste of water.—For l smaller than $\frac{\mathrm{L}}{3}$, we have $\mathrm{Q} = (3{\cdot}04)\ 1{\cdot}77\ l\ \mathrm{H}\ \sqrt{\mathrm{H}}$. For $l = \mathrm{L}$ as in an ordinary weir or dam, $\mathrm{Q} = (3{\cdot}39)\ 1{\cdot}96\ \mathrm{L}\ \mathrm{H}\ \sqrt{\mathrm{H}}$, and for intermediate values of l, the co-efficient of Q will vary from 1·77 to 1·96.—Let us now consider the case of a reservoir which is emptying itself—not keeping at constant level. The problems connected with this case are readily solved by help of the preceding data, and the following theorem;—the volume of water which flows out from any orifice of a prismatic vessel, which empties itself until it is completely dry, is only half what it would have been, during the time that the vessel had taken to empty itself had the level been kept constant. If, then, this charge and the horizontal section of the supposed prismatic reservoir be known, we may readily determine the time that it will take to empty, by translating the foregoing theorem into an equation. *Similarly*, the time that the level takes to sink any given amount, may be found by taking the difference of the times, which it would take to empty from the primitive level, and from the lower level. By transforming the equation which will give the time that the level takes to sink a given quantity, we may draw from this the expression of the volume of water which flows out in a given time. *Finally*, it remains to consider the efflux, when the fluid passes from one reservoir into another, arranged so that the orifice of communication be completely covered. When the levels are constant in each of the two reservoirs—which happens, for instance, when a canal supplies water to that immediately below it, by means of an opening placed below the level of the latter—the formulæ given above for the waste of water in free air may be used, taking H, as the difference of level between the two reservoirs. When the level is constant in the upper, and variable in the lower level, or *vice versa*—which is the case in canal-locks—we may determine the time necessary to empty or fill the well, by calculations, indicated above, for the case of a reservoir which empties itself in free air; replacing in these formulæ the horizontal section of the reservoir by that of the lock,—the charge of water being the difference between the original level of the water in the well, and its level in the upper or lower canal.

(2.) *Of Running Waters.*—Of *canals.* Canals differ from rivers in having a regular bed with everywhere the same slope, and the same limitations. The mean velocity of the water in them is very nearly 8-10ths of that at the surface. Let p be the inclination of the liquid surface, which is determined by levelling. Let c be the perimeter of the section $= l + 2\,h$ for a rectangular canal, and $l + 2\,h\ \sqrt{t^2 + 1}$, where (t is the slope given to the banks, determined by the nature of the soil), for one not rectangular. Let s be the area of this section, which is equal to $l\,h$ in a rectangular canal, and to $(l + t\,h)\ h$ in a trapezium canal. The proportion $\frac{s}{c}$, of the area to the moistened perimeter is equal to n. Let the mean velocity of the current $= \mathrm{v}$, and let $\mathrm{Q} =$ the waste. Then between these quantities the following proportions hold, $\mathrm{Q} = v\,s$, and $n\,p = 00036554\ (\mathrm{v}^2 + 0{\cdot}0664\ v)$; which, when resolved in respect to v gives $v = \sqrt{2736\,n\,p^2} - {\cdot}033$; equations in which, as all the quantities contained except one, are given, the latter may be determined.—For rectangular canals and aqueducts, it is proper to give them such dimensions, that the breadth may be nearly double the depth of water; that is, that $l = 2\,h$; whence $c = 4\,h$, $s = 2\,h^2$, and $n = \frac{h}{2}$; h will be given, as a function of the waste, by the formula $\mathrm{Q} = v\,s$, which becomes here $\mathrm{Q} = 2\,v\,h^2$, the velocity v being an arbitrary datum, depending on the slope and on the nature of the canal.—In the case of a trapezium canal, let m be the proportion of the breadth at bottom l, to the depth h; $l = m\,h$; and $s = h^2\ (m + t)$, equations which allow us to determine l and h, m being given. The value of t should be $\frac{1}{2}$—(1 of base to 2 of height)—for slopes of dry stones, 1, for those of vegetable mould, and 2 for such as are of sand, or slipping soil.—The area s is determined by dividing the waste of the canal by the mean velocity which the water should have. This mean velocity should be such that the velocity at the bottom, which is very nearly $\frac{3}{4}$ of it, be weak enough not to degrade the walls of the canal. The following table indicates the superior limits of the velocity which the water should have at the bottom of canals according to their nature, without wearing them away.

Nature of the Bottom.	Limit of Velocity Inches.
Weakened earth	2·5
Light soft clays	4·9
Sand	9·8
Gravel	19·7
Pebbles	19·9
Broken stones—flints	39·4
Agglomerate of pebbles—soft schist	49
Rocks in layers	59·1
Hard rocks	98

The velocity at the bottom, and, consequently, the mean velocity, being thus taken arbitrarily, and beneath the maximum limits just given, we may, by the foregoing formulæ, deduce the form to be given to the canal, and then its dimensions, as functions of the water, which it is to have. For great velocities, those of 40 inches and above, the value of Q just given must be replaced by the following $Q = 51\ s \sqrt{n\,p}$.—Next as to the *Source of the Water of Canals.*—Canals, with the exception of canals of navigation starting from the sea, obtain their water from reservoirs or basins placed at their head, which are most frequently portions of rivers, the level of which is raised for this purpose by a dam. The head of the canal at the point of entrance is sometimes entirely open, sometimes it is provided with a flood-gate.—*First. Canal free at its entrance.* The water, at its entrance into an open canal, forms a fall; its level falls to a certain distance, then it rises a little, by slight undulations, beyond which the fluid surface takes and preserves a form nearly plane and parallel to the bottom of the bed—its slope and outline being always considered constant. The velocity is accelerated from the top to the bottom of the fall; it then diminishes, during the rising of the surface, and, soon after, the movement proceeds in a manner sensibly uniform.—Let H be the height of the water in the reservoir above the line of entrance into the canal; h, the constant depth of the current after the movement has become uniform, and v the velocity of this movement; let D besides be the difference of level between the surface of water in the reservoir, and at the extremity of the canal, and L, the length of the latter; we shall have $H - h = 0{\cdot}06225\ v^2$; $-p\,L = D - (H - h)$; $v = \sqrt{2736\ np} - {\cdot}033$; and $Q = s\,v$. By means of these equations, we may determine the waste, the slope, or one of the dimensions of the canal; the other quantities being known.—Generally, such falls are intended *to utilize water as a motor.* Now the efficiency of a current *to turn machines* depends, not merely on the quantity of water, but also on the height of the fall, that is, on the difference of level between the surface of the reservoir and the point of the river where this water may be given back below the work. And this force is measured by the product of the quantity of water and the height of the fall. The greater the inclination that is given to the canal, the more water is used; and this increases one of the factors of the product, but, at the same time, the other factor, the fall, is diminished; and the result will be, that the product after having been at first increased with the slope, will then diminish, after which it will continue to increase: there is thus a *maximum of force,* which it is of importance to determine. This may be done by guess work;—by determining each of the factors of the product, and the product itself, therefore, for a series of falls, increasing, for instance, by thousandths, and stopping as soon as it begins to diminish.—*Canal, with flood-gates.* When a canal receives water by the opening of a flood-gate at its head, which is the case with almost all mill races, and when its charge on the centre of the orifice is considerable, and more than two or three times the height of this orifice, its higher limit, it is not covered by the water of the stream below, and the waste is given by the formula $Q = (5{\cdot}4)\ 3{\cdot}1 \cdot l\,h \sqrt{H}$, H being the charge on the orifice, l and h, the breadth and opening of the flood-gate. It will be enough to give the mill race, in such a case, an inclination so that the water to be expended may flow out, which we may readily calculate by the formulæ we have indicated when speaking of the movement of water in canals. If the water rise below the flood-gate to a considerable height above the upper border of the orifice, the charge of water H of the above equation, will be equal to the difference of the level of the water before and behind the flood-gates.—*Of Rivers.* We shall enter here into description only of the methods of measuring courses of water. Let us first indicate the method of discovering their velocities. The most simple means is by a floater which takes the velocity of the water. We begin to measure after it has attained the full velocity of the current, and in the strongest part of the stream. The mean velocity is about ·8 of the velocity thus observed (see BRIDGES). Very often the Woltmann's drum is used, in which is a turning shaft communicating by a screw channel with a meter, and carrying four small wings arranged as in a windmill. The current turns them, and we infer, from the number of revolutions N, made in a certain time T, which is indicated by the instrument itself, the velocity is directly inferred $v = a\ \frac{N}{T}$, a being a constant co-efficient for any given instrument, and determined by making it pass over a certain space in standing water, and dividing the space passed through by the number of turns made.—In order to measure great rivers, one takes a section of them, measures their area across, and the velocity (see BRIDGE); which two quantities are then multiplied. If the rivers are wide, the velocities will differ in different parts across the stream, and must be taken by averages over several measurements. In case of small water courses, such for instance as do not give more than 2 or 3 cubic yards of water per second, a dam is made in the water course, over which the water falls; then having measured the charge of water H, the width of the portion of the dam over which the water falls as l, and that of the dam as L, we calculate the waste of water by the formula $Q = (3{\cdot}04)\ 1{\cdot}77\ l\,H \sqrt{H}$,—when l is greater than $3\frac{1}{2}$ inches, and less than $\frac{L}{3}$, and $l\,H$ smaller than the fifth of the section of the current imme-

diately above the reservoir. When $l = L$, and H greater than $2\frac{1}{2}$ inches, and smaller than the fourth of the depth of the current behind the wall, the formula $Q = 1·96\ (3·39)\ L\ H \sqrt{H}$ is used. When H is above this fourth, the formula $Q = (3·33)\ 1·92\ L\ H \sqrt{H + ·115\, w^2}$, w being the velocity of the surface of the current at its reaching the dam, which is determined by experiment, and usually by the floater. The velocity of a river is slight when it is below 20 inches, it is ordinary, from about 24 to 40 inches, great when above that, and very great if it is more than 80 inches. The velocity of the Seine, about Paris, is from 24 to 25 inches, that of the Rhone and of the Rhine about 80 inches. A water course takes rank immediately among rivers, when, in its ordinary state, from 15 to 18 cubic yards of water pass in a second. From 45 to 60 is such as to make it about navigable. The Seine, at Paris, with a mean breadth of 144 yards, and a mean depth of 5 feet, gives 195 cubic yards of water per second; the Garonne at Toulouse, in its ordinary condition 225; the Rhone nearly 900 at Lyons; and the Rhine, at Strasbourg, 1,425, and at Nimeguen, before its junction with the Meuse, 2,550.—*Of the Movement of Water in Conduit Pipes.* Take first the case of a simple conduit; if we call H the charge of the conduit or the vertical height between the orifice of efflux and the surface of the fluid in the reservoir; D being the diameter of the conduit, L its length, and Q, the waste of water per second, we shall have

$$(1.)\quad Q = 21·22 \sqrt{\frac{H\ D^2}{L} - ·0216 \cdot D^2},$$

which, for velocities above 24 inches, will be approximately

$$Q = 20·3 \sqrt{\frac{H\ D^2}{L}}.$$

Very often the waste and the charge of water is given, and the diameter is required. D is first determined by the formula (2) which gives

$$D = ·298 \sqrt[5]{\frac{L\ Q^2}{H}},$$

which is sufficient for velocities above 24 inches. When the velocity

$$\frac{4\ Q}{\pi\ D^2}$$

is less, the value of D, thus found will be a little too small; it will then be augmented gradually, by substituting it every time in the value (1), of Q until we come to a value of the second member, above that of the waste which we are to have.—We have, in the above, taken for granted that the conduits are entirely open at their extremity; but (almost always) they are closed by cocks or spouts which narrow the opening. In that case, for velocities above 20 inches, as is most usual, we have

$$Q = 20·73 \sqrt{\frac{H\ D^2}{L + 35·47\ .\ \frac{D^5}{m^2\ d^4}}},$$

and

$$D = ·298 \times \sqrt[5]{\frac{L\ Q^2}{H - ·0826 \times \frac{Q^2}{m\ d^2}}},$$

d being the diameter of the spout at its orifice, and m the co-efficient of contraction.—Let us now suppose that we have to deal with the establishment of a system of water conduits intended, for instance, for supplying a town,—where usually a chief conduit is used, supplying numerous branches that conduct different volumes. First, we draw a plan of the conduit and its branches—multiply by $\frac{4}{3}$ the waste of water of each pipe, to make up for obstructions, turnings, and other accidental resistances, then taking a pipe of given diameter we calculate the partial wastes of charges occasioned by the successive transport of the volumes which each portion of the conduit must conduct. All these for the total waste up to the last orifice; then we see whether the remaining charge is sufficient to insure the efflux of the volume of water which is to pass through that orifice; and in this way we determine, by guess at first, the diameter of the principal conduit, and of its branches. The loss of charge is the difference between H and the *effective charge* or height $\frac{v^2}{2g}$, to which the velocity at the extremity of the part of the conduit considered would be due, and is represented by $H - \frac{v^2}{2\,g} = H - \frac{2\ Q^2}{g\ \pi\ D^2}$. These calculations are abridged by making use of tables which give this value per running yard, and according to the diameter of the conduit pipe and its waste. Such tables will be found in treatises of hydraulics.—Often when we have a sufficient motive charge, economy requires us to narrow the diameter of the conduits, so that the volume of water they ought to give, diminishes. It is necessary in that case to convince ourselves, by calculating the portions of the charges consumed by each part of the conduit, that the water would rise, at the place of each orifice, to a sufficient height to insure the efflux of the required volume.—The turnings or changes of direction should always be rounded; we may then neglect the losses of charge that arise from them, which become indeed very small in proportion to those of friction.—In establishing water pipes, M. D'Aubuisson recommends besides,—1st, In place of one single conduit or thread of pipes conveying a certain volume of water, to have two, one beside the other, each of which conveys half the volume, an arrangement which, indeed, augments from 25 to

30 per cent. the first cost, but which gives the great advantage of being able to insure, at all times, the continuity of the stream on all the principal points. 2d, To make the two pipes end on both sides, in a little cast iron reservoir which may serve as a reservoir of re-distribution. 3d, To place the principal conduits in subterranean galleries which renders their inspection and repairing a very easy matter; as for the secondary ones, it is enough to sink them about a yard below the pavement.—Usually, at the culminating points of the pipes, floating valves are placed, in order to give issue to the air which collects there; the water-posts which are established at the head of two streets to wash the two slopes, serve equally well as air holes.—At the lower parts of the conduit pipes, and at the top of the re-entrant angles, are fitted great *discharge cocks*, which are opened sometimes for cleaning the pipes by driving through them as much water as possible.—The entrance of all conduits starting from the reservoir, or the subordinate ones of Aubuisson, as well as those of the branches, should be supplied with a cock to throw back, or give free passage to the water; for pipes of a diameter more than 4 inches, sluice cocks are used, the opening of which is shut by means of a shovel suitably disposed, which is raised or lowered by a screw. Below 4 inches, ordinary turning cocks can be used.—*Of the Measurement of Distributions of Water.* It is important to measure at every instant the quantities of water which pass through the water orifices of distribution; to do for water, what the meter does for gas. Up to this time, the following is the only plan employed for small orifices. The dimension of the orifice is measured exactly—the pipe being supposed full and under a constant pressure; from which it is inferred, as an experimental result, that it must supply so many cubic metres in the unit of time.—*Cisterns of Measurement* and distribution. Let us now see how the number of *inches of water*, which a conduit pipe or pump

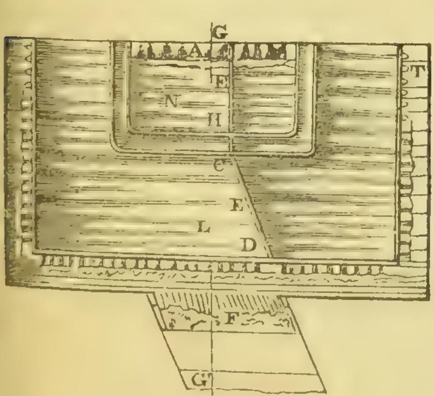

Fig. 1.

brings into a reservoir, is measured. The *inch of water* is thus determined. A circular orifice of ·786 inch in diameter is made in a wall, and supplied with a cylindrical spout of 5·68 inches in length, the level of the water in the reservoir being kept 1·18 inches above the upper part of the orifice, the quantity of water which flows out, is 20 cubic metres in 24 hours.—To explain how to measure as above, we may describe the measure cistern established at the top of the aqueduct at Marly, which is meant to measure the products of several pumps, some moved by hydraulic wheels, others by a steam engine which raises the waters of the Seine up to the aqueduct. Fig. 1 represents the plan of this cistern. Fig. 2 is a section made along the

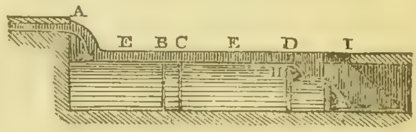

Fig. 2.

line G G′ of the plan. The water raised by the pumps comes to A, where it falls in a sheet into a rectangular reservoir. Two partitions B C envelop the part N of this reservoir, without going to the bottom, so as to prevent the movements occasioned on the surface by the water which comes to A, from transmission into the remaining part L; the water passes from N to L beneath these two partitions, and its free surface in the whole extent of the latter part of the apparatus L is thus made perfectly tranquil. The partition D, which serves as a limit for the reservoir, and extends in three different directions, carries a great number of orifices H in its whole length; the water leaves the reservoir by these different openings, and falls into a trench, outside the partition D, and in all its length; from that it falls at F into a covered canal which takes it to the other extremity of the aqueduct. A partition E E divides the reservoir L N into two completely distinct parts; that on the right receives the waters which come from the pumps moved by the hydraulic wheels; that on the left receives those supplied by the pumps of the steam engine.—By this arrangement, the waters which come from the two systems of pumps, meet only after passing through the orifices of the partition D, that is, after having been measured, in the way we are to explain. Suppose that the water leaves the reservoir L N, passing always through the same number of holes of the partition D, we may conceive easily that the level, which it will take in the reservoir, will be more or less elevated above these holes, as the pumps supply more or less water in a given time. If, on the other hand, a certain number of orifices of the partition D be shut by means of stoppers, the level of the water in the reservoir L N will be raised, for one same quantity of water supplied by the pumps; for, as the number of orifices of efflux is diminished, the velocity with

which the water passes through each must be increased, if the same quantity has to pass through. We may then vary, at will, the position of the level in L N by closing more or fewer of these openings; and this power is used so that this level may coincide with a mark fixed at the partition D, in A. When this coincidence of level of the water with the mark is established permanently for some time, it is enough to count the holes that remain open in order to obtain immediately the number of *inches of water* which the pumps supply.—In the measurement cistern at Marly, the part of the reservoir used for gauging the water brought by the hydraulic wheels has sixty orifices; the part corresponding to the water supplied by the steam engine has thirty. If, for instance, we found that the pumps moved by the hydraulic wheels raise 60 *inches of water* to the aqueduct, that would be equivalent to saying that they raise 60 times 20 cubic metres, or 1,200 cubic metres in 24 hours (that is, 1,560 cubic yards).—The distribution of waters between the four corners of a town, and even between the different places which have got water distributed to them, is effected by help of cisterns quite analogous to that above. The whole mass of water to be distributed passes into a reservoir whence it issues by openings made all along it, and it is disposed by tubes or conduit pipes which divide the whole mass of water, so that everybody receives from it the water which proceeds from a definite number of openings.—*Meters.* The systems above, are in many cases insufficient, where a meter, like one for gas, to accommodate itself to all circumstances of actual consumpt would be required. There is no efficient instrument for this purpose. We may distinguish two cases—the first where the efflux is from a pipe constantly kept full of water; the second where the pipe is not so. In the first case, we may determine the velocity of the water by suspending a little and very light *screw* at the middle of the tube, the velocity of which measures that of the water—the number of cubic yards passing through the orifice, being proportionate to the number of turns of the screw. If, then, the number of turns of the screw be registered by a simple meter, obtained by help of toothed wheels and endless screws, the axis of the first wheel being mounted on the axis of the screw, we may read on a dial-plate the number of cubic yards which have passed through the tube. This system has been proposed and put in practice slightly by M. Lapointe. He had proposed it in order to measure a course of water previously dammed. The results appeared to him so regular as to admit of its application even for pretty sensible differences of level. In the second case, when the pipe is not always full, an apparatus a little more complicated may be used. If we dispose, on the pillar of a floater, at the surface of the water, a little pinion as in the dynanometer; and, on the other hand, a system like the foregoing making a cylinder—partially toothed—turn round, it is clear that a second toothed cylinder, moved by the pinion, will register a number of teeth proportional at once to the position of the floater and the velocity of the water; to the product of the path through which the water has come, by the section of it, and consequently, the volume of water which has flowed out of the sections of the toothed cylinder must be proportional to the surface of the section of the water which the pipe contains.—*Water Jets.* Openings in their *wall*, carry jets highest, and give them the most solid form; on examining them we may often fancy we see a transparent bar of crystal; hence these are made use of, when only the elevation and the beauty of the jet are to be taken into account. Conical spouts also give united and transparent jets whose height, however, is not more than ·8 or ·9 of the foregoing. Lastly, cylindrical spouts give jets, disturbed from the point of departure, and which rise only to $\frac{3}{4}$ of the height of a jet of an orifice in their wall, with the same charge. Let h be the effective charge, h' the height of the jet with an opening in their wall, the latter will be given by the formula $h' = h - \cdot 01 \times h^2$.

(3.) *Of Water used as a Motor.*—There are two classes of water machines, the one possessing an alternating movement, the other, a movement of continuous rotation—for the latter class see WATER-WHEELS and TURBINE. To the first class belong Heros Fountain—the Hydraulic Balance—the water balance, and the column of water machine.

1. *Heros Fountain.*—This is also called Schemnitz's machine, unimportant.

2. *The Hydraulic Balance.*—This machine, in which the water acts by its own weight, has the inconvenience of alternate movement machines, in which the water after the charge in the direction of its motion retains its velocity. The simplest consists in a vessel moveable at its middle round an axis, and divided into two by a partition; the water falls successively into each compartment until its weight drags it down and empties it.—A more complete scheme, though still defective, is represented in fig. 4, when one of the vessels contains enough water it descends in virtue of the excess of its weight, and the valve opening upwards

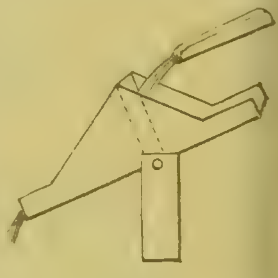

Fig. 3.

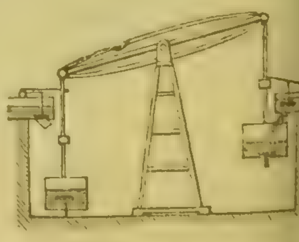

Fig. 4.

placed at its bottom, striking against an obstacle, opens and frees the liquid contained; during this time, the vessel at the other extremity of the balance is lifted up; a catch placed on the connecting rod which carries it, raises the sluice at the bottom of the feeding reservoir, so that this vessel fills as the other empties. — The only important use of such machines, which is exceedingly simple, and therefore very valuable, is in the WATER BALANCE made use of to move the pumps in certain mines; there is only one cask which raises a weight at the upper part of the beam of the pumps.—The WATER RAM, which was invented by Montgolfier towards the close of

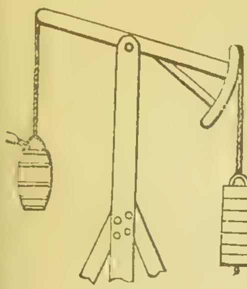

Fig. 5.

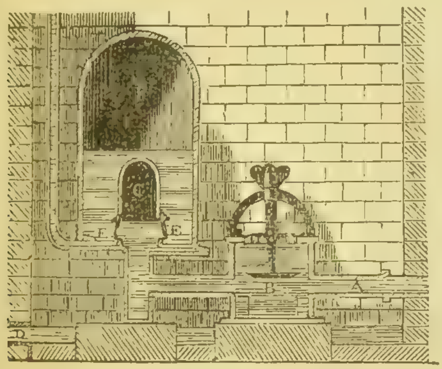

Fig. 6.

last century, consists independently of the feeding reservoir of a pipe or *body of the ram*, which carries the water to the operating head of the machine; this part, or *head of the ram*, consists of a short tube, straight or bent, at the upper part of which, as well as at its end, are two ordinary valves, called, the first, the *stop valve;* the second, the *ascension valve;* the extremity enters into a bell filled with air in the upper part and the lower of which, being filled with water, receives the ascension tube. The valve of ascension being closed, the water will come from the reservoir with increasing velocity, will leave first by the stop valve, will soon shut it, then striking with the *vis viva*, which it has acquired against the ascension valve, it will open it, penetrate into the reservoir of air, will compress it, and will make the water in the ascension tube rise; soon the elasticity of the compressed air, and the weight of the water of the ascension pipe will have absorbed partly, the *vis viva* acquired with the water, and will impress on it a powerful movement; the ascension valve will shut, then, in consequence of the retrograde movement of the water, there will be formed a void under the stop valve which will open, and so on.—COLUMN OF WATER MACHINE. This sort of machine consisted of a cylinder, or great body of a pump in

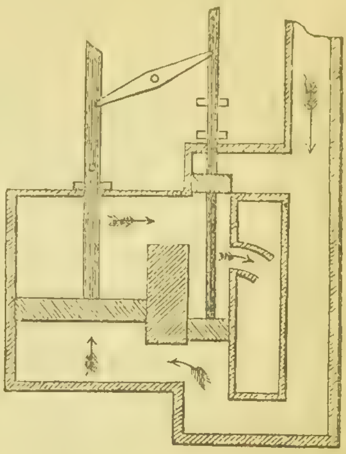

Fig. 7.

which a piston moves, driven by the weight of a high column of water contained within a mounting tube. There is adapted to the pillar of the piston of these machines, especially used for exhaustions, a balance which usually puts pipes in movement; rarely, the alternate movement is transferred into one of rotation by means of a suitable mechanism. The column of water machines are simple or double, the first being oftenest used. The machine itself regulates the distribution of slide-valves and cocks which are put in motion by the pillar of the great piston of the machine.

(4.) *Machines for Raising Water.* — A few machines chiefly valuable for their simplicity and their adaptation to local circumstances are made use of for this purpose. The principal of these are the screw of ARCHIMEDES and the PUMP, which are described in special articles.

Hydro-dynamics. The name applied to that very important department of mechanical science which treats of the motion of fluids. Its laws and principles are demonstrated almost exclusively by mathematical reasonings of the most intricate kind; but we shall here give only such general or experimental proofs as may be generally understood.— When a fluid is contained in a vessel, it makes a certain effort to escape, or to reach the level of the lowest adjacent ground. This effort must be restrained by some equal resistance; and this resistance is presented by the immobility of the wall. If these two be balanced, there will be a case of equilibrium; and, if not, the wall will be broken and motion must result.— In establishing experimentally the laws of hydrodynamics, there is one circumstance to be taken into account, capable of causing apparent discrepancies between theoretical and practical results: we mean the friction of fluids against the

sides of the vessel from which they flow. If the thickness of such a vessel be considerable, there will be, as we shall see hereafter, very considerable effect due to friction in the passage. Again, if the vase be small in proportion to the size of the orifice, there will be a rush of water to it more than it can emit, and part of its force will be spent in striking on the wall near the orifice.—Let us suppose at the outset that the vase is one of large dimensions, and besides, with very thin walls; in other words, that the two effects just spoken of, and always producing decided enough results in practice, are non-existent. In such circumstances, the following theorem, known as Torricelli's, holds good, viz., that the particles issuing from the orifice do so with the same velocity as if they had fallen from the height of the level. That this should be the case, one naturally enough expects; inasmuch as each particle of fluid right above the orifice, may be imagined simply falling down: and, as the whole work done in this operation is expressed by the fluid moving with such a velocity, we should anticipate the foregoing result; although this consideration by no means approaches to positive proof. It may be shown to be true by experiment as well as by theory.—Various consequences are very clearly deducible from this law, for instance, the following:—1°; the velocity with which the fluid issues forth depends solely on the depth of the orifice below the water, and not in any degree on the nature of the fluid. This will at all events be the case, if the analogy between a falling body and the water be not merely accidental but grounded on facts. In that case the actual mass or weight of the bodies falling, may differ enormously, but the velocity at a certain part of the fall remains the same. Thus, mercury and water fall from the same height in the same time. They will, therefore, also flow out from a vessel at the same rate. 2°; for one same fluid, the velocity of the mass issuing at the orifice is proportional to the square root of the depths of the orifices below the level. Preserving the analogy that has been established between a falling body and an issuing fluid, this becomes a necessary consequence. In the article on ACCELERATING FORCE we saw that $v^2 = 2\,g\,.\,s$, where v is the velocity at any moment, s the space through which the body has fallen from rest, and g the force of gravity. Hence, $v^2 = 2\,g\,s$ in the case of the fluid, and $v = \sqrt{2\,g\,s} = \sqrt{2\,g} \times \sqrt{s}$. It is, therefore, proportional to $\sqrt{s}$, the latter being the height of the level above the orifice. 3°; if there be a pressure acting at the surface, in addition to that of the mere weight of the fluid, this may be represented by supposing a column of water of such height, that the product of the height into the section of the vase at the surface shall be equal to the weight. The whole results which would be obtained in that case, would actually obtain in this, except that the whole would cease when there was no more water in the vase.—In order to demonstrate this proposition of Torricelli, with these three corollaries deduced from it by experiment, it is needful that opportunity for observing experimental results should be devised. The first essential is that the level be kept constant. Using any of the contrivances for securing the constancy of level and of velocity, we may institute experimental researches. Let us see what results Torricelli's theorem would lead us to anticipate, and compare them with those obtained. The formula is $v^2 = 2\,g\,s$. Now g is about $32 \cdot 2$ at Glasgow (g varying over the Earth, see PENDULUM, and Figure of the EARTH, about $\cdot 2$), that is, a body falling at Glasgow acquires a velocity of $32 \cdot 2$ feet per second in the first second. Hence,

$$v^2 = 2 \times 32 \cdot 2 \times s = 64 \cdot 4 \times s$$
$$\therefore v = 8 \cdot 025 \times \sqrt{s},$$

where s is the height in feet, of the space passed through by the liquid passing from the level to the centre of the orifice. This then is the theoretic velocity. Now, multitudes of experimenters have examined with all mechanical advantages the truth of this formula; and the result uniformly indicates that the theoretic velocity is $1\frac{1}{2}$ time the actual velocity; or this latter two-thirds of the former; so that the theorem of Torricelli, resting though it does upon an analogy so apparently trustworthy, is seriously discredited. Theory and experiment are reconciled by what is called *the contraction of the fluid vein.* When the fluid is emitted, the mass, before separating into drops, grows sensibly narrower the farther it is from the aperture. It was thought for some time that there exists a *maximum of contraction*, through which the fluid mass passes, and then becomes wider before being broken up. Savârt, however, has shown that no such thing occurs, but that there is a constant contraction from the point of emission to that where the fluid disintegrates; which contraction, at first very rapid, becomes very slight at a distance from the orifice equal to its diameter. If we take it then that the mass issuing from the orifice with a velocity due to the fall, has a section the same as at this point, and that the rest of the fluid filling the remaining part of the orifice, moves because it is dragged on by this—it will be seen that the velocity due to the fall of water from the level must be lessened by the distribution of the force producing it, over a greater portion of water than is really affected by it, and that this is enough to cause the apparent discrepancy. Suppose that the section of which we have spoken is only $\frac{2}{3}$ the size of the orifice. Then the velocity measured, being that which the motion of this $\frac{2}{3}$ of water produces on 1 of water, will require to be multiplied by $\frac{3}{2}$ in order to get the original velocity due to the fall. And as the velocity measured is $\frac{2}{3}$ of that inferred, the

velocity due to the fall, according to experiment ($\frac{2}{3} \times \frac{3}{2}$) coincides with the theoretic velocity according to Torricelli's theorem.—The jet of water which flows out from an orifice in the side of a vessel takes a *parabolic form*, and the actual outline varies with the angle of inclination to the horizon at which it is emitted. The parabolic form is a simple consequence of the law of gravitation; which impresses it on all bodies set in motion by a force once applied and then ceasing, and kept under the action of gravity. As each successive section of water is acted on in quite the same way, by quite the same forces, when the constancy of level is preserved, each describes the very path that the preceding one had, and thus, by their coherence, the parabolic curve is actually made visible. If the level is allowed to vary, the initial force alters and the various sections will describe different curves, each one slightly differing, but for a while not sensibly from the one before it, so that the figure will be a sort of composite of various parabolas nearly coinciding, and will form what is called the envelope.—There are several very curious and interesting results regarding the form of jets and their internal constitution, which Savârt has demonstrated, the mechanical causes of which we cannot here explain at length. One may readily see, however, that, in the eddies caused round the mouth of the orifice, by some water above it striving to get out and coming into contact with the resisting surface, and by some below it being moved by some of the mass falling downwards, there is sufficient explanation.—We have supposed that one section of water issued in a direction quite perpendicular to the surface of the vase, and that another exactly like it followed, which was followed by another, again, and so on.

Figs. 1, 2.

So far this is true. The issuing water is always perpendicular to the side of the vase, but it receives rotatory motion as well as forward motion, (from these *eddies*, as we may call the disturbing actions indicated above). Moreover, it is not at all so certain that each successive section of water is of the same form. If it be so, the irregularities of this rotatory motion cause a certain disintegration of each, and between the thicker and what we might call the *normal* drops there are smaller inserted ones, as if torn off from the mass of the larger. Thus, a mass of fluid descending from a height vertically takes the outline represented in fig. 1, while its actual constitution is represented by fig. 2. These horizontal and vertical elongations occupy fixed positions in the fluid vein, and there must, therefore, be some such universally acting cause as we have supposed, in rotation of the drops. The little drops which we see between the larger ones cause the appearance indicated in fig. 1, of a uniform pipe round which water is heaped at the thickest parts of the vein. Savârt has shown that each drop is originated by a sort of annular enlargement at the orifice, propagated along the whole length of the nearly uniform stream of water, until disunion of the particles occurs; and that therefore there is a succession of such pulsations at the orifice. Their number is in the direct ratio of the velocity of emission, and in the inverse ratio of the diameter of the orifice.—A very remarkable phenomenon connected with acoustics is noticeable here. These pulsations are regular and continuous enough to give rise to a musical note, and if, with a musical instrument, we produce the same note at some distance from the place, the pulsations become much more regular, and extend upward almost the whole length from *n* to *a*. They seem to occasion no change in the amount emitted or in the velocity of emission. The air set in motion by the sounding instrument, transmits its vibrations to the fluid, which repeats them naturally, as they do not disturb greatly the equilibrium of its particles, which are in the circumstances very susceptible of the slightest impression from forces not counteracting that of gravity. All these phenomena, as well of the shape of the vein as the sound produced by emission, are obtained whatever be the form of the orifice and whatever the direction of emission,—if it be downwards, or if it be horizontal, or below 45° from the horizon. If the orifices be not circular, very remarkable variations appear in the form of the successive sections. Thus, at distances represented by 2, 3, and 4, supposing the side of the orifice to be 2, and it to be a square, the forms given in fig. 3 are found.—Frequently, when a

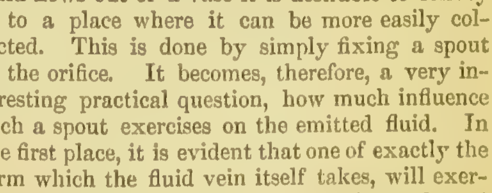

Fig. 3.

fluid flows out of a vase it is desirable to convey it to a place where it can be more easily collected. This is done by simply fixing a spout in the orifice. It becomes, therefore, a very interesting practical question, how much influence such a spout exercises on the emitted fluid. In the first place, it is evident that one of exactly the form which the fluid vein itself takes, will exercise no influence whatever. It can be made and fitted so that the fluid shall be freed from disturbing atmospheric influences, but shall not at all touch the inside of the tube. To accomplish this last, however, is not easy; but if the surface inside be very carefully polished, there will be, even when the fluid does touch, scarcely any difference in the amount emitted. In fact, much of the retarding power of such spouts arises from the fric-

tion, if it may be so called, of the fluid against their surfaces. Particles of fluid getting in among these uneven parts are retarded, and keep back other particles. If the interior surface be in any case well polished, this effect will be reduced to a minimum.—Orifices themselves have an influence like that of spouts. A curved side of a vase, for instance, pierced by an orifice, emits more water if the concavity be turned towards the enclosed fluid, and less if it be turned away from it than an opening of the same size in a plane side of a vase. As the opening in any actual vase (which must have a determinate thickness) is of the nature of a spout, this consequence might have been anticipated. — Whenever the fluid issuing, does not touch the sides of the spout it is evident enough that there will be no influence exerted on the fluid. When it becomes adherent, that is, when the fluid attaches to the spout, there is, in one of a cylindrical form, an increase of the velocity and an increase of the quantity of liquid which passes through, in the proportion of about 4 to 3; if the diameter of the spout be nearly one-fourth part of its length, the vein is always adherent with feeble pressures above; but with great pressures it is frequently quite uninfluenced by the spout. Any light obstacle, however, interposed in the tube will produce adherence. The form which the vein takes in passing through a tube is very like what it would take when passing through the air, even when adherence is established; as one may become readily convinced by using a glass tube through which the passage of the current is visible. A conical spout increases the quantity which passes out still more than a cylindrical one. These results depend, however, on the uniformity of the tube. If it be, instead of uniform, blown out in some places, like the tubes used in many processes of organic chemistry, it would lessen the amount instead of increasing it. Spouts may therefore be adapted to orifices which shall either increase or diminish the velocity of the fluid passing through them.—When a fluid passes through such a tube, it will or will not produce certain amounts of pressure upon the sides. Where the tube is of the exact form of the natural vein of water, it is evident that there will be no pressure. Where, on the contrary, it is of any other form there may be either a pressure outward or a suction inward. No very accurate experimental or theoretical determinations have been yet made of the amount of this pressure or suction. Daniel Bernouilli states the following as the true expression of it:—Let the velocity of the particles of the section for which we desire to determine the pressure be noted. This velocity, according to Torricelli's theorem, is the same as would be produced in water flowing from an orifice at a depth (h') below the level. Now, suppose that the tube beyond this were cut off, a new velocity would in all likelihood be given to the particles, due to a depth (h) below the level. The pressure or suction then may be expressed by $h - h'$. As we have already said, we cannot absolutely rely on this determination, which assumes the non-existence of any heating effects in the motion of fluids. In some instances its indicated results are, however, remarkably in accordance with the experiment. Thus, in cylindrical tubes, when the fluid adheres, the effective velocity is greater than the theoretical velocity, and as these may be represented by h' and h, there is a certain amount of suction, which ought to be, as it is, perceptible.—It is a principle of universal application in mechanics that action and reaction are equal and opposite, and that motion is produced only by motion destroyed. When, therefore, water flows out of a vase there ought to be some movement produced on the vessel itself. If the water flows out horizontally, the motion of the vessel ought to be horizontal. The truth of this anticipation may be demonstrated without difficulty by setting a vase upon very fine wheels, on a very smooth surface, and opening a screw in it, by means of which motion will be produced in the opposite direction. This is prettily illustrated in an instrument called the hydraulic tourniquet. It consists of a vase fixed on a vertical axis round which it may move, and having two issues horizontally, as in the figure, turned different ways, perpendicular to the radius of the circle, or as tangents to the circle in which alone they may move. In this way the vase of water is made to rotate, and its motion may be communicated elsewhere. It was long believed, on Newton's authority, that the amount of this recoil might be measured by the weight of a liquid column, having the contracted issuing vein for base, and the height of the level for its height; but it was shown by Daniel Bernouilli that double this weight gives the correct representation of the height of the equivalent column. The hydraulic wheel, or *turbine*, rests upon this principle.—The whole theory of the effluence of water from vases in this way applies evidently to the construction of what are called in this country falling fountains, or *jets d'eau*. If the original direction of the water were vertical, the direction would be very nearly that of a straight line, the water being scattered in falling back on itself. If, as is usual, the wall in which the orifice is pierced be slightly inclined to the horizontal line, parabolas of more or less curvature will be obtained. The application of the theorem of Torricelli with the modifications pointed out, will, in every case, give

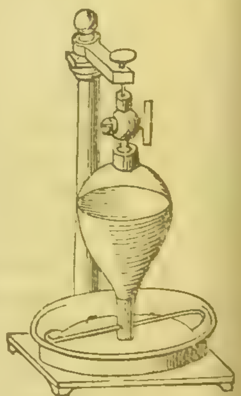

Fig. 4.

the height to which a jet can rise, and the theory of parabolic curves will enable us to describe its whole course when we have given either the height of the level from which it is supplied with water, or that representing the pressure to which the water with which it is supplied, is subjected. —It remains for us to notice several most interesting discoveries of Savârt's, relative to the forms which a fluid takes striking on a solid or on a fluid. We can give but one or two of the results here. When a rising column of water impinges against a horizontal disc of a certain size the remarkable appearance of fig. 5 is presented. The inner space is a transparent

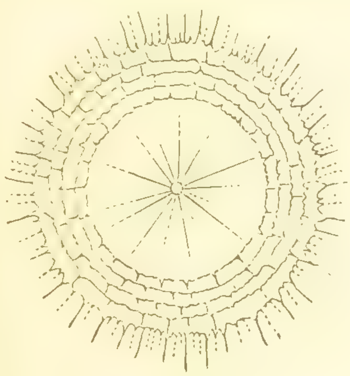

Fig. 5.

sheet of fluid; the outer a sort of streaked and marked space along which lines of fluid stretch out, rapidly falling back in a sort of spray. The transparent sheet sinks or rises (expanding or contracting), and the streaked space seems to undergo the same changes. The pulsations are regular—as in the case of the stream of water already noted—and may be made to produce a sound. According as the position of the intercepting solid is nearer to or more remote from the height to which the fluid if undisturbed would rise, so the diameter of the transparent sheet increases, while the extent of its striated ring diminishes. When a certain point is reached, this transparent sheet has attained its maximum diameter, and the striated ring entirely disappears. We can only give an idea of the many results to which the very beautiful experiments of Savârt have been able to lead him. The forms presented when the intercepting solid is not perpendicular to the stream of water are different from those represented.—The same author has entered into investigations of the cases when one vein of liquid strikes against another. We cannot attempt here to give even an outline of his researches or their results. They will be found very fully described in the *Annales de Chimie et de Physique*, vol. 54, and a short and clear account of them is contained in Pouillet's *Traitè de Physique*, vol. 1.—The mathematical theory of hydro-dynamics is still so obscure and imperfect that we have not chosen to offer any account of it in this place. If we could grasp within definite and manageable formulæ the molecular forces that determine the condition of fluidity or liquidity, it cannot be doubted that the more general theorems of *Dynamics* would become immediately applicable to all problems of the motion of liquids. It were vain to pretend, however, that this has at all been accomplished. See Hydraulics.

Before closing this article some recent investigations regarding the forms of jets of water demand our notice. Writers subsequent to the time of Sir I. Newton, Michelotti, Ettelwein, Bidone, Poncelet, and Lesbros, have given various descriptions and delineations of those jets. The spiral form which they frequently assume, asserted by Bidone to be illusory, was by the others justly maintained to be real. In attempting to account for the dilations on the surface (see page 370) to which he gave the name of ventral segments, Savary maintains that the efflux itself imparts a vibratory motion to the liquid as it passes through the orifice, and that the pulsations, being perpendicular to the plane of the orifice, alternately press out and draw in the liquid as it issues, and thus occasion the annular protuberances in question.—Professor Magnus has since (in the *Philosophical Magazine* for February and March, 1856,) undertaken a very interesting investigation into the forms of jets issuing from apertures of all kinds. Regarding every jet as composed of an indefinite number of jets united, he justly considers that the best mode of investigating the peculiarities of a single one is to observe the results of the confluence of two. He commences with the collision of two equal cylindrical jets, of equal velocities, meeting centrally from opposite directions. These he shows, as Savart had done before, spread out, at their confluence, into a circular plate perpendicular to the axes of the jets. When two such jets meet obliquely, but centrally, they again, by their collision, throw out a flat plate, not circular but elongated, in the direction of a plane bisecting the angle formed by the two axes, and perpendicular to the plane in which they are. The cause of this is evident; for the force of each jet may be resolved into two forces—one parallel to the first mentioned plane, and the other perpendicular to it, the latter causing the ejection of the plate in a plane perpendicular to the direction of the force, as in the former case. While the water thus spreads itself out laterally, its progressive movement does not cease, and the plate, by its cohesion, contracts in width as it advances, particularly at the edges, where it collects into two new jets converging towards each other. These, when they meet again, throw out a new plate perpendicular to the first, which goes through the same process. We have therefore a succession of plates, of elliptical forms, in two perpendicular planes, exactly resembling the alternate links of

a common chain.—If, again, two jets meet obliquely, but not centrally, the same liquid plate is formed by those parts which meet, not in this instance flat, but twisted in consequence of its cohesion with the unbroken parts of the jets, being spread between them as a connecting film. Unless the contact of the jets is too slight, or their angular inclination or velocity too great, the same cohesion with the connecting film will gradually deflect the unbroken portions of the jets from their original directions, and will keep them, for some time at least, from separation, causing them to wind in spiral convolutions round each other.—The two last-mentioned cases will enable us to understand the appearances presented by single jets issuing from orifices of various forms, these orifices being cut in thin plates inserted in the bottom of a vessel full of water, in its centre, and being small compared with the magnitude of the vessel. First, let the orifice be of a rectangular form, with the length much greater than the breadth. As the film of water makes its exit, its edges approach one another, each of them being drawn together so as to form almost a cylindrical jet. These two jets, meeting at an acute angle, throw out, between them, the plate described in the second case of two cylindrical jets, in a plane perpendicular to the original film; and the subsequent successive modifications of the jets are the same in both cases. Any small object presented internally at either extremity of the orifice, and on one side of it, converts the chain movement, as we may call it, into a spiral one.—Orifices of other forms produce jets of singular variety, but all formed on the same principle: that is, any two equal portions of a jet, collapsing, form plates in planes intermediate to them, or in the extensions of such planes, and the re-collapsing of these plates throws out others in the same manner. In all these orifices, slight modifications of their form readily give to the jets a spiral twist. A particular instance is that of a jet issuing from a square orifice, the successive changes of which we have described in page 370.—Independently of modifications in the form of the aperture, there is another very common cause of jets assuming the spiral form. That cause is motion in the cistern from which the water is discharged. The slightest movement in the water within it, even when imperceptible to the eye, is sufficient to produce that effect. Professor Magnus has not sufficiently explained the reason: it is this:—Every motion in the water soon resolves itself into one of rotation, because all other motions are destroyed by the sides of the vessel. When a rotation has once begun, any portion of the water, in moving from the circumference to the centre, retains the velocity of rotation which it had previously: but that uniform progressive velocity becomes, every moment, a greater angular velocity, until that which, near the exterior, was imperceptible, becomes, on emerging at the centre, a rapid whirl.—Even

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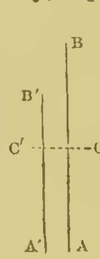
when there is perfect stillness in the water within the vessel, a rotation will take place in a discharge from a central orifice. Some light has been thrown upon this by the discussions which arose regarding M. Foucault's famous pendulum experiment. Everything on the earth's surface has, in consequence of the earth's rotation, two motions; one, a revolution round the earth's axis in twenty-four hours; the other, a rotation upon an axis of its own, parallel to that of the earth, in the same period. The latter, in the case of a vessel of water, may be resolved into two, one parallel to the surface, and another perpendicular to it. Neither of these is perceptible to the eye, or, under ordinary circumstances, to any experimental test, because the vessel itself, and everything around it, partake of the same movement. The horizontal rotation, amounting, in the latitude of Britain, to somewhere about one-fifth of a degree per minute of time, the liquid, though nominally and apparently at rest, has that amount of angular rotation round a vertical axis. When any portion of the liquid has been drawn in, towards a central orifice, so that it has approached within one-tenth of its original distance from the centre, the angular rotation of one-fifth of a degree is converted into one of two degrees, thus passing from a state of apparent rest to one of perceptible rotation. This, however, will scarcely be discernible in the jet if the vessel is kept constantly full; but, when allowed to empty itself, as was the case in Magnus's experiments, the angular rotation of the water near the orifice is gradually imparted to the whole quantity remaining in the vessel, and that again increased as each particle approaches the centre, till, at last, a very perceptible spiral movement takes place in a jet issuing from a circular orifice, although the water itself was at first perfectly at rest.—In order to observe the jets free from all such rotation, Magnus introduces what he calls a *tranquillizer*, consisting of four vertical plates of metal radiating from the centre, but not reaching it. Perfect tranquillity being thus obtained, and the jet from the circular orifice carefully observed, Magnus comes to the conclusion that a descending jet, from such an orifice, so long as it remains unbroken, is a column constantly diminishing in diameter, but perfectly smooth, and subject to no irregularities of any kind, provided that no bubbles of air have been permitted to enter, that it be kept free from vibrations of all kinds, and that all causes of disturbance be carefully removed. In such a jet, he denies the existence of Savart's ventral segments, perceptibly at least, and is even disposed to question the existence of the *vena contracta*, or at all events of any particular section to which that name can be peculiarly applied, since, as he says, there is no section of minimum area. The ultimate separation of the mass he attributes to the tension resulting from the acceleration in the velocity of the falling liquid, which tension, at a sufficient

distance from the orifice, becomes strong enough to overcome the cohesion between two adjoining sections of the jet.—In regard to the *vena contracta*, however, although the column continually diminishes in diameter, yet it diminishes at first so much more rapidly than afterwards, as to indicate a change in the cause of contraction, and, at the section where the one law ceases and the other commences to affect the form of the jet, *there* evidently is the position of the contraction in question. Its exact place may not be easily determined experimentally; but its exact diameter is; and, had Professor Magnus experimented upon an upward instead of a downward jet, he would have seen that there is then an actual section of greatest contraction.—M. Plateau (*Phil. Mag.*, for Oct., 1856), after using various arguments to prove that the reason assigned by Magnus, for the ultimate separation of the jet, is not tenable, maintains the existence of the ventral segments in jets from circular orifices, even when disturbances from vibrations, and from all other causes, have been carefully guarded against. He then describes a cause of a different kind which he considers adequate to account for these segments.—"All physicists," he says, "are now acquainted with my method of neutralizing the action of gravity upon a large mass of liquid, and, at the same time, leaving it free to obey molecular actions. By means of this, and afterwards of another method, I have been able to obtain liquid cylinders and to study their properties. I have thus corroborated the following facts:—1. A liquid cylinder constitutes a figure of stable equilibrium as long as the ratio between its length and its diameter does not exceed a certain limit, between 3 and 3·6.—2. Beyond this limit the cylinder constitutes a figure of unstable equilibrium, so that it cannot be obtained in a permanent state except by means of certain hindrances.—3. A liquid cylinder, whose length is very great in comparison to its diameter, converts itself, by the spontaneous rupture of equilibrium, into a series of isolated spheres, equal in diameter, equidistant, and having their centres on the line which forms the axis of the cylinder: in the intervals between these are spherules of different diameters, having their centres on the same line.—4. This transformation commences with the origination of regularly placed contractions alternating with expansions: afterwards both one and the other become more developed, the contracted parts becoming thinner, the expanded ones thicker. When the centres of the contracted parts become sufficiently thin, they do not rupture suddenly; but the liquid, receding on both sides of each of the centres towards the expanded parts, still leaves the latter for an instant connected two and two by a thread sensibly cylindrical. Lastly, these threads transform themselves in the same manner as the cylinders; and, by the rupture of threads still more attenuated, resulting from their own contractions, they leave the isolated masses which form the above spherules, whilst the larger masses proceeding from the expanded parts of the original cylinder, and which are at equal distances asunder, assume spherical figures.—5. This spontaneous alteration and this transformation, whose final result is the formation of isolated spheres with spherules arranged in the intervals, are not peculiar to cylinders: they accompany every other liquid figure of which one dimension is considerable compared to the other two.—For the sake of those who have not repeated my experiments, I will here mention, in support of the above facts, a phenomenon observed by all physicists. When along a thin wire held horizontally, an electric discharge capable of fusing but not of melting is passed, the wire becomes first heated to a white heat, and at the same time bent in consequence of its elongation: afterwards it is observed to resolve itself into a great number of separate globules, which fall, and whose form when cool is found to be rounded. Now this wire, at the moment of fusion, constitutes a liquid figure which satisfies the conditions above expressed in 5.—Now a jet of liquid issuing in any direction whatever, fulfils the same conditions: that is to say, it constitutes a liquid figure whose length is considerable compared with its transversal dimensions. It should then of necessity alter its form in order to transform itself gradually into a series of isolated spheres with interposed spherules; and the phenomenon ought to manifest itself by the formation of contractions and expansions which develop themselves more and more, until the generation and rupture of the threads take place as above described. But the liquid of the jet, having a motion of translation, which carries with it the expansions and contractions, it is during this journey that each of them completes all its progressive modifications. . . . Further, a fresh quantity of liquid being always supplied, the transformation must be incessantly repeated. Hence the continuous and discontinuous parts of the jet: hence, also, the origination of expansions and contractions scarcely perceptible near the orifice; but becoming more and more developed as they move onwards with the liquid, until the expansions arriving one after another at the extremity of the continuous part, successively detach themselves, and pursue their course as isolated masses, which assume, or tend to assume, a spherical form: hence, too, the spherules interposed between these masses; and, lastly, the laws discovered by Savart connecting the length of the continuous part, as well as the tone produced by the shock of the jet, with the change in the diameter of the orifice."—M. Plateau, however, does not deny the effect of vibrations, and even musical tones, in accelerating the formation and completion of ventral segments, especially when the expansions and contractions caused by the vibrations coincide with those originating in the molecular forces.

Hydrometeors. The whole aqueous phenomena of the Atmosphere are designated by this name. The chief specific Hydrometeors, viz., *Clouds*, *Dew*, *Fogs*, *Snow*, and especially *Rain*, are described and their causes investigated under those several titles in our dictionary, and under HYGROMETRY; so that it simply remains for us in this place to offer some account of the ground or root of such laws as have been ascertained with regard to the habitudes of the aqueous or vaporous portion of our composite Atmosphere. The rational foundations of the whole inquiry were laid by the illustrious Dalton; but we owe to the late Professor Daniell the vigorous and skilful carrying out of Dalton's general principles. It is scarcely necessary to recall to the reader the nature of Dalton's great discovery regarding the intermixture of gases. It consists simply in this, that one gas offers only a mechanical obstruction to the diffusion of another. Suppose that any porous solid substance—a sponge, for instance—were plunged amidst a certain gas, that sponge would displace a portion of the gas, but through its pores the gas would diffuse itself according to its own laws—spreading over larger space, but in nowise otherwise affected. Exactly in the same way the dry, or what may be called the permanently-elastic Atmosphere, affords only a mechanical obstruction to the Aqueous Atmosphere; *i. e.*, it constrains it to occupy a larger space—*to mount higher*—than it would do *in vacuo:* and in return, the dry Atmosphere is similarly affected by the pressure of the moist one:—each Atmosphere nevertheless being affected by its own laws of diffusion, as depending on its ratio of elasticity; and each one accordingly might be treated independently, and its entire phenomena independently evolved, were it not that their corresponding horizontal strata at different heights have wholly different relations to *Heat.* That is to say, suppose A B a vertical column of dry air, and A′ B′ a vertical column of the vaporous atmosphere, diffused through each other, they might be treated quite apart, and their phenomena independently deduced, but for the circumstance that at the same height A C, A′ C′, the strata C′ C, have not the same *specific heats*, or the *same relations to heat.* In which latter circumstance, their mutual actions and reactions arise, and give birth to the infinite varieties and complications of the problem and its results. Such being its general conditions, Mr. Daniell conducted the inquiry with signal industry and acuteness. Following the exactest rules of *Method*, he divided it into three parts, viz.: *First*, What would be the habitudes of a *permanently elastic Atmosphere*, surrounding such a globe as our earth? *Secondly*, What would be the habitudes of a purely *Vaporous Atmosphere*, surrounding such a globe? And *thirdly*, In what manner must the two Atmospheres, if intermixed, act and react on each other, in consequence of the incongruity of the relations of their corresponding strata to *heat*, and thus modify the independent or natural habitudes of both?—For the general results of the *first* portion of Mr. Daniell's investigations, we refer to our article on the WINDS, where the whole subject is minutely treated. Had our globe been a sphere of equal temperature, a dry envelope of the kind referred to would have simply remained at rest over all its surface, and presented no phenomena worthy of notice, except regularly decreasing elasticity and temperature along every vertical column. The regular diminution of Heat, however, from Equator to Pole, and the irregular variations of surface heat, depending on the constitution of the different parts of the Earth's surface, disturb this rest and give rise to interchanging currents—some comparatively uniform and universal, such as the Equatorial and Polar Currents—and others very irregular, or our ever shifting winds.—In the same manner, with regard to a *Vaporous* Atmosphere surrounding a sphere homogeneous in temperature, neither motion nor condensation could supervene. Add the element, however, of a decrease of temperature from *Tropic* to *Arctic Circle*, and we discern the necessity for an incessant evaporation, or of clear weather at the equator, and a current of vapour rushing northward and southward, condensing as it flows, and deluging the whole of the Temperate and Frozen Zones with Rain and Snow. In this case too, of course, the physical irregularities of the Earth's surface—its division into land and water, into sandy deserts, marshes, forests, and cleared and cultivated ground—come in to modify the general result, and to necessitate patches of drought and patches of intensest rain, through all its zones. These irregularities, however, only *modify* the general laws. The *third* division of Mr. Daniell's able and remarkable investigations is the difficult and complex portion of it, and takes account of the actual circumstances of the case. Its conditions will be readily understood. Suppose that at the point C′ of the foregoing diagram the heat of the stratum C is not sufficient to permit the vapour at C′ to remain in its natural or elastic condition, condensation must take place and rain fall, or clouds be formed. But this act of condensation necessarily evolves or gives out a quantity of *heat*—that which was formerly called *latent heat;* this heat communicated to the elastic stratum wholly alters its elasticity and entire condition, and new currents or winds—bringing with them multifarious changes—must instantly arise. View under this new conception the complex structure of both Atmospheres, as depending on the irregular distribution of Heat over the surface of the Earth, and it will be seen how intricate and refined is every problem in practical Meteorology. No doubt, however, can rest on the fact, that there is a *rational* mode of treating the subject, and we shall expose under RAIN and WIND the leading positive results already



reached.—It ought to be mentioned that the Dry Atmosphere exerts another very important influence with regard to the production of Hydrometeors. *It retards the flow of the vaporous currents*, and so prevents sudden and impetuous changes. As to the rate or degree of retardation, or as to the actual velocity with which Aqueous Vapour travels, much obscurity and vagueness still prevail. The element, however, is a most important one in practical Meteorology—involving, as it necessarily does, the effects of winds of various degrees of strength on the processes alike of Evaporation, and of the deposition—especially the distribution, of Rain. Under certain circumstances the *apparent* motion of a cloud—which, however, rather signifies the *diffusion of the conditions under which a cloud is formed*—is enormous, touching even on *three hundred miles per hour!* For details of the remarkable investigation, of which we have described the mere bases, the student is earnestly referred to Prof. Daniell's work on Meteorology: its leading results will be found under RAIN and WIND.

Hydrometer. An instrument of the same nature, and on the same principle, as the AREOMETER (*q. v.*).

Hydrostatic Balance. See BALANCE HYDROSTATIC.

Hydrostatics. The science which treats of the equilibrium of fluids, and of the pressures which they exercise against the walls of their containing vessels.—It is manifest that this science must be subject to all the laws of the general theory of equilibrium exhibited in ordinary statics. But there are two fundamental axioms which constitute it into a distinct branch of that science—including a peculiar class of problems, and suggesting and explaining a separate class of physical phenomena. The *first* of these axioms is, that all action between two fluid surfaces, or between a fluid and a solid surface, is *normal* to the plane in which they meet—or to the common tangent plane to the surfaces at the point considered. The problems of ordinary statics are complicated by the introduction of an indefinite force—indefinite so long as it does not exceed a certain amount fixed by the conditions—which is called *friction.* The science of hydrostatics founds on the assumption that there is no such thing as *Statical Fluid Friction.* In hydrodynamics, the same assumption is in most theoretical books habitually made—in consequence of the yet insuperable analytical difficulties presented by the problems the science would otherwise present us, where it is not supposed to hold. But the assumption that there is no tangential action when fluids are actually moving *inter se*, that there is no *dynamical fluid friction*, is certainly entirely fallacious—in the case of any and every fluid. Viscous fluids, such as treacle, for instance, offer a certain resistance to the sliding of one particle along another; but in what are called perfect fluids, such as water, if there be any such resistance, it is of infinitesimal amount, and may be disregarded in actual problems of equilibrium, where we are dealing not with actual motions, but with ineffectual tendencies to motion, which are capable of being counteracted. In the case of such fluids no such tendency actually is counteracted by friction.—Before stating the second axiom, we shall see how the *pressures* of which it treats are to be measured. If we conceive a plane, containing a unit of surface within a fluid—the pressures exerted on it by the latter consist, according to the first axiom, of a number of parallel forces, all perpendicular to the plane. If any one point of this plane be pressed exactly as much as any other, the sum of these parallel forces is the pressure *at* the centre of the plane. If all these points are not pressed by equal amounts, it is clear that we can still attach an idea to the term pressure, *at* the point we have mentioned. It is, what would be the pressure on a unit of surface, if instead of the unequal forces spoken of, forces acted at every point of it, all equal to that which acts at the point.—We have defined, then, what we mean by pressure at a point. But nothing has been said about the direction of the plane on which we considered the pressures to act. Suppose that to be vertical, horizontal, or oblique to any degree—may there not be corresponding alterations in the amount, as well as in the direction of pressure? The *second* hydrostatical axiom answers by asserting that the amount of pressure is independent of the direction of the surface pressed. So far as it is a special mathematical subject, hydrostatics is entirely deducible from these axioms,—which, by the way, are not independent of one another. But we can only exhibit in this place those less abstract illustrations of the subject, which depend least on high analysis.

There is always one force acting upon fluids whose operation must be borne in mind—that of *gravity.* Whenever, therefore, a problem in hydrostatics presents itself, we must remember that this force is present, and allow for its effect. Another essential condition is, that property of liquids which allows them readily to separate,—the particles being capable of arranging themselves in any way without the intervention of any opposing forces. We have, therefore, gravity and liquidity always as elements in problems in Hydrostatics.—A principle resulting from the quality of liquids, lies at the very foundation of the whole science, *i. e.*, that of the *equality of pressures*, *i. e.*, that liquids have the property of transferring equally in every direction any pressures applied at their surfaces.—Experimental proofs of this principle are readily supplied. Imagine, for example, the vase G, filled up to the level A B with water, or any other liquid; and suppose, for the present, that gravity is not acting upon it. The vase in that case might at once be removed, and there would

result no motion of the water. Gravitating force not soliciting it, and no other force being present, the water would remain at rest. Imagine now, however, that a piston is laid over A B having a weight of, suppose 30 lbs., then the layer of water A B, or any part of it, E, would fall, if not supported. It does not move; so that there must be an equal force acting against it. It transmits, therefore, the weight, to the layer or part immediately below. There is thus a downward weight on that part, and an upward force from it, counterbalancing one another, and equal to the weight on E, this upward one exactly counterbalancing it. This second part (E′, suppose) would fall, if not supported: so that, it must be kept up by the transmission of the force to what we might call E″, and so on, until the force is transmitted to F. For every portion similar to E, having the same weight at the top, an equal pressure is transmitted to the bottom, and thus the weight is faithfully transmitted *downwards*. But it must be transmitted also to the *sides* of the vessel, which must experience an equal pressure. This could be readily shown by boring a hole in the side of the vessel, through which the water would immediately gush. In fact, from the liquid's perfect freedom of motion, it seeks to move in one way, just as much as in another; and it is not permitted to move in any way. The whole resisting force is equally distributed over the outline of the liquid; and therefore the pressure is equal on all sides of the vessel. And as it is so, the amount of pressure on any one part, will be to that on any other part, in the ratio of the areas pressed respectively.—From this law are deduced two conditions of equilibrium—the one amounting rather to an expression, in another form, of the same law, or of the principle of liquidity: *i.e.*, 1st, that every molecule of the mass must be solicited by equal and contrary pressures in every direction—becoming, in fact, a sort of centre of forces; and that, 2d, the upper molecules of a liquid which are free, must form a surface perpendicular to the direction of the impressed force. This last proposition needs little proof. Suppose, for example, the vessel A B D F, filled with fluid submitted to the action of gravity alone, it is clear that the outline of the upper surface cannot be such a figure as C H K G E, but must become flat, that is, horizontal, and so, perpendicular to the vertical direction of gravity. Suppose, that it did actually take such a shape as C H K G E. Then a particle at K, for example, is pulled downwards by its own weight, and as all the liquid below experiences the pressure of that weight in every direction, the particle adjacent at H will feel it pushing it horizontally, in fact, outwards—that behind will take its place, and be again pushed outwards, and so on. This state, in which there is no equilibrium, will go on just as long as there is any difference of level in the outline C H K G E; and therefore, there can be equilibrium only when the level is preserved constant. It follows, that one means by which we might ascertain the direction of gravity, would be by taking it as perpendicular to that of still water. We have seen elsewhere that the direction of a plummet, by which one usually tests the direction of gravity, may vary from that which we would naturally expect, in consequence of the mass of a neighbouring mountain. (See EARTH. *Figure of*, and *Density of*). May not this *cause* be eliminated by the method of taking the vertical disturbance as perpendicular to still water? Let us remember what the direction of the plummet indicates;—it is the direction in which a small body is drawn at that spot by the attraction of the whole actual earth. And this deviates from the direction in which it would be drawn, if the actual earth were to be removed, and what we may call a normal earth substituted for it—that is, one not broken up by mountainous irregularities of surface, but homogeneous, and of uniform shape. Now the same actual earth is that obeyed by the levels of the sea and lakes. When a mountain is near a sea, the level of the sea must be deflected somewhat upward, towards that mountain; and just by the same amount as the plummet is deflected. In fact, the force of gravity is, in neither case, that of the normal earth, but the resultant of its attraction and the attraction in an upward direction, of the mountain; and the level of water must be perpendicular to that disturbed line. If the Cordilleras, for example, were a hundred times higher than they are, the seas would slope upwards along the shores of America on both sides, and the ports of France and Western Britain, as well as those of Japan and China would be drained. The fact, however, that this result is actually found in nature, to a quite measurable extent, proves the truth of the principle, that the free surface of fluids is perpendicular to the force that solicits them.—The phenomena of capillarity depend on the same principles. We have there, the force of gravity tending to produce a *horizontal* level. But there is also a certain attraction between the particles of the fluid, and those of the containing vessel—and a further attraction—resulting from the fact, that no body is perfectly liquid, between the particles of the fluid itself. The force which the free surface of the fluid obeys is the resultant of this set of three forces; and that resultant for

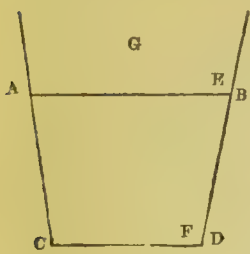


Fig. 1.

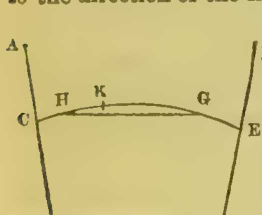


Fig. 2.

each point of the free surface, has certainly a direction perpendicular or normal to the surface. —We have thus established the conditions of equilibrium of a fluid. We shall now investigate the pressures which act upon the walls, and which they must be capable of resisting. This is also fruitful of practical applications. Our *first* proposition is, that the vertical pressure which a liquid exerts against the bottom of its containing vase, is entirely independent of the form of that vase, and is equal in weight to the solid content of a figure on the same bottom, and with the same height of level. Take a number of tubes, each having the narrow

Fig. 3.

arm on the right hand of the same dimensions, but with entirely different forms for the left one. If the right arm tube be filled with water to any uniform height in them all, it will be found that the left arm tubes appear filled to exactly the same level. More water has been necessary to accomplish the task in the one than in the other, but the level in each of the two arms is the same. The meaning of which is, that the pressures upon a layer, which we may suppose to bound them, is the same. The right arm, with its uniform bore, rests a uniform weight of water upon this layer,—producing a definite pressure. It is kept in equilibrium, however, by the pressure of the unequal masses in the other arms. Hence, the pressure does not at all depend on the *shape* of the containing vessel. From a similar experiment, we may draw our other conclusion. In a tube, of vertical axis and horizontal base, the pressure on the base is evidently exactly the weight of the contents of the tube. Every particle drawn downward by gravity, if permitted to fall freely, would strike the bottom, and the force of gravity is therefore transmitted entire to the bottom. This weight is measured by the product of the area of the base by the height of the level. But, as we have just seen, the pressure so exercised is the same as in the case of the base, with the same height of level, in a tube of any form. Suppose the vertical tube, in fact, to be that in the second figure given. The pressure of the water in the other vessel is exactly the same, and would be so, whatever the shape of that tube.—What is called the *hydrostatical paradox* depends on this same principle. Let C D and A B be two horizontal boards, bound together by

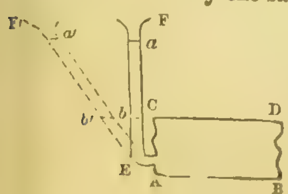


Fig. 4.

leather sides, and with a funnel, *a b*, either vertical or oblique. Let water be poured into this, until the bellows-like vessel is full. If a weight be now laid upon C D, the water will rise to *a*, and the weight of the vertical column, *a b*, or of the oblique column, *a′ b′* × cos. of obliquity, will apparently balance the weight. It will be readily found, however, by experiment, that the weight on C D will be the weight of a column, which would have a base as broad as A B, instead of the narrow funnel of the vessel, and a height *a b*. Thus, a few ounces of water may be made capable of balancing many lbs. of solid weight. The close analogy between this and what is called the Hydraulic Press (*q.v.*) will immediately suggest itself. — This pressure, however, is not only exerted against the bottom, but also against the sides of the vessel, and upon all the points of the interior of the mass. The latter point we especially desire to establish. Thus, let A B E F be a vase filled with water. Consider a portion, *m n*, of the layer, C D. Horizontally, it must meet equal pressures; with these, however, we have not to do. Vertically, it is evident it must meet on the two sides with equal pressures, otherwise it would move in either one or another direction. The downward pressure is due to the whole mass of water remaining above it. The vertical upward pressure must, therefore, be equal to that. The mass, *m n*, therefore, is in equilibrium, under the counteracting influences of two equal pressures. This would be very strikingly illustrated if a foreign solid body, *p m n o*, be introduced into the fluid. The downward weight on the layer *m n*, from the upper water is now removed—at least, ostensibly—and the upward force is brought into full play. If there be a smaller downward force to counteract this, it is evident that the body will be shoved up. Let us examine the downward forces which counteract this. First, the weight of the mass of solid *p m n o*, and next the weight of the mass of water *r s p o*. Hence, on the one hand, a weight equivalent to that of a mass of water containable in *r s m n*, acting upward, and two equivalent to the water in *r s p o*, and the solid matter in *p m n o*, acting downwards. As these forces are contrary to one another in their line of action; the force ultimately upward is represented by their difference, *i.e.* (*r m n s* — *r p o s*) of water (or other fluid) — *p m n o* (of solid), or by *p m n o* of the fluid — *p m n o* of the solid. The solid is pressed downwards with a force equivalent to its own weight diminished by the weight of an equal bulk of water. If, then, the equal bulk of water be heavier than the solid, it will be pushed up—if lighter, it will sink down—if equal, it will re-

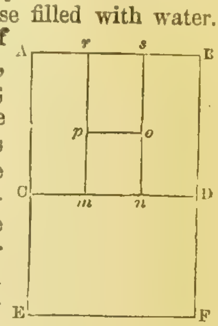


Fig. 5.

main in any position without moving. Such a solid, however, generally *soaks*,—*i.e.*, its pores, which contained only air before, become filled with a heavier material, and so the whole solid becomes heavier than the equal bulk of water, and sinks. The story of Archimedes' detection of the deceitful goldsmith of king Hiero, is a well known illustration. The *Hydrostatic Balance* (see BALANCE) is another.—When a solid floats in a fluid, it displaces a certain proportion of its own bulk of water. The weight of the body is then balanced by a bulk of water equal to that of the immersed portions of the solid. The centre of gravity of the solid—in a line through which its weight acts—and that of the displaced bulk of water—in a line through which the upward force acts, must be in the same vertical line, should there be a balance of forces, or an equilibrium. From this consideration, three states of equilibrium exactly like those described in the article STABILITY, take place—an equilibrium *stable;* when the centre of gravity of the solid is below that of the displaced fluid, in which case, on disturbance, the body oscillates until it finally settles in its old position, like a pendulum set in motion: an equilibrium *unstable,* when the centre of gravity of the body is above that of the displaced fluid, and the body, therefore, on disturbance, topples over, as a stick balanced on the finger does,—not oscillating back into its original position: and an equilibrium *indifferent,* where the body disturbed refuses to rest in any position in which it may be placed, like a body rotating around its centre of gravity—or a sphere, whose centres of gravity and suspension being the same, may rest on any point of its spherical circumference.—A good practical illustration of the theory now stated, is found in the circumstance, that the keel of a vessel and the bottom of it must be made so much the thicker and stronger, the deeper the vessel sinks in the water. Suppose that a vessel sinks 16½ feet in the water when loaded. Its keel and the whole bottom of the ship must be capable of resisting a pressure of 7½ lbs. per square inch; *i.e.* the weight of a cylinder of water 16½ feet and a square inch, in section. If the vessel sunk only 2¾ feet, the sea would press on it with a force of 1¼ lbs. per square inch, and so on. Hence, if a leak should spring, in a vessel of either kind, a weight of 7½ lbs. per square inch in the one case, and of 1¼ in the other, would be required to keep a plank, hastily thrown across, from being driven up.—We may hence gather what enormous compression the bodies of sea animals or plants—or of the chemical substances held in solution, or of the rocks, far down in the sea, must undergo. All round them, at a depth of 33 feet, there is a pressure, crushing inwards, of 15 lbs. per square inch (see DIVING BELL), and this pressure increases exactly as the descent increases. We can readily conceive how impossible it would be for many of these animals, or plants, to live near the surface of the sea; just as it would be impossible for us to live there—from the unnatural expansion or contraction of the material frame. Exactly, therefore, as along the mountain ridges there are regions of flowers and shrubs, within which alone certain vegetable productions are found; and as certain strips of earth, sharply enough outlined, seem the necessary limits of certain races of animals and men; so, unquestionably, must there be in the sea many totally different kingdoms of animals, each with their corresponding plants and minerals, most of which may visit one another at slight and rare occasions, but generally remain as completely isolated as if they were dwellers in another world.—Our next proposition refers to the lateral pressures which vessels containing fluids are subjected. This pressure sustained by the side of a vase is equal to the height of a column of liquid having that base, and having a height (perpendicular to that base) equal to that of the centre of gravity of the side from the level. This rests on the principle of the equality of pressure. We have seen that each point of the layer C D, such as *m*, sustains the load of the superincumbent weight *m r* (see the last figure); which being transmitted in every direction, is transmitted also to the particles at C, in a direction vertical to the side A E (that is, the direction of the complete pressure sustained by the side of the vase). The pressure at C, then, depends on the extent of the little space covered by the pressing molecules; or on the height of the molecules and the density of the fluid. Abstracting this last, (which, as it will enter into the consideration of the pressure on every point such as C, we may replace, once for all, at the end), we have the pressure at each very small space C, depending on the space and the height. All the pressures, being alike perpendicular to the side, are in parallel directions. Hence, we have a number of parallel forces acting at distances along A E, proportional to the forces themselves. The united action, therefore, is equal to that of a weight acting at some point along the side A E, and equal to the weight of a mass of fluid of bulk equal to the product of the side A E, by the height of its centre of gravity from the level.—We thus have the value of the pressures on all the sides of a vase containing water. We next want to discover the points where these pressures could be resisted. Wherever we have, as here, a number of parallel forces for their united action, one force parallel to them, and equal to their sum, may be substituted. We have found the value of this force, it is required now to find its line of direction, and point of application. This is of importance; because, if a rigid surface, subject to such various pressures, have applied to it one force equal and opposite to this, which we may call their resultant, there would be no motion in the surface. Of the existence of this, one may easily convince himself. Suppose that a part of

a plank on the side of a vessel, beneath the water, is somehow torn up, and that water is gushing through. Suppose a plank pushed across the aperture, and that no means of fastening this can be got but by holding a stick, or a bar of iron, right against it for some time. How is one to hold it? In the first place, quite perpendicular to the surface of the plank. But he may put that stick on many parts of the plank, and not accomplish the end. The water will turn round the plank, unless he get it in the proper place. But there *is*, evidently, a proper place, where, if he put the stick, the pressures on one side will balance those on the other all round, and equilibrium be preserved. If it slip momentarily from the perpendicular, however, or go from the point so fixed, the water will rush in. This point is called the *centre of pressure.*—Its position is evident in the horizontal bottom of a vase, or at the keel of a ship. There all the pressures round about are equal, and we will have simply to press vertically upwards or downwards, with a force equal to the weight of a column, having the moveable base for its base, and for its height its distance from the free level of the surrounding fluid, at the centre of this moveable base. But on the side of the vase, and on the side of a ship the pressures are not equal at the different points. They are proportional at the different points, to the different vertical distances of these points from the surrounding level. Common sense will teach us here something of the result. It would evidently be needful to put the stick lower down than the middle of the moveable plank in the ship, because stronger turning forces are exerted at the lower than at the upper part, and they must, therefore, be brought to act at a less leverage than they. So in the vase, the centre of pressure will be nearer the bottom than the top, for the reason of the inequality of the adjacent pressures. The problem of finding the metacentre depends on the use of the integral calculus. Where the side of the vase has the form of a parallelogram, it is on the line bisecting the horizontal sides, and one third of its length from the bottom. In a pyramidal, or conical vase, it is one fourth of the length of the side from the bottom, and so on.—The term vase, which has been employed throughout this article, applies to any sort of vessel in which water may be held. If two or three vessels are connected by pipes, they are looked on as being, in fact, the same vessel, and are subject to laws just as if they were. Hence, everywhere, in the water pipes throughout a town, the water seeks to rise to its original level, in accordance with the principles just explained, and presses against obstacles, with force corresponding to the difference between the actual and the possible level. Hence also the great force with which the water in the streets—the lowest position—which in towns is generally supplied from these sources, gushes up and out. That on the tops of high houses, again, or of houses on elevated ground, does not flow, except quietly out of the pipes; and, unless the highest situation has been chosen for the reservoir of water, those houses which are higher than it will not be supplied by this principle of gravitation. If they are to be supplied at all, it must be by the expenditure of enormous artificial pressures.—In the case of vessels communicating, containing fluids which do not intermingle, it is not at all needful that the same level should be preserved. In fact, unless there be the same density in the two fluids, it will not. Thus, in the vase A B C D E, there is water and mercury. The fluids touch at B C, suppose. Then, in order to equilibrium, there must be equal pressures of water and of mercury. The pressure of water is the weight of that contained in A. That of mercury is that of a column of base B C, and height the difference of levels of B C and E; and if D be on the same level as B C, the pressure of mercury is that of a column with B C for its base and D E for its height. Hence, the bulk of the two actively pressing masses, are as the heights B G and D E; and as their weights must be equal, those heights must be inversely as the densities. The height of two communicating, but not intermingling fluids, above their level of contact, must, therefore, be in inverse proportion of their densities.—Such the general principles on which *Hydrostatics* at present rests. Their applications to practical engineering are abundant; but as already frequently indicated, it does not come within the scope of the present work to specify these.

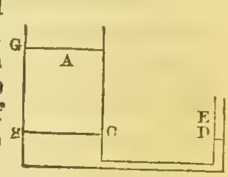


Fig. 6.

Hydrus. (The Water-Snake.) A southern constellation, situate between the south pole and Achernaz, a bright star in Eridanus. The stars α, β, and γ Hydri are each of the third magnitude.

Hyetograph. A graphic representation of the average distribution of *Rain* over the surface of the Earth. See RAIN.

Hyetometer. See PLUVIOMETER.

Hygeia. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Hygrometer. One of the most important instruments required by the Meteorologist;—its object being, as its name imports, to detect the dryness or moisture of the Atmosphere. All theoretical considerations concerning Hygrometry being consigned to that article, the construction and character of the requisite instruments shall here be alone described. Hygrometers are of three classes.—(1.) The first class—being mere *Hygroscopes*, or *indicators*, not *measurers* of atmospheric moisture—need not, in the present state of science, long occupy us. Of *Hygroscopes*, Saussure's is the best, and it has been very ex-

tensively used. It depends on this, that a perfectly dry hair, and the same hair saturated with wet, have quite different lengths—the dry hair being the shortest. Intermediate lengths are taken as roughly indicating proportional humidities; and these are marked on a dial, connected with the hair by much the same device that one finds in *dial-barometers.*—(2.) The second class consists of true Hygrometers, and their object is to determine the *Dew-point* by direct experiment. The *dew-point* is this—the amount or number of degrees, by which the temperature must at any time be lowered, so that the surrounding atmosphere become inclined to deposit moisture: if this amount be small, the inference is, that the atmosphere is nearly charged with moisture,—if large, that the atmospheric condition is comparatively a dry one.—There are three good Hygrometers, adapted to the immediate determination of the dew-point in this way. *First.* DANIELL'S. This instrument consists of two glass balls, communicating with each other through a bent glass tube. The ball A, is of black glass, about one and a quarter inch in diameter; the ball D is of the same size, but transparent. A small mercurial thermometer is fixed in the limb A B, with a pyriform bulb, which descends to the centre of the blackened ball. A portion of sulphuric ether, sufficient to fill three-fourths of the ball A, is introduced, and—the atmospheric air having been expelled by boiling as completely as possible—the whole is hermetically sealed at E. The ball D is covered with muslin, and the apparatus is supported on a brass stand, F G, to which is fixed another delicate thermometer, whose readings show the air-temperature. To ascertain the temperature of the dew-point, the ether is all brought into the ball A, by inclining the tube; the temperature of the air is then registered, and ether is poured from a dropping tube, which fits the mouth of a small phial, on the muslin cover, D: the cold produced by the evaporation then causes a condensation of the vapour of ether which fills the connecting tube and the ball, D E, and produces a rapid evaporation from the ether in A; which being a cooling process, the temperature shown by the enclosed thermometer rapidly sinks. The instant that the ether within the black ball is cooled down to the temperature of the dew-point, a film of condensed vapour from the air surrounds the ball like a ring at the level of the surface of the ether within A: if the thermometer be read off at the instant this ring of dew is formed, we obtain nearly the true temperature of the dew-point; that of the air at the time being shown by the exterior thermometer, F G.—The ring of dew will gradually lessen as the ether within the ball is recovering its original temperature; and will

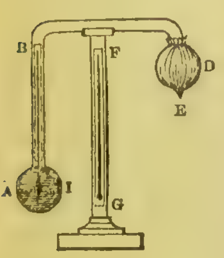


Fig. 1.

finally disappear. At that instant the enclosed thermometer should be again read off, and this reading will give another approximation to the dew-point temperature; the mean of this and the former determination may be supposed to approach very near the truth.—The portability of this beautiful instrument is a great recommendation; the whole apparatus is packed in a box, which may be readily carried in the pocket. An important deficiency is the smallness of the enclosed thermometer, which will not admit of a reading by estimation of nearer than half a degree; other drawbacks to its use are the difficulty of obtaining pure ether for the experiment, and of catching the instant of the formation and disappearance of the ring of dew,—seeing that the thermometer has to be watched and read off at the very moment that the dew appears or vanishes.—*Secondly.* An instrument, essentially the same with Daniell's, has been recently invented by Regnault; and it must be allowed that Regnault's is not subject to the objections in use, that as every experimenter feels, stand out strongly against the former instrument. Regnault's Hygrometer consists of a glass tube, over which is slipped a thimble, A B C, made of silver, very thin and highly polished, 1·8 inch in depth and 0·8 inch in diameter; it is fitted tightly on the glass tube, C D, which is open at both ends.—The tube leads by a small aperture into the hollow upright support, N M. The upper opening of the tube is closed by a perforated cork (sometimes of India-rubber), through which passes a very sensitive thermometer, T, which, being much longer than any that can be used with Daniell's hygrometer, is more nicely and readily read off: its bulb descends nearly to the bottom of the silver thimble; and, from the same depth, rises a hollow, thin glass tube, G, which also passes through the cork. Ether is poured into the tube as high as P Q; the pipe, D, leads down the hollow upright which is in communication with the flexible tube O, to an aspirator (not shown in the drawing), that is, a jar containing something less than a gallon of water; this jar is closed, except a small aperture, over which the flexible tube is made to fit air-tight; at the bottom of the jar is a stop-cock, on turning which the water will run out. The aspirator-jar is near the observer, but the instrument may be at any convenient distance, so as to be removed from the heat of the person.—When the

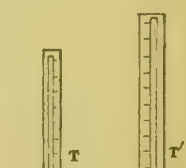


Fig. 2.

dew-point is to be ascertained, water is allowed to run from the aspirator-jar; this will produce a current of air through the fine tube, whose mouth is at G, and the pipe of communication, M N; to reach the space above the ether, the air must pass, bubble by bubble, through it, which will produce an uniform temperature throughout the whole mass while it is subject to the agitation produced by the rapid passage of air; and the silver thimble will be of the same temperature as the ether within, the degree of which will be shown by the immersed thermometer, T. If very great nicety is required, the thermometer may be read off by a small telescope, but, with care, this is seldom necessary.—At the moment that the ether is cooled down to the dew-point temperature, the whole external surface of the silver thimble becomes covered with a coating of moisture; and the degree shown by the thermometer, at that instant of time, must be marked. Let us suppose the first reading to be 46°; it is probable that, as a fraction of a second was lost as the eye glanced from the silver to the thermometer, this reading is too low. In a few moments the dew disappears and the thermometer rises; but now its reading is probably too great, say 48°. The stop-cock of the aspirator is then gently opened, and a small stream of air-bubbles rises through the ether, and, by nice adjustment, the thermometer is observed steadily to read 47°·3 at the instant that the dew is formed: repeated trials are sometimes necessary, but five minutes will generally suffice for the whole operation. The inventor found three or four sufficient to determine the dew-point to the tenth of a degree.—The second tube, A′ B′, holds a second thermometer, T′, circumstanced in every way like that used in determining the dew-point, and its reading gives the temperature of the air at the time of observation. This additional thermometer need not necessarily be mounted in the same manner as the dew-point thermometer, as the temperature of the air may be learnt from one suspended in the usual way.—The direct method by which the dew-point may be ascertained by marking the clouding of the polished silver surface, is so great a recommendation, that nothing but cheapness is requisite to bring Regnault's hygrometer into general use. Messrs. Negretti and Zambra have constructed the instrument in a simpler form, at a price not greater than is paid for Daniell's.—The aspirator need not be a vessel of water: at Kew two circular boards are united by leathern sides, after the manner of a pair of bellows: to the bottom board is attached a weight; and in the top board is a projection to which the india-rubber tube is attached: when an observation is to be taken, the boards are made to approximate by a pulley, and as the lower one is allowed to descend, a draught of air is produced, the force of which may be regulated at pleasure. This apparatus does away with the trouble of constantly supplying water to the aspirator-jar, as is necessary in the usual construction; though some aspirators are made double, so that they may be reversed when the water has run out of one compartment into the other, and thus the same quantity of water may be made serviceable for any length of time.—The *third* instrument of this class, we shall mention, is due to Professor Connell of the University of St. Andrews. It is represented below (fig. 3) —A is a small bottle made of highly-polished brass or silver: it is partially filled with ether, and the temperature is lowered by means of an exhausting syringe, D E, which causes the ether to evaporate rapidly: a thermometer, T, whose bulb dips into the ether, shows the temperature of the dew-point at the instant that the polished exterior of the bottle becomes clouded with moisture. G is a clamp to fix the instrument to a steady support.—

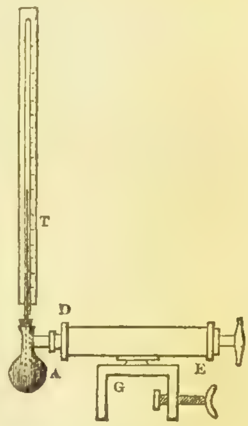


Fig. 3.

(3.) To the foregoing second class of Hygrometers, there is one essential practical objection. Each determination of atmospheric moisture, by these instruments, is of the nature of an *experiment*, not of an *observation*; and the magnitude of this objection may be guessed, should one suppose it necessary to perform an experiment, instead of merely reading a scale, previous to determining the thermometric condition of the air. The conversion of *experiment* into *observation* is the object, and the important object of the class of instruments about to be described. So successful are they, that every other kind of hygrometers must now be regarded only as instruments of convenience or verification. The dry and wet-bulb thermometers of Mason, or the *Psychrometer* of August, is represented in fig. 4; and its principles are as follows:—Under general circumstances, or rather, whenever it is not saturated, the atmosphere will take up the vapour of water: the drier it is, the more rapidly will evaporation proceed; and this the more slowly as its condition approaches that of complete saturation. When in such a state, no more moisture will rise in the air. Now, as evaporation proceeds, heat is absorbed by the

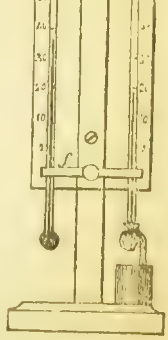


Fig. 4.

conversion of the water around the wet-bulb into vapour, and the mercury in the wet-bulb thermometer will fall a greater or less number of degrees below the air temperature, according to the dryness of the atmosphere. When the air is saturated, the readings will be the same.—Evaporation from the wet-bulb will proceed even when the temperature is below freezing; but in this case the readings must be taken with great care, and the differences will always be small.—The reading of the wet-bulb thermometer gives the temperature of evaporation: and the important problem to be solved, is to deduce the dew-point from this record, combined with the temperature of the air as indicated by the dry-bulb.—The scientific problem now enunciated has received several solutions. Until quite recently, that of Dr. Apjohn, of Dublin, had general acceptance. It is this:—

Let f = tension of aqueous vapour at the dew-point temperature which we desire to know.

f' = the tension of vapour at the temperature of evaporation, as shown by the wet-bulb thermometer.

a = the specific heat of air.

e = the latent heat of aqueous vapour.

$(t - t')$ or d = the difference between the reading of the dry-bulb thermometer and that of the wet.

p = the pressure of the air in inches: then Apjohn's formula is,

$$f = f' - \frac{48\,a\,(t - t')}{e} \times \frac{p - f'}{30};$$

or with the co-efficient,

$$f = f' - \cdot 01147\,(t - t') \times \frac{p - f'}{30}.$$

Since the year 1834 and 1835, during which Dr. Apjohn produced his *Memoirs;* and, indeed, very lately, M. Regnault (see VAPOURS) has laboriously, and with unprecedented accuracy, investigated the entire habitudes of vapours. Modifications—apparently the final ones—have accordingly been imposed on the foregoing formula. At present, at all events, this scientific problem has the following as its best solution: viz.,

$$n = f' - \frac{0{\cdot}429\,(t - t')}{610 - t'} \cdot h$$

where n is the elastic force of the vapour actually in the air; t, the temperature of the air; and t', that of evaporation in *centigrade* degrees. M. Regnault has pursued his researches farther; he investigates the effect of the wind; and the result is, that the foregoing formula will quite apply in all cases where the wind is not very strong.—Many practical difficulties are still in the way of accurate hygrometrical observation:—for instance, every practical observer knows that he can trust to no existing mechanical contrivances which aim at keeping the *wet-bulb* always moist. Nevertheless, certain important general laws, or approximations to such, have been determined:—these will be found in next article.

Hygrometry. That peculiar constitution of the Atmosphere, which gives rise to phenomena connected with Moisture, in its various forms, has been explained under HYDROMETEORS. The specific object of Hygrometry is to ascertain, by suitable instruments, (*Hygrometers*), the elastic force of the vaporous Atmosphere under various conditions, and to determine the laws of the variations of this force. In this article we shall give a brief account of the conclusions already reached.

(1.) *Diurnal Variation of the Elastic Force of Vapour in the Air.*—The following facts or laws may be taken as nearly ascertained. In the morning, before sun-rise, the absolute quantity of vapour in the air reaches its *minimum;* at the same time the *humidity* of the air attains its highest point. As the sun ascends, evaporation quickens; the tension of the vapour augments; it reaches its maximum with the temperature; at the hottest hour the humidity of the air is the least.—(It is right to define that by the rather vague term, *humidity*, Meteorologists mean the ratio of the vapour actually in the air to the amount which the atmosphere, at a certain temperature, could contain).—In winter there is but one *maximum* and *minimum* during the day; but it appears that, in summer, there are two *maxima* and *minima*—the one following the course of temperature, the other maximum occurring before noon, and the minimum between that and the hour of highest temperature. These irregularities and apparent anomalies, are doubtless owing to the action of ascending atmospheric currents, which carry up the vapours and diffuse them. Depositions take place in these higher regions, assuming the form of clouds; and the pressure of the vaporous atmosphere is thus lightened. We wait for that steady observation of a few years, which can alone be furnished by photographic instruments, to clear difficulties away on this series of questions.

(2.) *Monthly Variations.*—These follow very nearly the progress of temperature. In our latitudes, the minimum of vaporous tension occurs in January—the maximum in July. The *minimum* of *humidity*, on the contrary, is in August, and the *maximum* in December. Science urgently demands that observations be accumulated, in different climates, and even in all zones of the earth.

(3.) *Vapours at different Heights.*—The rapid descent of temperature as we ascend in the atmosphere, is necessarily accompanied by a rapid diminution also of the tension of the vapour contained in it; but the interesting question arises,

whether the *humidity* of the air also diminishes —that is, whether the fraction $\frac{t}{T}$ (t being the tension or elastic force of the vapour, and T that of the air), diminishes, or otherwise varies greatly? The best existing determinations of this ratio are by Saussure, at elevations among the Alps; by Kæmtz, also among the Alps; and by Gay Lussac, during his celebrated balloon ascent. Saussure was inclined to think that the element of humidity, (or the foregoing ratio), actually decreases along with the positive amount of vapour: but his conclusions are not supported by these subsequent observers. Kæmtz contends very strongly that the ratio of humidity remains constant at all heights; and it appears from Gay Lussac's observations, that while that ratio diminished rather irregularly to the height of 3,000 French *metres*, it maintained itself invariable from that elevation up to the extreme height to which he ascended, viz., 7,000 *metres.* It may on the whole, then, be accepted as most probable, that the humidity of the various beds of the atmosphere, varies exceedingly little in consequence of their elevation.

(4.) *Humidity of the different Winds.*—We have seen that the humidity of the air varies with the hours of the day: but just as in the case of the *barometer* and the *thermometer*, (*q.v.*), this regular march of phenomena, is disturbed by the different *Winds.* Each wind, indeed, appears, in every country and locality, to have, as proper to it, a certain condition of humidity and dryness,—depending on relations between the regions from which it blows, and those at which it arrives. See WINDS. The mean humidity of any day or hour cannot thus be correctly ascertained unless account is taken of the mean state of the wind. Observations on this interesting subject are still by far too few; but the researches of Kæmtz, and a few other Observers, justify the following general conclusions in respect of Western Europe. In winter the *east* wind is humid, and dry in summer; while the reverse is the case with the *west* wind. This inversion holds, apparently because the west wind in winter is warmer than all other winds,—a character belonging to the east wind in summer. But, for example, if in winter, after the west wind has for some time prevailed, a more humid and cold wind arises from the east or north-east, the sky immediately clouds over, and part of the vapour is condensed into rain, or dense weather,—the barometer all the while continuing high. The reverse of course, holds, when the opposite change takes place. In summer, on the other hand, both sets of phenomena are necessarily reversed.—Such are the leading facts already ascertained in Hygrometry. The reader is referred to Kæmtz's work on *Meteorology*, to our own articles, HYDROMETEOR and RAIN; and to some admirable dissertations in the French *Annuaire Meteorologique.*

Hygroscope. All substances which absorb the humidity of the atmosphere, and so change form, weight, volume, or any physical attribute, are called Hygroscopes. They indicate moisture. It is only when the change bears some relation to the amount of moisture imbibed, that they answer hygrometric purposes. See HYGROMETER. Bodies with this property, such as hair, cords, chloride of calcium, potash, &c., are said to be *hygrometric* or *hygroscopic.*

Hyperbola. A curve which is the *locus* of the point to which two lines being drawn from two fixed points, their difference shall be equal to a constant quantity. It is one of the conic sections. It has two branches equal and similar. It has four asymptotes passing through the centre, or point of bisection of the line between the two original points. Taking this line as axis of x, and that perpendicular to it through the centre as axis of y, the equation of the hyperbola is,—

$$\frac{x^2}{a^2} - \frac{y^2}{b^2} = 1.$$

Hypothesis. A logical instrument of research, of consummate importance. There are few Inquirers, save those of the lowest order, who are not inspired with the conception of some theory, or the hope of obtaining a certain kind of result, when their inquiries are undertaken. This hope or conception "shapes their ends;" and this hope or conception is the true logical *hypothesis.* Success or failure—complete or partial—depends of course on the breadth and fitness of the *analogies* which have induced and which sustain such hopes: but no Inquirer deserves his name who will refuse to sacrifice the most sanguine expectation, on the instant that it is contravened by unquestionable fact.—This latter remark leads to the question, are there not *illegitimate* as well as *legitimate* hypotheses? Their trial being by the test of facts, can any Hypotheses be legitimate, which in their very nature are incapable of being verified or disproved by facts? And if our reply must be in the negative, what becomes of those hypotheses regarding invisible, intangible, undiscoverable *ethers*, by which and their supposed undiscernible *undulations*, the phenomena of physical optics are still attempted to be explained! Ethers and imponderable Fluids have now given place, in the Science of Electricity, to the rational consideration of POLAR FORCES. We are fast losing faith in similar artifices, in our Theories of Heat. May it not, by all this, be foreshadowed, that before many years shall have elapsed, the doctrine of Undulations shall—like the old doctrine of Emission—come to be treated merely as a "rude and beggarly element," which, in its day and generation, stimulated men to *think*, and so did good service?

Hypsometry. A term applied to the process by which *Heights* are measured by aid of indications of the *Barometer.* Sufficient has been said on the subject under ALTITUDES.

Ice. When water is brought to a certain temperature it solidifies, with expansion. Solidified water is called ice. See FREEZING.

Ignis Fatuus. See METEOR.

Ignition. The art of kindling a body. Usually by applying heat to it, sometimes by chemical processes. See HEAT.

Images, Electrical. A very curious subject, connected with the theory of the distribution of Electricity. The effect of a body electrified in any way, upon an uninsulated sphere, is completely represented by "the image of the electrified body in the sphere." When, again, an electrified body is placed in the neighbourhood of two uninsulated spheres, an inductive effect is produced that may be represented by an infinite series of "successive images" in each sphere. When a single conductor, bounded by segments of two spherical surfaces, cutting at an angle which is a submultiple of two right angles, is electrified by the influence of a charged body, the effect may be represented by a finite number of "images" disposed in a symmetrical manner in the circumference of a circle passing through the exciting or inducing body, and cutting the two spherical surfaces at right angles. The phenomenon quite resembles the production of symmetrically arranged images in a kaleidoscope.—For the development of this curious subject, we are indebted to Professor William Thomson of Glasgow University. See his remarkable papers.

Images, Electrographic. The phenomena now referred to, were discovered by Riess. Place a sheet of *glass* between two points that connect the poles of a battery, and there will be found proceeding towards its edges clear indications of disintegration. These lines or marks are *conductors;* but the most singular thing is this,—other parts of the surface of the glass have become conductors, without having seemed to undergo any external modification. If the surface of the glass be breathed on, these points become apparent, and are found arranged symmetrically. The same thing occurs with the surfaces of other substances: the figures are finest in the case of *mica.* The process by which they are produced is evidently a disintegrating one,—to which may probably be referred those remarkable mechanical effects of Electricity first noticed by the late excellent Mr. Crosse of Fyne Court.

Images, Photographic. Amply discussed under several appropriate articles.

Images, Thermographic or Moser's. An extremely interesting series of results whose fixed bearing is not yet determined. They are connected evidently with the chemical action, as it is termed, of rays of light, even although they occur under circumstances that appear to exclude Light. Detailed description in this dictionary is impossible, so that we must refer to the memoirs of Moser, to the account of them by Arago, to the very original investigations by Dr. Draper of New York, and to the subsequent researches of M. Knorr. A full digest may be seen in the *Repertoire d'Optique Moderne*, by the acute and industrious Abbé Moigno. The class of facts may be defined by the following rather startling proposition. *If any two bodies be placed sufficiently near each other, and face to face, they tend to impress their images on each other.* And Knorr has shown that the effectiveness or celerity with which they do this is augmented by increase of Temperature. The full discussion of this curious subject would demand a separate treatise.

Imaginaries. Imaginary quantities in Algebra, are expressions involving the symbol $\sqrt{-1}$; a symbol having no assignable arithmetical or numerical meaning, and therefore termed an *Imaginary.* It is often thrown up in the course of processes of *Arithmetical Algebra;* but this is not accounted illegitimate, since—if the problem being resolved be not an impossible or contradictory one—it disappears in the course of the operation, and in nowise affects the result. Should such an expression appear in the result, the inference drawn from it is, that the Elements of the problem are self-contradictory or incompatible. Or, if, in the case of a numerical equation—say of the *fourth* degree—two roots turn out imaginaries, the inference is, that the problem is such an one that *two* numbers only—not *four* as usual with such equations—can be found to answer its requirements.—Taken as an *abstract symbol*, however, the expression $\sqrt{-1}$, has quite a different importance. Viewed in this light it is a most important *positive* symbol—indicating *position* in geometry. We shall discuss the entire subject under QUATERNIONS. The student is also referred to De Morgan's *Trigonometry.*

Impact. When one body strikes against another, the question as to the velocity it will assume, and the direction along which it will proceed,—in fact, the whole resulting circumstances—constitute a problem of simple dynamics, to which is applied the name, of the problem of *Impact.*—In the general case of two bodies, each moveable, striking against one another, a motion is communicated, independent of its direct motion, in some straight line. *Rotation* is usually imparted to each body. This part of the problem —viz., the determination of the nature and amount of the rotation—we shall not concern ourselves with here.—If a body strike against an immovable obstacle, with its centre of gravity perpendicular to that obstacle, it ought—on the supposition that each body is perfectly rigid, and incapable of any alteration in its form—

to have its motion completely destroyed. This supposition, however, though frequently useful to keep out of sight for a time a disturbing element n the problem, is not quite true. The body moves back with a certain portion of its original velocity, which varies with its own nature and hat of the obstacle, and which admits of experimental determination in each case. This velocity bears the proportion e : 1, to the original velocity, being a fraction, called the co-efficient of elasicity. The cause of the non-destruction of motion is this,—the impinging body and the matter of the obstacle are both compressed at the oint of contact by the impact, up to a certain point; the original velocity is then entirely detroyed, but the compressed matter in each prings out, as much as may be, into its former limensions; in which act of springing there are volved two forces, opposite in direction, producng motion in the only way possible—a retireaent of the body originally impinging, and also, oubtless, to some extent a vibration of comressed and expanded matter for some time in he obstacle. If the effort which the bodies make o regain their original position, were quite equal o the forces of compression, the bodies would be erfectly elastic, and e would become = l, *i.e.*, he impinging body would move back with the ame velocity as it had advanced. If, on the ther hand, they made no effort at all, e would ecome nothing; and the impinging body, as well s that struck, would remain, so far as the forces f impact go, constantly at rest, in their comressed form. Neither of these results, in all kelihood, ever occurs in practice. What, then, ecomes of the mechanical value of the motion, hich is partly lost? Is it contrary to the genral law, that any mechanical effort can be lost out f the universe, or annihilated; and, if not, what ecomes of it? It goes to *heat* the bodies which re struck together. Suppose a bullet fired from cannon against an iron target, it will be very ot, if it is lifted immediately after its fall, nd sometimes the heat is powerful enough to use off some drops of the lead during its fall.—

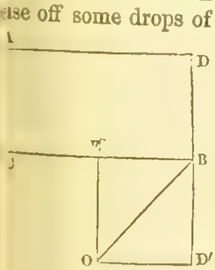


Fig. 1.

If a body strike against an obstacle obliquely, as in the figure, the circumstances will be changed. There will be a rotating effect, which we do not take into account now. Suppose the body to move up to B with a velocity which may be represented by A B, and strike there gainst the wall, B D. Then, it could be represented y a velocity, A D and A C (FORCES, *Composition*), and A D (neglecting the turning effort), by its qual, C B. Now, the velocity, A C, is not at all fected by the impact, because it, in truth, would ever produce it, being in a line parallel to B D. he velocity, B C, however, is so, and the body would be flung back from the board, B D, with a speed represented by B C $\times$ e, or B E (suppose). There remains the velocity equivalent to B D (B D′) not changed by the impact, and therefore the body will move under the influence of the two B E and B D′, in a line B O, with a velocity which it represents. B O is the diagonal of the parallelogram E D′. If the elasticity were nothing, the ball would move along the line B D′, and if again it were = 1, it would move along a line B A′, obtaining by producing A C till C A′ became equal to it and joining B A′.—When both bodies are moveable, we shall suppose them spherical balls moving along the same plane in the same line of their centres—to avoid considering rotation in them. It will be easy, having arrived at the result in that case, to consider afterwards the cases of oblique motion, as we have just done in the simple case. Suppose we have two perfectly inelastic bodies, A and B, with velocities, a and b, respectively. Such that A a = B b, moving in contrary directions, it is manifest that all motion will be destroyed. The quantities of motion in the two bodies are quite equal and simply opposed. If instead, the two bodies move in the same direction, the one overbutting the other, and both after that moving together, the resulting velocity may be thus found. The quantities of motion, A a and B b, belonging to both bodies must evidently be added together—because the motion is in the same direction in the two cases. But this total quantity is partaken between the two bodies A and B, and therefore

$$\frac{A\,a}{A} = \frac{B\,b}{B}$$

will represent the resulting velocity. Before proceeding to take further into account the resilience due to partial elasticity, we must make the postulate, that if the same velocity be added to or taken from both balls, so as not to alter at all their rate of approach, the impact will be the same as before; and if we take care to abstract or restore these foreign velocities after impact, the circumstances will be entirely the same. Thus, if two balls flying in mid air, strike one another, each partakes of the motion of the Earth as well as of its own motion, and yet we safely enough never take this into account, resting on this principle. Taking it, then, for granted, let A and B, moving with velocities a and b respectively in the same direction, A being the last of the two, and a, the greatest velocity, and let the velocity x, in the contrary direction be given to each. Then, if x be less than a and greater than b, they will now move, A in the same direction as before, with velocity $a - x$, and B in the contrary direction to meet A, with velocity $x - b$. Let the value x to be added, be determined by the equation A $(a - x)$ = B $(x - b)$, so that the bodies now, if quite inelastic, would remain quite at rest in impact, the velocity x having been taken from each. It is manifest that, if so, they would, in

the actual case (restoring that velocity), move each with the velocity whose value is x, that is, by solving the equation

$$\frac{A\,a}{A} = \frac{B\,b}{B},$$

the result we already reached. But they are not so non-elastic, but move back with a velocity $e\,(a - x)$ and $e\,(x - b)$, depending on the impact alone, the first in the direction C A, the last

Fig. 2.

in that C B. Restore the velocity x, which was taken from both. Then, as in both cases it was in the direction from A to B, it will have to be subtracted from $e\,(a - \lambda)$ and added to $e\,(x - b)$, and we have the velocity in the direction from C to A $= e\,(a - \lambda) - x$, and that from C to B $= x + e\,(x - b)$. In order to have the two velocities stated in the same direction, which, according to algebraical principles (*the meaning of Negative Quantities*), can include the other case. Suppose the direction from C to A changed into that from C to B; the value of the velocity will then change sign, becoming, instead of $e\,(a - \lambda) - x$, $x - e\,(a - \lambda)$. Now, substitute for x the value found, and we obtain the new velocities

$$\text{of } A = \frac{A\,a + B\,b - e\,B\,(a - b)}{A + B}, \text{ call this } = u,$$

$$\text{of } B = \frac{A\,a + B\,b + e\,A\,(a - b)}{A + B}, \text{ call this } = v.$$

It is interesting, from the general formula, to come to some of its direct algebraical results. For instance, the original velocity of A being a, and its new one u, it has lost a velocity of

$$(l + e)\,(a - b)\,.\,\frac{B}{A + B}, \text{ which call } \alpha,$$

that gained similarly by B is,

$$(l + e)\,(a - b)\,.\,\frac{A}{A + B}, \text{ which call } \beta.$$

Again, the momentum which A has lost and b has gained is,

$$(l + e)\,(a - b)\,.\,\frac{A\,B}{A + B}\,.$$

A curious relation holds between the new and old differences of velocity. The one, $v - u = e\,(a - b)$. The *vis viva*, originally, $A\,a^2 + B\,b^2$, and the *vis viva* now, hold this relation,

$$A\,u^2 + B\,v^2 = A\,a^2 + B\,b^2 - (l - e^2)(a - b)^2 \frac{A\,B}{A + B}$$

$$= A\,a^2 + B\,b^2 - \frac{l - e}{l + e}\left\{A\,\alpha^2 + B\,\beta^2\right\}$$

As the *vis viva* is just double of the mechanical value of the motion, the original mechanical value (say 0) bears this relation to the new (say N),

$$0 = N - \left(\frac{l - e^2}{2}\right)(a - b)^2 \frac{A\,B}{A + B}$$

$$0 = N - \frac{l - e}{2\,(l + e)}\left\{A\,\alpha^2 + B\,\beta^2\right\}.$$

And these differences between 0 and N are *mechanically equivalent to the heat evolved.* That heat is distributed variously between the bodies and the surrounding air.—From the general formulæ just deduced, a multitude of special ones can be obtained by giving special values to A, a, B, b, and e. The case of the perfectly inelastic balls is solved by taking $e = 0$, and gives the results already indicated. If we wish to take the case of such balls, meeting one another, we have simply to suppose one velocity (suppose b), negative, instead of positive. In the case of two perfectly elastic balls, we should have,

$$u = \frac{A\,a + l\,B\,b - B\,a}{A + B},$$

$$v = \frac{B\,b + 2\,A\,a - A\,b}{A + B},$$

$$v - u = a - b$$

$$A\,u^2 + B\,v^2 = A\,a^2 + B\,b^2$$

$$\text{and } 0 = N,$$

so that no heat, in that case, would be generated. Similarly, for two perfectly elastic balls moving in opposite directions, suppose b negative in the formulæ. If one ball be originally stationary, suppose $v = 0$; then,

$$u = \frac{A\,a - B\,a}{A + B},$$

$$v = \frac{2\,A\,a}{A + B}.$$

If the two balls be equal in size, as well as perfectly elastic, A and B being equal, the formula

$$u = \frac{A\,a + 2\,B\,b - B\,a}{A + B}, \text{ becomes } u = b$$

$$\text{and } v = \frac{B\,b + 2\,A\,a - A\,b}{A + B}, \text{ becomes } u =$$

so that the balls interchange velocities in impact. Exactly the same happens if one of the ball move in the contrary direction to the other (b suppose, being negative), only, that instead of going on in the same direction after impact, they simply go back over the same path as before i —In the less simple case, where two balls strik one another obliquely, omitting the rotatory effe as involving too complex mechanical principl for statement here, we have only to compoun

he velocities each into one along the line of entres, and another perpendicular to that line. 'he latter would have no effect in making the odies meet, and may, therefore, be disregarded. 'he former two forces, along the line of centre, lone operate in impact, and the determination of he resultant velocities is precisely the problem re have solved. Certain velocities would then e found just as if those perpendicular to the ne of centre were not there. Compound the fter-impact velocity of each ball, with the efore-impact velocity perpendicular to the line f centre, and we obtain the resulting velocity of ach ball in magnitude and direction. All the rdinary problems of impact can, therefore, be lved by means of this preliminary solution.

Impenetrability. That property which we elieve to exist in matter, of resisting the entrance f two pieces of matter into the same space. Thus hen a nail is knocked into a piece of wood the ood is compressed—forced into occupying a maller space, but the nail and it cannot exist gether. It is supposed that all matter is porous a certain extent; for our common conception matter will not allow us to conceive it com-essible; so that when apparent compression of atter is found, we consider merely that the non-mpressible particles of real matter have been ought closer together so as to occupy the spaces hich were formerly void. In such a case, erefore, as that of the nail in the wood, we cur to our theory of porosity.—But there is phenomenon which does to a large extent sturb this solution. Frequently a body may dissolved in water or other liquids without sible increase of their bulk.—Now how is this be accounted for? Do the solid and liquid rticles actually interpenetrate? Theory an-ers that they do not; but this theory may be e of those universal prejudices which we are one to believe, and have been encouraged in usting? If, however, they do not, how is the t to be explained? There does not seem chemi-l mixture generally, and we cannot see how at l there should be force enough generated by the ere dissolution, to compress the matter to such extent, as the difference between the liquid, d the solid, together, would indicate. The very eatest forces are needed for the compression a liquid—15 lbs. per square inch, giving one ly of $\frac{1}{20000}$th on the bulk of water. Can ces be generated by mere solution, enough to npress, perhaps, to a fifth or a sixth, the bulk that solution? The same thing is observable mixing gases; but that problem can be solved. re it is not easy to see it,—since, at all events, ordinary experiments on the compression of ids prove them very slightly porous,—unless admit their particles themselves to be com-ssible when some force is brought to bear on m, prevented in ordinary circumstances by elopment of repulsive forces between the ticles, while not interfered with in solution; or admit that two kinds of matter may at once occupy the same space. The admission of this last would drive us to Berkeley's matter-annihilating theories; and we can scarcely understand how to admit the former.—It is usual to define impenetrability rather as a term indicating that matter has a property of resisting force, than as any very clearly conceivable *quality* of matter. When ranked as a *quality*, it is called one of the *primary qualities.*

Impermeability. A property which certain substances have, of not being permeable by others, —that is, so that others cannot pass through their mass. In all likelihood there is no body which is impermeable to any fluid under sufficient pressure, but there are many, such as glass, whose pores are so small that no pressures have yet driven fluids through them. Some substances are impermeable to others from repulsions between their molecules. Thus, waterproof cloth or oil skin is impermeable to water from the repulsion of oil and water. Some substances are permeable for certain others, and impermeable (under known pressures) to different ones.

Impetus. See MOMENTUM.

Imponderable. A name given to such agents as heat, light, and electricity, according to the old theories; in which they were supposed to be fluids, so light as to be incapable of being weighed. Now it is recognized that the phenomena classified under these are due to motions excited in ponderable matter, and that they may be rather considered as *forces* than as *fluids.*

Impulse. When a body falls from a height it commences with a very small velocity, and this gradually increases. In one second it has gained a speed of 32·2 feet per second, and at the end of the next of 64·4 feet. But this velocity has in every case taken a certain time to be attained. Now, when a body is struck by another, it seems at once to acquire a velocity, and to proceed with that. Is there then, at the moment of contact, a definite velocity imparted, no time being occupied? Is impulsion or impulse a different method of action from the usual one, in which force gradually produces visible effect? According to all our ideas of force, it is not and cannot be. Force never accomplishes any effect but in some time. Otherwise, calling the unit of force that which makes a unit matter move through a unit space in a unit time, we should have an infinite force. Whatever small space of time is required, some time comparable to the unit (a second) must have been occupied before velocity can be attained. The phenomenon of impulse, then, merely differs from that of gravitation in this, that the force in the latter acts during a period measurable by sense, and in the former not so measurable. Nevertheless it does act in a definite period. There is no such thing *in rerum naturâ* as a force which produces effects in no time. Still the term impulse is convenient, and is therefore employed.

Incidence, Angle of. The angle between the direction in which a line strikes on a plane, and the perpendicular to that plane. See DIOPTRICS, CATOPTRICS.

Inclined Plane. Let A B be an inclined plane on which the body H is set, it is required to determine the circumstances of its descent. The force acting on A B due to gravitation, draws it vertically downward, but it cannot fall downward—the part nearer B, for instance, would have to press through to the level of that near A, before it would be ready to fall without disturbance in its motion; and the plane A B resists.

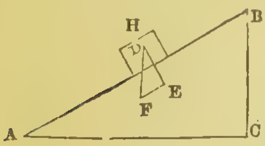


Fig. 1.

The force of gravity (suppose represented in magnitude and direction by D F) can, however, by the composition of forces be resolved into the two forces represented in magnitude and direction by D E and E F (the latter force E F, being along the line A B, instead of its parallel, E F), where D E is perpendicular and E F parallel to A B. Now, this force, D E, simply presses against the plane, and is, evidently, destroyed by it. But the force along A B, equivalent to E F, and in the direction A B, is unbalanced, and will, therefore, pull the body down the plane. It is evident from the figure that, as the angle F D E = B A C, the value of F E will be D F $\times$ sin. B A C, *i.e.*, to the product of the weight by the sine of the angle of inclination of the plane. The pressure on the plane itself will manifestly be D F $\times$ cos. B A C. It is clear, that, since the angles of the triangles A B C, D F E are all equal, we shall have

$$\frac{FE}{FD} = \frac{BC}{AB},$$

or the pulling force bears to the weight of the moving body the same proportion as the height of the plane does to its length along the slope. Again,

$$\frac{DE}{FD} = \frac{AC}{AB},$$

or the pressure on the plane bears to the weight of the body the same proportion that the horizontal length of the plane bears to the length along the slope.—In ordinary books on mechanics, and in many popular physical works, the student will find the inclined plane ranked among what is called the mechanical powers. It is not to be supposed from this name, that any of them create forces,—what they actually effect is, the possibility of using a small force through a great space and a long time, instead of a great one, which we may not have at command, for a small time; or *vice versa*. Thus, in the inclined plane which is employed to slide or roll weights up. The weight, when in such a position as H, tends to fall down the plane with the force E F, and this force must be opposed by an equal one, that the body may not fall, and by a greater—as little greater as we choose, that it may move up. Hence, we are enabled to roll up a weight with a force, a very little greater than E F, instead of having to use the force D F in lifting it. This is the origin of the position of the inclined plane among the mechanical powers. It will be at once evident that, abstracting all considerations of friction, the velocity with which the body arrives at A, will be the same as that with which, falling freely, it would have reached C. The force acting throughout has been, indeed, in the case before us, only F E, but,

$$\frac{FE}{FD} = \frac{BC}{BA},$$

and, therefore, F E $\times$ B A = F D $\times$ B C. That is, the force multiplied by the space through which it has wrought is the same as the force by the space if the body had fallen freely to C from B. The work done is the same; and, in consequence, the resultant velocity—to produce which, the whole work is spent in each case—will be the same. If, then, we let a body, H, fall from B, either by the inclined plane B A, or by the vertical descent B C, it will reach the level of A and C (which are supposed to be on the same level), having the same velocity. Not, however, in the same time. The force F E has, in fact, been acting instead of F D, and, according to the definition of forces, in the same time they will have produced motion through space proportional to F E and F D. Now, if A M be drawn perpendicular to A B, and B C produced to meet it, B A will bear the same relation to B M that F E does to F D. Hence, since the force F E has produced motion through B A, the force F D would have in the same time produced motion through B M, which bears B A the same ratio that B A does to B C, and is a mean proportional between B C and B A.—In all actual applications of the inclined plane, we require to recollect that *friction* exists. See FRICTION.

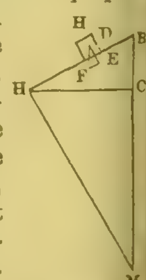


Fig. 2.

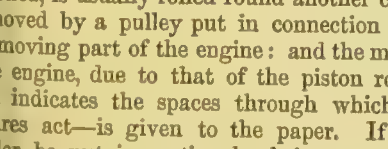
Incompressibility. A property supposed to belong to fluids. It is now found that though very slight effects are produced on most of their by compression, they are yet compressible. It is supposed that no incompressible matter exists.

Increments, Doctrine of. A Calculus invented by Dr. Brook Taylor. It is really equivalent to the Calculus of Finite Differences, (*q.v.*)

Index, Refractive. See DIOPTRICS and REFRACTIVE.

Indicator. A Dynamometer applied to the determination of the work actually done by steam engines.—Its principles are exactly those of the ordinary Dynamometer, and we only explain it separately, because of the extensive use of th

ıachine with which it is connected. A very full ccount of it will be found in the *Dictionnaire des rts et Manufactures*, Paris, 1852; and another ı the treatise on the *Steam Engine*, published y the Artizan Club, edited by Bourne.—It con-sts of a small cylinder with piston attachable the cylinder of a steam engine, and connected ith it by valves in the ordinary way. The ston rod consists of a spiral spring of any con-enient strength. When steam is admitted from e cylinder of the engine, the valves connected ith the indicator cylinder, which must be com-ratively small, are opened, and the piston rod is shed up—proportionally to the impressed force since the spring affords a considerable resist-ce. If, in the course of the stroke, the pressure the steam gets weaker, the spring relaxes and s down the indicator piston. If the piston rod ry a pencil, it may register its motion upon a ce of paper, so that we may know the height, d, consequently, the pressure. Now, the pres-es upon the piston of the steam engine are the ces to which its work is due; and by this ans, we may obtain the successive values of forces. We have seen in the article DYNA-METER, that this would not be sufficient to ermine the work really done by the engine; l we have seen there, besides, that the arrange-nt described would not be sufficient for even aining the successive pressures. But a remedy y be found, by giving the paper a *lateral mo-;*; and, still further, use may be made of such edients, by connecting this motion with the on rod of the engine, or with any other part ved by it. In this case, as we have already , we may obtain diagrams indicative at once he successive forces, and of the spaces through ch these forces have severally acted. These grams must give the whole work done at the in of power in the engine.—The paper, just tioned, is usually rolled round another cylin-moved by a pulley put in connection with e moving part of the engine: and the motion he engine, due to that of the piston rod,—ch indicates the spaces through which the sures act—is given to the paper. If this nder be put in motion by being connected the engine, while the valves of the indicator shut, the pencil, not rising or falling, but ing constantly on the revolving surface of paper, will describe a horizontal ring round orresponding to atmospheric pressures. If valves be now opened and steam admitted,



encil will rise along the paper, and describe cending spiral, sometimes, perhaps, descend-ing where the pressure momentarily lessens, and descending considerably after the steam is cut off—something as if the curve A P were rolled round a cylinder.—When the piston of the cylinder falls, the pressure, from the vacuum created, suddenly becomes very low, and the indi-cator piston falls almost immediately below the point where it stood before the admission of steam —because now the atmospheric pressure above it is not balanced by an equal one below, and the spring must relax downward, until a balance ensues. The pencil will suddenly fall therefore, and so go on, until the admission of new steam gives a sudden rise of the pencil, which would lift it up from Y, along to P, when it again falls. —The cylinder round which the paper is rolled must have a reciprocating motion. Its connection with the piston, which moves first in one direc-tion, and then back in the opposite, may be easily made to secure this. Were it not so, the lower part of the diagram would be prolonged in the same direction as the upper.—The usual method of determining the work done during a double stroke is to divide the figure by 12 ordinates, equidistant and perpendicular to the atmospheric line, and to take the mean of all the ordinates between two of them as the representative of the average pressure for the space intervening. If the piston have a 9 feet stroke, the value of each division will be represented by $\frac{9}{12}$ feet, or 9 inches, and the number of feet found of work done in the double stroke will be obtained by adding the products for all the 12 spaces, of all the pressures represented in the side scale by the perpendiculars, by $\frac{9}{12}$.—In engines such as those now in use, where both the upper and lower parts of the cylinder are employed for the entrance of steam, and for the procuring of a vacuum, we may have the indicator applied either to the top or bottom of the cylinder. It indicates by the successive ordinates, not the value of the pressures in either cylinder, but the difference of pressures, just as in the original engines open to air at the top, an indicator would have given the difference between the pressure in the cylinder and that of the atmosphere.—The figure is an actual diagram from the engines of H.M.S. Spiteful.—The desideratum yet to be effectively and simply supplied, is to secure a means by which the engine will not only give one diagram when it is wanted, but may be set to register its own work for any length of time. The prac-tical value of such a persistence can scarcely be over-estimated. Even the indicators we have, though falling far short of this, are of the utmost mechanical value in indicating faults in the engine, and in telling exactly where these may be found. (See Bourne, as above.)—The merit of the invention is due to Watt; though the method of obtaining diagrams, which gives such a value to the instrument, was not suggested by him. It was first used in the Soho Engineer-ing Establishment.

Indiction, Cycle of. See Cycle.

Indifferent Equilibrium. See Stability.

Indivisibles,—*the Method of Cavalieri.* The method of Indivisibles must be taken as the foundation of the Infinitesimal Calculus of Leibnitz, in the same sense as the method of *Exhaustion* is the foundation of the doctrine of Limits of Newton. Compared with the latter, its logic and processes are loose and unsatisfactory, although in action it is comparatively facile and seldom inaccurate. Its principle is that a line consists of a number of contiguous points; a surface of contiguous lines, and a solid of contiguous surfaces. To value either line, surface, or solid, therefore, it is needful simply to value these contiguous *indivisibles* or *infinitesimals*, and to ascertain the law of their progression or connection. If the law is such that we can *sum* or *integrate* them, we may thus obtain the value of the whole *line*, *surface*, or *solid.* See Calculus, Exhaustions, Limits, &c.

Induction. A term now employed to designate a very large and important class of Electrical Phenomena, and intimately connected with all Electrical Theories. There are two aspects under which Induction manifests itself.

(1.) *The Ordinary Static Induction.*—Electricity, as essentially a *polar force*, is in every phase and character subject to the fundamental law of *polarity*, viz., *one kind of Electricity never can be manifested by itself:* evolve the positive or vitreous force, and we may be certain that an exact equivalent of the negative or resinous force must be evolved at the same time. If a conductor charged *positively* is placed isolated in any position, the molecules of the surrounding atmosphere immediately assume the negative state; and if a second isolated conductor is placed in the neighbourhood of the first one, this polarity communicated—*via* the atmosphere—instantly effects it, *i.e.*, it takes on an equivalent *negative* state. The second conductor is said in such a case to be excited through *induction.* The theory of the *Electrophorus* and of the *Condenser* depends on this remarkable form of action. The student will find the entire subject discussed in the writings of Faraday, to whom we owe most of our accurate conceptions of Polarity.

(2.) *Induction Dynamic, or the Induction of Currents by Currents.*—This much more complex and singular manifestation of indirect Electricity also owes its virtual discovery, and certainly the discovery of all its laws, to Faraday. In its simplest form the phenomenon may be stated as follows:—Let A B be a wire whose ends are connected with the poles of a voltaic battery, and C D another wire unconnected either with the wire or the battery, but lying parallel to A B, and having its ends connected with a galvanometer or multiplier; it is found that on the instant when a current is either established or broken through, A B, the needle of the galvanometer, is deflected, showing the establishment of an *induced current* through C D: when contact is first established the induced current has the same direction as the inducing one; the contrary when contact is broken. During stable connection of the ends of A B with the voltaic battery, the needle of the multiplier returns to its condition of repose.—So much has been already detailed in this dictionary concerning the relations of currents original and induced, under Electro-Dynamics, that we shall only recapitulate here some facts and laws on this interesting subject. (1.) *A magnet may induce a current.*—If a helix or cylindrical mass of isolated wire have its centre hollow, and its two ends connected with a Multiplier, the needle of the Multiplier will deviate, on the instant that a magnet is pushed through the hollow centre of the helix, or withdraw from it. (2.) *A current, named technically the* extra-current, may be induced *through a wire along which a direct current is passing.*—The use of this extra-current is very important. Delarive has employed it with great effect in augmenting the electric tension of a couple. See the valuable treatise on Electricity by this distinguished Physicist. (3.) *Induction is produced by magnets on electric conductors at some distance from them.*—When metallic masses of copper, silver, gold, &c., are placed at a little distance from the poles of powerful electro-magnets with horizontal faces, the effects of induction are readily recognized. And these effects originate precisely the same attractions and repulsions as the ordinary electric currents—showing that ordinary currents and their induced currents are subject to the same Laws. (4.) *General laws of induced currents.*—The following leading facts may be accounted ascertained. *First.* The establishment of an electric current through a metalli[c] wire, does not take place instantaneously, or with[-]out having to overcome a certain resistance. Thi[s] resistance is expressed by a certain *co-efficient o[f] inertia*, which is always constant for the sam[e] metal.—*Secondly.* The intensity of induced cu[r]rents, whether inverse or direct, is an exponenti[al] function of the electro-motor force of the pile, [of] the resistance of the circuit traversed by the cu[r]rent, and of the time—reckoned from the insta[nt] of the opening or closing of the circuit.—*Third[ly].* The induced current has for its limit of intensit[y] the intensity of the inducing current; *i.e.*, t[he] final current has no intensity. There is no spa[rk] accordingly when it is *closed.—Fourthly.* T[he] direct or returning induced current has, at t[he] moment of *opening* the circuit, an additional [re]sistance to overpower, depending on the dista[nce] between the two extremities of the broken [or] opened circuit. A spark may always, or alm[ost] always, be produced then.—*Fifthly.* The ti[me] requisite for the complete establishment of a c[ur]rent, in a circuit of known resistance, is prop[or]tionate to that resistance, and to the *inverse r[atio]* of the electro-motor force of the pile. (5.) *In[flu]ence of media on the effects of Induction.*—

most general known results, with regard to the effects of media, may at present be expressed as follows:—On covering electro-magnets with metallic envelopes, the velocity of magnetization or unmagnetization is diminished: this may be explained by the presence of currents of induction within the metallic masses; and it accounts for the method employed to diminish the shock in magneto-electric apparatus by enveloping these machines in a metallic cylinder. It is essential to notice that by such a contrivance *the intensity or energy of the inductive action is not altered—only its duration.* For details of the effects of media, see the writings of Davy, Henry, and especially Dové. (6.) *Different orders of currents of Induction.*—Investigations with regard to the influence of media, show that the interposition of a plate of a conducting substance between the primary current and the secondary or induced current, may originate a secondary current destructive of the effect of the primary one. It is not therefore unexpected that when a conducting wire is traversed by an induced current, it can originate another induced current—a current of the *secondary order*—in a third wire; and that this may establish in a fourth wire an induced current of the *third order*, &c., &c. The following is the law of the nature of these currents as established by Henry and Abria:—

I. *When the Circuit is Closed.*

Inducing current		+
Induced curent	1st order	—
Do.	2d order	+
Do.	3d order	—
Do.	4th order	+
&c., &c.,	&c.	

II. *When the Circuit is Opened.*

Inducing current		+
Induced current	1st order	+
Do.	2d order	—
Do.	3d order	+
Do	4th order	—
&c., &c.,	&c.	

(7.) *Induction due to the action of the Earth.*—It has been quite established by Faraday, Palmieri, and Sinari, that Terrestrial Magnetism acts on all bodies on the surface of the earth, by inducing electric currents within them precisely as if a strong magnet were placed within the interior of the globe in the direction of the free magnetic needle; and that these currents are directed from east to west parallel to the Magnetic Equator. This *telluric-magneto-electric* action has no doubt much to do with meteorological phenomena. (8.) *Induction due to Electric discharges.*—This can scarcely be termed a separate phase of Induction. If, as has been seen, induced instantaneous currents can develop in their turn other currents of induction, it might be expected that electric discharges, such as the discharge of the Leyden jar, must equally produce them. See the theoretical and experimental researches of Henry, Marianini, Matteucci, Riess, and Verdet. —The student is further referred to the admirable general treatises by Delarive and Becquerel.

Inductive Electric Machine, Rhumkorff's. One of the most powerful means of obtaining intense electric effects at present existing. The following is an outline of its structure and character; but the student who would understand it thoroughly and appreciate its varied applications is referred to a pamphlet by M. Moncel. The apparatus consists of a long cylinder of thin card-board, terminated by circles of glass or wood, and covered by a *primary* circuit, composed of insulated and pretty thick copper wire, through which the inducing electric current is made to pass. The extremities of this primary wire are attached to copper columns fixed on the framework of the apparatus.—Over this primary circuit is rolled a thin copper wire of great length, covered with silk and gum lac, the recipient of the *induced* current. The length of this second wire, by opposing resistance to the movement of the Electricity in the first or inducing wire, *augments its intensity;* and this is the main cause of the astonishing and really immense effects producible by Rhumkorff's machine. The mechanism of the whole is most admirable, proving the artist to be a man of genius. The instrument is not yet adequately known in this country.

Indus. A constellation of Bayer, situate between the South Pole and Sagittarius. Its largest star α Indi is of the third magnitude.

Inequality. It is usual in computing the motions of a planet, to calculate the place in which it is, at any given time, *first*, as that in which it would be, if it moved in a circle, having its average distance from that centre round which it moves, for radius, at a uniform rate. Whatever corrections require to be applied, as for an elliptical orbit, for a variable rate, for a precession of the line of the major axis, &c., are called *Inequalities.* They are generally divided, in a systematic account of them, into those which do, and those which do not depend on the eccentricity.

Inertia. The principle generally named the principle of the *Inertia of Matter*, is twofold. The first part of it is a pure, but convenient *hypothesis.* This hypothesis is, that all external nature is naturally inert, motionless, lifeless; and that action, or activity, can be impressed on it solely by external energies or forces. But in so far as we can form any conception of the constitution of matter, this is physically quite untrue,—not an atom existing which is not the centre and source of manifold and multiform activities. The hypothesis is convenient, however, and even necessary to the development of science:—we inquire theoretically what kind of motions could be impressed on an inert point by the action of certain external forces; and—these determined—we infer, or expect to infer, something concerning the nature of those inherent activities, or affections, which positively establish

the place occupied by that point amid the changes of the Universe. No other possible account can be rightly given of this conception of *Inertia*.—But there is, besides, the *Law of Inertia*,—the fundamental *Law of Motion*,—the Law; viz., that a body subjected to any impulse, or cause of Motion, *would*, if not further disturbed, *move on for ever in a straight line, and with an uniform velocity*. For a full discussion regarding this and cognate Laws, see LAWS OF MOTION.

Inflexion. See DIFFRACTION.

Insulation. When Electricity is given to a mass of iron held in the hand, it does not remain on it, but passes off immediately to the earth. This happens whether the Electricity imparted be vitreous or resinous, positive or negative. If Electricity be, on the other hand, imparted to a bar of glass it will hold its charge for a long time, though in contact with the hand. Some kinds of matter, therefore, conduct electricity, and some other kinds seem to resist its passage. In the case of the glass, the earth has not changed, and is ready to conduct the electricity away, but the glass resists its passage down.—It is called, therefore, a *non-conductor* or an *insulator*. The latter name it gets from this fact, that when a conducting body, such as iron, is not in connection with the earth except through some such insulator, it receives electricity and retains it, as if it had been a non-conductor. Thus, if we stand on a stool, with glass legs, and place ourselves in connection with a working electric machine, we receive a charge which we can give off when we put ourselves in connection with a conductor. Bodies then, which like glass, resist the passage of electricity, are called insulators, and those which do not are non-insulators. Glass, the resins, such as shellac, gutta-percha, &c., are good insulators. Air also is an insulator, else as all bodies are surrounded by it in ordinary cases, it would be only possible to give a charge in a Torricellian vacuum. Water is not an insulator, and from this cause arises the chief difficulty found in producing insulation. A light film of aqueous vapour very readily deposits on the surface of such an insulator, as a glass stool, and conducts the electricity downwards to the earth. It is needful, therefore, to heat our insulators well before using them, carefully rubbing off all particles of moisture. It is useful besides to coat a glass insulator with a thin surface of shellac, which is not so liable to become moist, as the glass, and round which, therefore, this conducting film does not so readily form. Shellac is not probably, in its interior, so good an insulator as glass, but its surface is much better, in consequence of this property.—No body is a perfect insulator.—Electricity, however, of very low tension, *i.e.*, feeble transmissory power, will pass along an iron wire at the rate of thousands of miles per second, while it will take an hour, even if the tension be very high, to creep an inch along a piece of glass. This will give an idea of the extent of this insulating quality, and of the differences it must imply in the nature of these substances.

Integration. The process of the integral calculus is essentially of the following nature:—Given a series of quantities, each of which becomes indefinitely small while their number increases indefinitely, it is required to know to what quantity their sum constantly approximates. Thus, supposing that we have a curved line, along the arch of which we draw several chords, then if we take the chords as such a series, increasing the number of the chords, and so diminishing the magnitude, since they all travel between two definite points, *i.e.*, the extremities of the chords, in a sort of general direction or course, that is, along the arch, it is required to know to what constant quantity the sum of the chords approaches without ever reaching it. It is sufficiently evident that this sum will approach to the arch itself, and so if we can by means of the integral calculus discover its exact amount, we shall clearly be able to tell the length of the arch. Hence the integral calculus enables us frequently to obtain definite values for otherwise unknown quantities. Similarly by taking what are called elements of the area, we can obtain the area included between a curve and what is called its ordinate, or if the curve be, like the circle, an enclosed one—terminating in itself,—we can tell thus the area which the line of curve contains. The elements are the little areas contained between indefinitely small arcs of the curve, and indefinitely near parallel lines. The smaller they become, the nearer they will be in area to little rectangles having one of the parallel lines for sides, and the breadth between them for the other, and the whole sum making up, in any case, the complete area of the curve, we may take instead of it the quantity, found by the integral calculus to which the sum of these constantly diminishing rectangles approaches. The sign of integration is $\int$, the old form of *s*, the initial of *summa*.—If we have a series of quantities which, on being thus individually diminished, but increased in number, constantly approach to any given value, suppose the value 2, then they will also manifestly constantly approach to any number above that value, such as 3, 4, &c., although they always remain far below these latter numbers. Hence, the integral calculus might give us with as much propriety, in answer to the question, which as we have stated it answers, the numbers 3 or 4 as the number 2. How then shall we be able to distinguish? In fact there is difficulty in doing so, but it is nevertheless possible in most cases. The answer usually comes out $= 2 + a$, taking (*e.g.*) 2 as the correct answer or proper limit, and this quantity a is indefinite, but its value may usually be assigned through some datum of the special question. Sometimes it happens that the

first quantity 2 does not come out quite evidently at first, part of the quantity a' being as it were brought into and mixed up with the algebraical quantity expressing it, but, then also, it is possible to tell the exact value in ordinary cases. The methods by which this lowest value to which the sum approximates—or *limit* of the sum—is usually found, are called those of *definite integrals.* (See *Todhunter's Calculus*, *De Morgan's Calculus*, *Whewell's Doctrine of Limits*, and our articles LIMITS and CALCULUS.

Interference. A name given to an occurrence that must take place, under certain circumstances, with regard to all phenomena which depend on, or are affected by, the propagation of Waves; and the fertile explanation of many of the most striking facts in Physical Optics.—In order that its nature be clearly apprehended let us refer to the case of the Tides. In the free ocean there is but one great tidal wave; but when that tidal wave enters channels among clusters of islands, it is broken into several subsidiary ones; and, on the other side of an island, these subsidiary waves frequently meet and modify each other,—the actual tide on the coast being then not the general great luni-solar wave, but the result of the meeting of these partial waves. The tide at the port of *Batsha*, for instance, is a perfect example of such interference in its completest form. The actual tide of this harbour results from two sets of waves passing through two channels, whose lengths are so different, that the tidal wave through the one, reaches Batsha exactly six hours earlier than the corresponding wave through the other. *High water*, as determined by the one wave, thus meets *Low water* as determined by the other; and the consequence is, that at those seasons at which the morning and evening tides are equal, there is no tide at all at Batsha—the hollow of the one wave being merely filled up by the crest of the other; while at all other seasons there is only a small tide, due to the difference between the heights of the morning and evening tides. In the same manner have several apparently perplexing irregularities of the tides in the port of London been recently explained by Dr. Whewell. This tide is really the result of the *interference* of two tidal waves, one of which flows through the English Channel direct from the Atlantic, and the other—also from the Atlantic, but propelled towards London from the north—through the German Ocean.—Ingenious experiments, contrived by Mr. Hopkins, show, as one might quite expect, the applicability of the same principle of interference to the aerial waves of *Sound.* These waves or pulsations differ from the tidal waves in this,—they consist of alternating expanded and compressed portions of the Atmosphere; so that *interference* would consist, in this case, in the meeting of the expanded portion of one wave with the compressed portion of another. Now it is found that if two separate tubes be made to join or meet in one common tube, and if sound-waves, in opposite states, be propagated through the separate tubes, there is no wave or sound whatsoever in the tube in which they meet.—But let us hasten to occupy ourselves exclusively with the remarkable subject of the *Interference of Light.*—The fundamental *fact*, connected with this large class of phenomena, is unquestionably due to the Jesuit Grimaldi. To him science is indebted for the earliest statement of the apparent paradox, *that light added to light may produce darkness.* This was the form of his experiment:—Let rays of light pass into a dark room through two neighbouring small circular openings,—each of these openings will be the apex of a luminous cone surrounded by a penumbra less bright. Receive these two cones on the same screen, and it will appear, that although portions of the region within which the cones mingle with each other, are brighter than they would be, were only one cone received on the same screen, there are other parts much darker, or rather *wholly unilluminated*; and, further, that these dark parts became bright again, the instant that *the light of one of the cones is removed* by closing the orifice at its apex. Until the period, however, of the labours of our countryman, Dr. Thomas Young, nothing additional accrued in relation to this curious subject, and not a step was taken towards explanation of the phenomenon. Young first greatly improved the form of Grimaldi's experiment. Instead of circular openings, he employed narrow rectilineal ones, and by this means obtained Grimaldi's results much more clearly and definitely: if homogeneous instead of the usual composite solar light be employed, a screen in this case exhibits an alternating series of well marked dark and bright bands passing on either side of the centre of the space separating the two slits. *Close one slit, and the dark bands affecting the light entering by the other wholly and instantaneously disappear.* The mode of the experiment received its last perfection from Fresnel. Lest it might be said that these bands arise from some action of the edges of the *hole* or the *slit*, Fresnel substituted the two inclined mirrors described under DIFFRACTION, and thus completed the demonstration, that Light added to Light may produce darkness. To the further and ingenious modifications of this fundamental experiment, as proposed by Lloyd and others, we cannot here refer.—The fact clearly established, what is its cause? It is one of the strongest of Young's many titles to a high permanent place in Scientific History, that, first of all, he answered the question. Before his time, the Undulatory Theory of Light had been propounded, and was rising into esteem. But if Light is not an *Emission*—if, like the *Tide*, or like *Sound*, it is propagated by systems of waves, is it not certain that two systems of waves proceeding from different sources and intermingling, will necessarily present lines or curves along which the crest of the one just

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fills up the trough of the other,—in other words, where there must be darkness? We are not required to carry out here specific illustrations of this principle, because of the full details in article DIFFRACTION; but it is hoped that what has been said even here, will give the student a distinct conception of the nature of what is termed INTERFERENCE.—In the remainder of this article, we shall state briefly the general laws of Interference, advert to a few interesting specific inquiries connected with it, indicate the probable extent of its range among phenomena, and notice a few of its proposed applications.

I.—According to the Undulatory theory, the waves of light are found in two conditions, the condition that constitutes *Ordinary light*, and the condition that constitutes *Polarized light*. The difference is supposed to be, that while, in the former case, the vibrations of the particles constituting the wave always correspond in all respects in regard to their *plane*,—these vibrations, in the latter case, are, in every separate instance, confined to some special plane. The student will fully comprehend this rather obscure subject on referring to POLARIZATION: he is required at present merely to keep in mind, that if a beam or wave of ordinary light, meets another beam or wave of the same ordinary light, we are not required to raise any question as to the direction in which the wave particles are vibrating, seeing that these are the same in both beams; but that the case is quite different when a question is started regarding two rays of Polarized Light. Under these conditions let us study *Interference* as it must be supposed to influence the meeting of rays of both sorts.

(1.) *Unpolarized, or Common Light.*—This subject also consists of two parts—viz., when the rays that *interfere* are *homogeneous*, and when, as in the case of the ordinary solar beam, they consist of several distinct colours, or, what is the same thing, of several distinct and easily distinguishable sets of vibrations. Both cases, however, have already been so fully treated under DIFFRACTION, that regard to the narrowness of our space, prevents our further adverting to them here.—On one collateral point of much interest, however, a remark may be made. It long remained one of the *crucial* questions between the theory of Emission and the theory of Undulation —whether light is retarded or accelerated on passing from a rare medium into a dense one. Accept the theory of *Emission*, it ought to be *accelerated*: accept the theory of *Undulation*, and we expect it to be *retarded*. The question has at length been finally settled by direct experiment—(see LIGHT, VELOCITY OF), but it is also determinable by aid of the principle of Interference. If one of the rays whose interference causes those symmetrical fringes on the screen alluded to above, and so fully described under DIFFRACTION, be made to pass through a dense medium—say a thin or transparent plate or a tube containing water; and if its velocity be thus altered, the system of fringes will no longer be found on the screen symmetrically with the space between the two slits—*i.e.*, the central band will no longer be opposite the centre of that space. It is clear that if the one ray of light has been retarded by passing through the denser medium, the action of the other ray must, so to speak, have *gained upon it*, and the system of fringes will shift *towards* the slit through which the retarded ray is entering: if, on the other hand, the ray has been accelerated, the reverse must follow. Experiment in the hands of Arago pronounced for the former result; and thus stepped out in advance of the very brilliant measurements of Foucault and Fizeau.

(2.) *Polarized Light.*—We have now to take into account the *planes* in which the particles of the Light-wave vibrate. The following are the laws established by Arago and Fresnel:—1. Two rays polarized in the *same* plane necessarily interfere exactly as if they were rays of unpolarized light. This in fact is a mere case of that general one in which we do not require to take any account of the planes of vibration.—2. Two rays polarized in planes at right angles to each other can in no case produce the phenomena of interference, at whatever phase of their vibration they may meet. It is manifest at first sight that a vibration from A to A′, and *vice versa*, cannot at all affect a vibration from B to B′, and *vice versa*. And experiment amply confirms this.—3. Two rays polarized in planes inclined to each other at an acute angle, *interfere partially* when they meet at the proper phase; the intensity of the fringes produced is in that case comparatively feeble. Experiment proves this; and the student will at once discern the cause of it, by resolving one of the vibrations into two—the one at right angles to the plane of the other vibration, and the other in the direction of that plane.—4. Two rays polarized in opposite planes may be brought into the same plane of polarization without thereby acquiring the power to interfere with each other. —5. Two rays treated as above, interfere, as in the case of ordinary light, *provided they belong to a beam, originally and as a whole, polarized along the same plane.* — 6. The phenomena relative to the interference of rays that have undergone *double-refraction*, and the position of the fringes, are not determined solely by consideration of the distances through which the rays have travelled, or their velocities:—regard must be had to a *half-undulation* that may be said to have been *lost*. The loss of this half-undulation is a simple and beautiful consequence of the theory of *transversal vibrations*. In reality the vibration of the wave is divided, in the interior of the crystal, into two, at right angles to each other, and situate—the one in the plane of the principal section, and the other perpendicularly to that plane. Each

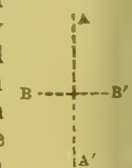

Fig. 1.

of these vibrations must be decomposed anew into two others, according to their rectangular *directions;* and it is easy to conclude that of these *four*—(coming from the primitive vibration)—two, along one of the definite directions, conspire or agree; while the other two, along the direction at right angles to it, are opposed. It is clear, then, that the vibrations of the two couples must be regarded as differing by half an undulation —All these laws have been amply and brilliantly confirmed by the experiments of Arago and Fresnel, and, it may just be added, that they entirely consist with the doctrines of Undulation and Interference.

II.—The foregoing are the simple laws of *Interference;* but we desire to give an idea of the multifarious and complex conditions under which it is found to exert its influence, and of the extent of unexplored field yet remaining in this region of Physical Optics. To do this, in the most palpable manner, we shall specify three problems or classes of phenomena, that have recently engaged attention.

(1.) A very singular experiment was brought a few years ago under notice of scientific men by Prof. Baden Powell. Take a hollow prism or wedge of glass as below, and enclose within it some very refringent or dispersive liquid, such as oil of *sassafras*, or *cassis;* next introduce into it partially, as in the figure, a plate of glass whose lower edge shall be parallel to the base of the prism, or the surface of the liquid. This done, introduce a beam of light through a narrow horizontal slit, and observe the spectrum formed by it, with the eye at the *arrow.* The strangest appearances manifest themselves,—the spectrum thus formed is *crossed by a certain number of dark bands parallel to the slit or to the base of the prism.* Professor Powell tried various combinations of oils and other media, with plates of glass and other transparent substances of different thicknesses:—he found each combination distinguished by its peculiar system of dark bands. Further, he varied the inclination of the immersed plate, sometimes even laying it on the side of the prism,—the bands did not cease to be visible, but their intensity faded as the deviation of the plate from the perpendicular increased. Mr. Stokes proposed a new form of the experiment as below,—the vessel with the liquid and plate, being rectangular, and a prism (of the same material as the vase) fitted to produce a spectrum, being placed outside. The phenomena are precisely the same; but this form of the experiment is preferable, as it separates the agency of the prismatic shape, from the agency which evolves the dark bands.—Complex though these strange appearances unquestionably are, they have all yielded to the principle of *Interference.* Their origin in general terms is this:—the portion of the ray passing through the *plate* is *retarded;* and when the retarded part and the unretarded part emerge and mingle, there is a destruction of light at all those points of the spectrum where the retardation amounts to half an undulation;—hence the dark bands. But this general explanation is not enough;—account must be given of specific cases and their specific phenomena: and it is precisely here that the doctrine of Interference has achieved its triumph. By aid of the formulæ of Fresnel and Airy, Professors Stokes and Powell have traced the dependence of every specialty of these dark bands — their number, thickness, and distribution—on the relative refringent powers of the plate and the liquid medium. Nay, so thoroughly has this been accomplished. that if the refringent or dispersive power of the liquid medium be known, and the number of the bands ascertained by observation, we may *deduce by calculation* the *index of Refraction* of the *Plate.*

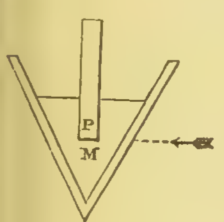

Fig. 2.

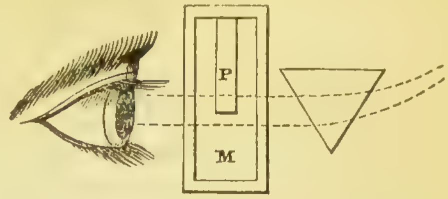

Fig. 3.

(2.) The second instance to which we shall allude, is one of those innumerable classes of facts with which the unequalled industry and accuracy of Sir David Brewster have enriched every department of Physical Optics. Form a spectrum either by *Refraction* or *Diffraction.* Look at it across the edge of a thin plate of *glass*, *quartz*, or *mica;* in other words, place such a plate so that it cover about half the pupil of the eye. If the plate be on the violet side of the spectrum, the entire spectrum will appear traversed by numerous black and nearly equi-distant bands parallel to the dark lines of *Frauenhofer*, and which, speaking generally, increase in breadth with the thickness of the plate. But if the plate is on the red side of the spectrum, no bands are produced. Sir David has studied this curious subject under all its aspects—varying the inclination of the plate, &c., &c. Mr. Airy immediately occupied himself with the theory of these new bands. At the meeting of the British Association, in 1840, he produced a very able memoir, referring them to *Interference;* but on the supposition that, when seen, the spectrum was slightly *out of focus.* Sir David objected, that the phenomenon explained could not be the true phenomenon, because, along with his dark lines, he saw the fixed lines of Frauenhofer, and, therefore, that the spectrum must have been perfectly *in focus.* Mr. Airy resumed his investigations, and generalized them,

finding his former supposition unnecessary; but Sir David again objected to several of his formulæ and specific results. Professor Powell has since then reviewed the whole curious question, expressing a strong conviction that Mr. Airy's two memoirs contain a satisfactory reference to the theory of *Interference*, of all phenomena belonging to this class, at present known. He confesses at the same time that peculiarities may occur not easily reducible by the calculus, but contends that even such a circumstance ought not to be held as invalidating a general theory which has explained so much. The whole history of this curious case is most interesting, and ought to be studied in the original *memoirs* by the eminent men we have named.

(3.) It is impossible to omit reference to those recent very interesting researches by Foucault and Fizeau, concerning the interference of two rays of light which have passed through spaces of very different lengths:—the phenomena usually produced, are evolved by rays, differing from each other, in regard of the course they have traversed, by only a very few half undulations. Imagine the fringes of diffraction produced by the mirrors of Fresnel to be received on a screen; and let that screen be pierced by a very fine slit in the midst of the central fringe. If the ray emerging from this slit be passed through some very refringent medium, it will be expanded into the ordinary spectrum, and the observer viewing this spectrum by a telescope will simply discover the usual colours and the fixed lines of Frauenhofer. Suppose now that one of Fresnel's mirrors is made to advance, or brought forward in a direction parallel to its original position, the central band or fringe will be displaced, and instead of it a fringe will be found of an order that will be the higher, the farther the mirror is brought forward. Instantly the spectator will find the spectrum covered with dark and bright lines; and these increase in number and crowd on each other the more, as the mirror is farther and farther brought forward. The ordinary fixed lines, remaining in the spectrum at the same time, one can easily count the similar lines between any two of them, and it is found that this number depends exactly on the amount of the advance of the mirror. If, instead of causing the mirror to advance, a thin plate (a plate of retardation) be placed in the way of one of the rays, precisely the same phenomena occur, and the bands increase in number with the thickness of the plate. The following is the account given by MM. Foucault and Fizeau, of their first general conclusion:—"The number of bands may be made very great, without their ceasing to be observable, through the whole length of the spectrum. When 66 were counted between the Frauenhofer lines, E and F, the phenomenon was distinct and defined, and there were then about 500 in the whole spectrum. When this number is largely augmented they begin to fade, and even no longer to be perceived, near the red extremity;—after that they fade away in the orange,—next in the yellow,—crowding up towards the violet end of the spectrum. The greatest number counted between E and F is 141; they have been seen still more crowded, but in that case they could not be counted, because of the feeble intensity of the light employed in such circumstances to form the spectrum. The progressive disappearance of these bands, beginning with the least refrangible portion of the spectrum, as the difference of the *routes* of the two rays becomes greater and greater, is a phenomenon constantly observed, whatever the means used to produce the interference. If it be remarked, that this disappearance does not begin until the bands have attained an extreme thinness, and if it be recollected that in consequence of the *decreasing dispersion*, from the violet end of the spectrum to its opposite, the light at the latter is necessarily less pure or homogeneous, one may be inclined to attribute the fact just mentioned, to the imperfect separation of the simple rays at the least dispersed extremity of the spectrum."—Foucault and Fizeau have not stopped with the exhibition of the new interferences now described; their elaborate memoir contains a mass of original and most curious investigation, concerning interferences produced by *reflexion* from *thin plates*, and by means of *double refraction*; and they have further investigated the singular effects produced on polarized light by bi-refringent crystals. We cannot follow them through such researches here, but must content ourselves with assenting to the opinion of M. Babinet, who reported on their labours to the Institute, that henceforward the mode of analysis they have used must be the essential instrument in the hands of any one occupied with the higher problems of Physical Optics.

III.—It cannot be doubted, that a principle so fertile and flexible as the one now explained and exemplified, must play a very important part in the production of optical phenomena. Among the more remarkable aspects of external nature, we find as its result, the twinkling of the fixed stars. See SCINTILLATION. As is shown in many separate articles in this Dictionary, it is the source of all these brilliant chromatic phenomena, displayed in diffraction, and the colours manifested by crystals subjected to polarized light; and, indeed, sets of fringes, rectilineal or circular, are rarely met with, unless through the agency of *Interference*. But the point of highest interest, requiring notice in this place, is one suggested by these most instructive experiments of Foucault and Fizeau. These ingenious physicists have placed it beyond all doubt, that by the interference of rays, whose routes differ very much in length, the ordinary spectrum can be crowded with dark bands—these bands increasing in number as the difference augments. It is not possible, then, to refrain from the hypothesis that the puzzling dark lines of Frauenhofer, and even those wonderful

phenomena discovered by Sir David Brewster, concerning the effect of nitrous acid gas, the vapour of iodine, brome, &c., &c., may here find their rational explanation. It had, indeed, been conjectured long before the singular experimental confirmation by Foucault and Fizeau, that such lines are simple results of interference. Sir John Herschel very early took this view, and exhibited it in a paper, published in the *Philosophical Magazine.* But he assigned every individual absorption to a special cause, and was thus obliged to imagine as many different causes within absorbing bodies, as there are dark lines or absorptions in the spectrum. No such explanation could be received; it was the *ignotum per ignotius:* nevertheless, our countryman had the honour of first suggesting what seems the true origin of these puzzling appearances. The credit of carrying out this suggestion, and of first presenting it in an acceptable form, is unquestionably due to Baron Von Wrede, whose most instructive and elaborate Memoir (already frequently referred to), has been worthily included in the valuable repertory of Mr. Taylor. The Baron assumes nothing except one general fact—or rather, what every physicist accepts as a fact regarding the constitution of matter:—viz., that all matter is composed of particles maintained by certain forces at determinate distances from each other. Now, while it is clear that such a constitution must impress certain general conditions on a luminous wave passing into, or through any portion of matter; it is equally manifest that no *à priori* statement can be made as to the specific effects to be expected from the transit of such a wave through any specific kind of matter, because we are quite ignorant of the specific difference between the molecular constitution of any two substances. Theory, in this case, therefore cannot predict particular results; it can only show that molecular differences have the power to produce such. Not passing beyond the foregoing general assumption regarding the constitution of matter, let us imagine a light wave entering or passing through such a mass of connected, but yet separate molecules. It is clear, that these particles will offer a resistance to the wave; part of it will be reflected from surface to surface of the molecules, and when it emerges, it will consist of different waves; some of which have traversed,—through effect of the deviations caused by the various reflections,—distances, very different from those traversed by others. These differences of distances must, of course, be determined, in each case, by the peculiarities of the molecular arrangement, or by the constitution of the body; in some substances, the effects may be inappreciable; but it is easy to conceive that there are others which will cause the ray to emerge with its parts in the precise relationship which gave rise to the interferences, or the dark bands in the striking experiments of Foucault and Fizeau. Baron Von Wrede has not left the subject in the foregoing merely general form. By the application of the calculus, he has shown how, in the chief cases, alike the difference of march, and the intensities of the emerging rays, may be calculated for any number of reflections; and, what is even more satisfactory, he has devised experiments, afterwards perfected by M. Soleil—experiments in which the reflecting surfaces of the molecules are represented by cylindric surfaces of mica—by which it can be made manifest that such bands may be produced, as Sir David Brewster observed in some of his most remarkable experiments. Should Von Wrede's views and ideas, when further developed, fulfil their present apparent promise, they will lead us towards something of a solution of the yet obscure question concerning the natural colour of bodies; and what is of equal importance, the bands produced by the interposition of any gas or other substance will cease to be isolated phenomena,—they will be our first clue to the peculiarities of the molecular constitution of these substances themselves.

IV.—We conclude this article by a brief reference to a few of what may be termed the practical applications of the principle of *interference.*

(1). By its aid, an objection that at one time appeared fatal to the wave theory of Light, is entirely removed. This theory seemed at first inconsistent with the rectilineal propagation of light, inasmuch as a wave, although meeting an obstacle, turns round that obstacle. How then could shadows exist? The reply is quite satisfactory. The wave does turn round obstacles; nevertheless shadows necessarily exist because the different portions of the lateral wave *destroy each other by interference*, and the only efficacious part of the wave, is, on this account, those portions of it which are propagated straight onwards. The grounds of this assertion have already been in so far exposed under DIFFRACTION.

(2). In (1.) I. of this article, it is explained how the interposition of a refracting substance between one ray and the screen, necessarily displaces the system of fringes, in the fundamental experiment of diffraction. The amount of this displacement depends, of course, upon the index of refraction of the interposed plate; for it is in proportion to this index, that the velocity of the ray is accelerated or retarded. It was early suggested by Arago, that the measurement of the displacement furnishes by far the most delicate means of determining the refringent power of any transparent substance; and the accuracy of his opinion is made strikingly apparent by an elaborate investigation of this kind on which he ventured. The question whether the humidity of the atmosphere ought to be taken into account, in the theory of Astronomical refractions, had long been a vexed one. Laplace and Biot had both engaged in the controversy; but instruction by experiment remained absent. The subject was taken up by Arago and Fresnel, —the leading experiment being made by them

conjointly; the correction and confirmation of it by Arago alone. The form of the experiment was simple, although the preparation and conduct of it demanded that precision and care, which one can look for, only from the most expert and distinguished physicists. Two tubes above a yard (a metre), in length, were filled—the one with dry, the other with moist air, at the same temperature and the same elasticity. The open ends of the tubes were closed by plates of glass, in all respects corresponding. The two rays of the diffraction experiment were then made to pass through the two tubes. If the refringent powers of dry and moist air are the same, it is clear that the central band of diffraction would retain its symmetrical position; if they are different, that band and the whole system of fringes must move *towards* the tube containing the most highly refracting medium. *Displacement took place by the breadth of a fringe and a-half, towards the side of the dry air.* Dry air, therefore, is more highly refringent than moist air; and the difference could be calculated. At 80° Fahr. the index of refraction of dry air came out to be

1·0002945,

while that of moist air appeared

1·0002936;

a difference almost infinitesimal, but real and certain;—so delicate an instrument is the principle of Interferences! It might be applied in the same way to test all other qualities of the Atmosphere; and Arago has shown, how by a very simple apparatus, it might indicate variations of the air's pressure and temperature,—thus supplanting the barometer and thermometer. Into further details, however, our limited space forbids us to go.—Such, in outline, the general character of that most important principle, known as the Interference of Light. The student will find it fully developed in all good modern treatises on Light. That he is acquainted with it, will be assumed in many articles on Physical Optics, in the further portions of this Dictionary.

Interpolation. The operation of finding terms between any two consecutive ones of a series which shall conform to the law of the series. In most cases the law of the series is not given, but only numerical values of certain terms of the series, taken at fixed and regular intervals. In this case we may approximate to the interpolated term by the formula—

$$\mathrm{T} = a + \frac{n}{1} d_1 + \frac{n.(n-1)}{1\ 2} d_2$$

$$+ \frac{n(n-1)\ n-2)}{1\ 2\ 3} d_3 + \&\mathrm{c.} \ldots\ldots\ldots (1.)$$

Formula (1.) expresses any term of a series whose terms are computed for values of the variable in arithmetical progression; a denotes the term of the series preceding the interpolated term; d_1, d_2, d_3, d_4, &c., are the first terms of the successive orders of differences, counting from the

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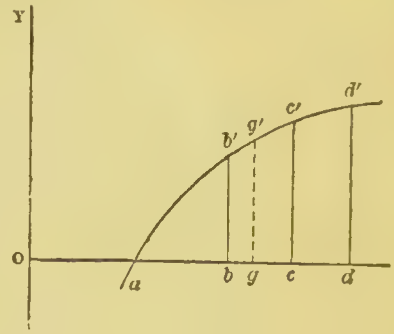
term a, and n denotes the order of the interpolated term. To illustrate the process of interpolation, let us take the equation $y = f(x)$. Now, by assigning values to x, and deducing corresponding values of y, we shall have sets of values of x and y which may be regarded as the co-ordinates of a plane curve that may be constructed. Suppose



O X and O Y to be the axes of co-ordinates, and $a\,b'\,c'\,d'$, &c., the curve; $b\,b'$, $c\,c'$, $d\,d'$, &c., to be ordinates taken at equal intervals, that is, so that $o\,a = a\,b = b\,c = c\,d$, &c. Now, if the curve were accurately constructed, any ordinate $g\,g'$ between $b\,b'$ and $c\,c'$, might be found by drawing $g\,g'$ parallel to O Y, and measuring the length of it by means of a scale of equal parts; but if the curve were only approximately given, the value of $g\,g'$ could only be approximately determined. But if we have tabulated a series of values of y for values of x in arithmetical progression, we can by interpolation obtain, to any degree of exactness, any intermediate ordinate. In order to apply formula (1.), to find the value of $g\,g'$, we should make in it—

$$a = b\,b',\ n = \frac{b\,g}{b\,c}$$

and taking the tabulated values of $b\,b'$, $c\,c'$, $d\,d'$, &c., find the successive order of differences to any required degree of accuracy, and make d_1, d_2, d_3, &c., equal to the first terms of the successive orders of differences. Substituting these expressions, in formula (1.), the value of T will be the ordinate required, or the interpolated term. To illustrate; let it be required to find from the tabulated values of the logarithms of the numbers 12, 13, 14, and 15, the value of the logarithm of $12\frac{1}{2}$:—

Nos.	12.		13.		14.		15.
Log.	1·079181		1·113943		1·46128		1·176091
1st or diffs.		0·034762		0·032185		0·29963	
2d do.			—0·002577		—0·002222		
3d do.				0·000355.			

Counting from log. 12, we have—

$$a = 1{\cdot}079181,\quad n = \tfrac{1}{2},\quad d_1 = 0{\cdot}034762$$
$$d_2 = \quad 0{\cdot}002577 \qquad\qquad d_3 = 0{\cdot}000355$$

Substituting these values in formula (1.), and neglecting all the terms after the fourth, as inappreciable, we have—

$$T = \log. 12\tfrac{1}{2} = a + \frac{1}{2}d_1 + \frac{1}{8}d_2 + \frac{1}{16}d_3 + \&c.,$$
$$= 1\cdot079181 + 0\cdot017381 + 0\cdot000322 + 0\cdot000022 = 1\cdot096906$$

Had it been required to find the logarithm of 12·39, we should have made $n = 0\cdot39$, and the process would have been the same as above. In like manner we may interpolate terms between the tabulated terms of any mathematical table. The method of interpolation is of extensive use, not only in pure analysis and geometry, but also in various other subjects of mathematical inquiry and computation, particularly in Astronomy. In this latter branch of investigation it is the means of saving, in many cases, immensely laborious computations. Thus, for example, in finding the places of some of the planets whose motions are not very rapid, it will be sufficiently accurate to compute their places for every fourth or fifth day, and then, by interpolation, to find their places for intermediate days. Again, in finding the moon's place for any particular hour, supposing its place for every three, six, or twelve hours to be given, the method of interpolation may be applied with great success, the results differing inappreciably from those of actual computation. By this means, also, the place of a comet at any particular time may be determined, from observations made previous and subsequent to that precise period. In a word, Astronomy has derived more assistance from this principle than from almost any other mathematical device.

Invariants. See POLYNOME.

Involute. See EVOLUTE.

Involution, in *Arithmetic* and *Arithmetical Algebra*, is the process by which *numbers*, or literal representations of numbers, or combinations of such, are raised to *powers*, *i.e.*, on which, the act of multiplication into themselves is repeated any number of times. The *Binomial Theorem* is the general representation of the results or laws of such Involution.—In Symbolical Algebra, or the Science of Operations, the significance of the term *Involution* may be extended to represent the repetition upon a quantity or a symbol, of the same operation, any number of times.

Involution, in *Geometry.* A very important and fertile relation between *points* is indicated, in the Higher Geometry, by this term. It is explained fully under RATIO. Technically, it is this,—if a, a'; b, b'; and c, c', be three systems of two points, *conjugate* to each other—*two* to *two*—viz., a to a'; b to b', and c to c'; and if the *anharmonic Ratio* of four of these points—say, a, b, c and c', is equal to that of their four conjugate points; viz., a', b', c' and c—these six points are said to be in *involution*. The real significance of this term and its value in Geometry will be indicated under articles RATIO and POINTS.—See, for the whole subject, the great work of Chasles—*Geometrie Superieure*.

Irene. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Iridescence. The name given to those brilliant displays of colour always manifested when transparent substances are presented in very thin plates—as in the case of *fish scales*, the laminæ of *mother of pearl*, *soap bubbles*, &c. See THIN PLATES, and INTERFERENCE.

Iris. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Iris. See RAINBOW.

Iriscope. An ingenious and simple instrument, invented by Mr. Reade, by which Newton's coloured rings can be readily shown. It is simply a plate of black glass perfectly polished. Let the experimenter cover its surface with thin and very fine soap, and rub it afterwards with a piece of dry chamois leather. On breathing on it afterwards through a glass tube, the vapour deposits itself in brilliant rings, the exterior of which is black, and the interior either white or coloured, according to the quantity of vapour deposited. The colours of these rings seen under ordinary or composite Light, correspond with the reflected rings of Newton,—with this difference, that as the film of vapour is here thickest at its centre, the rings of the Iriscope have *black exteriors* or *contours.* See THIN PLATES.—The *Iriscope* is much more a philosophical toy than a philosophical instrument.

Irradiation. An optical phenomenon briefly explained as follows:—It is well known that the images of objects *persist* on the retina, in *time*, *i.e.*, we continue to see a light for a certain period after that light has gone out; witness the trains of light of the falling stars, and a common child's amusement; viz., the whirling round of a bright point, causing thereby the appearance of a continuous bright circle. *Irradiation* is a precisely similar action of the retina in respect of *space.* Not only does the retina not lose its impression at once, but no part of it can be affected, apparently, without its immediate neighbourhood being affected also. Hence, objects appear to the eye slightly larger than they really are; and when the objects are thin—such as Saturn's Ring seen on edge, the effect of this Irradiation is so great, that it becomes impossible to execute accurate measurements of them. The laws of this special optical illusion—for such it is—have been investigated with great acuteness by M. Plateau, who has thrown so much theoretical and experimental light on the cognate phenomena of *persistance of impressions.* The following are the conclusions of his very remarkable memoirs. For the sake of distinctness, and because of their eminent practical value, we give them in the form of separate and substantive propositions. They express the whole phenomena of Irradiation, as it affects simple vision, or vision aided by Telescopes.

I. *As to simple Vision.*—(1.) Irradiation is a fact thoroughly established, easily confirmed,

very variable, but capable of being measured with precision, under every circumstance.—(2.) It occurs, whatever the distance of the object at which we look; and its amount, or the *visual angle* which it subtends, is independent of that distance. Hence, the *absolute breadth* that we ought to attribute to it, is—all things else being equal—proportional to the distance which exists, or seems to exist, between the object and the eye. —(3.) Irradiation increases with the brightness of the object, but not proportionally. If the law of its increase were represented by a curve, whose *abscissæ* represent increasing brightnesses, beginning at darkness or *zero*, and having for its *ordinates* the corresponding amount of Irradiation,—this curve would pass through the *origin* of the co-ordinates; have its concavity towards the axis of the abscissæ, and finally pass into an *asymptote* parallel to that axis. For a brightness equivalent to that of stars in a clear northern sky, the curve would be found very close to its *asymptote*. —(4.) When the space surrounding the object looked at, is not wholly dark, the irradiation belonging to the object is diminished; and when the illumination of the field of view approaches equality with the brightness of the object, the illusion attributable to Irradiation altogether vanishes. Hence two important practical consequences. When two objects of equal brightness *touch*, or can be made to appear to touch, irradiation is at *zero*, at their point of contact. And any two irradiations, occurring in neighbourhood diminish each other,—the diminution being the greater, in proportion as the edges of the luminous spaces are nearer each other.—(5.) Irradiation augments, the longer we contemplate the object; but as it depends very much on the state of the eye, or the impressibility of the *retina*, it varies considerably, even in the same individual, from day to day.—(6.) Irradiation is greatly modified when a lens is placed before the eye:—it is diminished by converging lenses, and augmented by divergent lenses. This action of lenses, seems to depend solely on their focal distances—not on their diameters or apparent curvature. The shorter the focal distance, the more distinct and decisive it is. The theoretical difficulty of the subject really lies in this action of lenses. What M. Plateau has established on the subject, is now to be explained:—

II. *Irradiation, as affecting Vision through Telescopes.*—(1.) The error produced in Astronomical observations, flowing out of what is called *Irradiation*, springs from two causes essentially distinct; viz., the ordinary ocular irradiation just described, and the *aberration* of the Instrument. —(2.) The part of the total error due to irradiation properly so called, depends on the magnifying power of the eye-piece, the brightness of the image, and the nature of the eye of the observer. It is greatly diminished by the action of the eye-piece, and that in proportion to its magnifying power, or to its *convergency* as a lens. But it will vary, of course, with the state of the observer's eye. It is also clear, that this portion of the total error must disappear when a double image Micrometer is made use of: it must, likewise, affect only slightly, observations with the Heliometer.—(3.) The other portion of the total error, that, viz., which originates in aberration of the Telescope, necessarily varies with the Instrument used, but must be constant for the same Telescope. One part of the error is thus constant, while the other varies with circumstances, and with the observer. Nevertheless, it is manifestly possible, even in the case of an imperfect instrument, and an eye very sensitive to irradiation, to obtain means of freeing observations from the effect of this peculiar error.

Isobarometric. The term itself indicates, equal barometric pressure. It is employed by Kaemtz to denote lines on the surface of the globe, connecting places that present the same mean difference between the monthly extremes of the Barometer:—Lines, or rather curves, whose geographical position and inflections yield important conclusions regarding the influence exercised by the form of the land and the distribution of the seas, on the oscillations of the atmosphere. Hindostan with its high mountain-chains and triangular peninsulas, and the eastern coasts of the New Continent, where the warm gulf stream turns to the east at the Newfoundland banks, exhibit greater isobarometric oscillations than the group of the Antilles and Western Europe. The prevailing winds exercise a principal influence on the diminution of the pressure of the atmosphere; and this is accompanied by an elevation of the mean level of the sea.

Isochimenal. A term applied to lines connecting places on the surface of the globe, at which there is an equal mean heat during winter. See ISOTHERMALS.

Isochronous. Vibrating or oscillatory movements performed in equal times are called isochronous; and the same isochronism belongs to that very remarkable property by which all systems that are in equilibrium, when disturbed by moderate but different forces, from that position, vibrate with oscillations performed in what is sensibly the same time. A demonstration of the general property cannot be well given here. It is to be found in the *Mecanique Analytique* of Lagrange, and depends on the processes of the higher calculus. The most ordinary and best known instance of the property is in the motion of the pendulum, which being set a-going either with a very great or a very slight velocity, will beat seconds quite regularly if of the proper length. Another excellent instance is detected in this,—when a note of a pianoforte is struck sharply or gently, the finest ear cannot discern any difference in the pitch. That depends on the time of the vibrations exerted, and they, therefore, are isochronous in the two cases, although the disturbance in the one case may be more than

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twenty times that in the other.—If there be isochronism—that is, if the larger space through which the disturbed body moves, be traversed in the same time as the smaller, it will be needful that the force in the first case have been greater than in the second, and not only so, but that it have borne the same ratio to the second that the first space did to the second. It is not difficult to prove that, for small distances of equilibrium, this is sensibly the case; and it follows, therefore, that the vibratory motions—before restitution to the original place is completed—must be isochronous.

Isoclinal: signifying *equal inclination.* A term applied to lines traced over the surface of our globe, connecting places at which a free and perfectly balanced Needle has, when magnetized, the same *inclination* or dip. See MAGNETISM TERRESTRIAL.

Isodynamic: signifying *equal force.* A term applied to lines connecting places on the surface of our globe, at which the total magnetic power of the Earth is the same. See MAGNETISM TERRESTRIAL.

Isogeothermal. A term applied to a set of lines, or rather curve-surfaces, within the Earth, supposed to connect surfaces equally affected by the internal or proper heat of our globe. Such surfaces are assuredly not yet determined. They are supposed to be indicated partly by the depths of hot springs, partly by very ambiguous theoretical considerations.

Isogonic. A term signifying *equal angles.* It is applied to those lines traced over the surface of the globe which connect places at which the deviations of the magnet, from the meridian or true north, are equal. The course of these curves over our globe is extremely simple and interesting, as well as the history of their changes. Every one of them, as Humboldt observes, has a *history.* The student is refered to MAGNETISM TERRESTRIAL.

Isometrical Projection. A species of projection on a single plane, whose fundamental condition is the following:—If three *equal* lines lie parallel respectively to three rectangular axes, the single plane must be so chosen that their projections on it be also *equal.* It is a mode of projection most valuable in the arts. See PROJECTION.

Isoperimeters. A branch of the Higher Geometry, treating of surfaces having equal perimeters, and of solids bounded by equal surfaces. The curious problems suggested by it contributed more perhaps than any other circumstance to originate the *Calculus of Variations.*

Isotheral. A term applied to lines connecting places on the surface of the globe, at which there is an equal mean summer heat. See ISOTHERMALS.

Isothermals or **Isothermal Lines.** Nearly forty years have elapsed since the illustrious and now veteran Humboldt published in the *Mémoires de la Societé d'Arcueil,* his famous dissertation on Isothermal Lines, and the distribution of heat over the surface of the Globe. Uniting, by lines, the points of equal annual mean temperature, on the east coast of America and the western coast of Europe, he prolonged these through the interior of the two continents as far as the state of observation at that epoch permitted him. Much has been done since the date of Humboldt's Memoirs, to enable the physicist to carry out this master-idea. The temperature of the polar regions has been determined; every ocean has been traversed by skilful navigators, and its relations to heat at the different seasons carefully scrutinized; and in the interior of the continents, meteorological Observatories, furnished with choice instruments, have been planted in abundance. The general map of the annual Isothermals, now presented to the reader, is the result of all these various investigations, as collected and methodized by Kaemtz, Berghaus, and Dové. On the left hand side, the degrees are marked according to the Centigrade scale, and on the right are the corresponding degrees, in the scale of Fahrenheit. It must be observed too, that the influence of *altitude* is eliminated from all these mean temperatures by aid of the well known law, which expresses the amount of that influence in any given latitude; so that the globe of which we are supposed to be treating, is not our irregular globe, but one with an even surface enveloping ours, at the mean level of the sea. The most cursory glance at this map cannot fail to impress one with a sense of its great importance, and of the profound interest that in every respect attaches to it. Speaking generally, the temperature diminishes as one passes from equator towards either pole; but how utterly independent withal, is any of these lines of mean temperature, of the mere parallel of latitude! No wonder, that the early American colonists were so cruelly disappointed with the new climate into which they had adventured. Albany, in the State of New York, is of the latitude of Rome; and yet, the Hudson which flows past it, is often frozen over for nearly ninety days in the course of a year! Look, also, at the North of Europe, and the South of Asia, the town of Iakoutzk has the same latitude as the Faroe Islands, while the difference of their temperatures amounts to 31° of Fahrenheit. What is the cause, then, of these remarkable anomalies? How does the distribution of land and water influence them? What is the influence of ocean currents? What, of the prevailing winds? &c., &c. Problems of highest interest in physics: but, before attempting to resolve them, let us attend to another all-important consideration.—The mean annual temperature of a place is only the rudest element of its climate. The effects of climate are mainly felt through the changes that occur during the year. Now, if, according to the happy conception so well wrought out by M. Dové, we lay down maps of

monthly Isothermals, we shall find that, although as the heat increases in either hemisphere, these lines have a direct and well pronounced motion towards the respective poles, they do not retain any approach to parallelism to the line of the annual mean. On the contrary, they entirely change their form, some portions of them pushing onwards with remarkable velocity, and many new sinuosities appearing. Maps 2 and 3, offer to the eye of the reader, M. Dové's determination of the isothermals of January and July; and a glance at the curves into which they are thrown, will suffice to bear out our assertion. Observe, for instance, the isothermals of July. In this month their modifications attain their maximum. In the long space passed over by the line of 30° Cent., or of 86° Fahr., a sinuosity is developed, within which the temperature rises to 32·5 Cent., or 90·5 Fahr. This comprehends Nubia and the south of Arabia, — "countries of which it is said by Hagi Ismael, that the earth is of fire, and the wind a flame.". Is it astonishing, under such circumstances, if the trade wind of the south-east—there called the monsoon of the south-west—drives back the trade wind of the north-east, even to the feet of the Himalaya; if elevated temperatures are found even in the north of the Asiatic continent; or if near Baganida, trees are found in the 72° of latitude, although the earth remains frozen a few feet beneath the surface? In the interior of the continent, the curves become *convex* towards the north,—Scotland and Ireland possess a marine climate, just as Labrador, Canada, Australia on the south and north, the coast of California, and on as far as the embouchure of the river Mackenzie. The centre of heat in the Gulf of Mexico presents by no means so high temperatures as Africa or India. At Maracaibo, it reaches 30° Cent., or 86° Fahr. The thermic equator there is only slightly bent towards the north, whereas, in the Eastern continent, it reaches in some places even the tropic of Cancer. In the polar regions—the entrance to the Icy Sea—the Straits of Lancaster and Behring—transform the isothermals near the poles almost into triangles. In the north of America, these lines are driven downward both east and west. In Europe and Asia, their convexities are transformed into concavities, and for the most part their directions are perpendicular to what they were in January. In the southern hemisphere, few of these irregularities appear. The lines are quite near each other, and almost straight between 1° and 15° S. latitude. Comparing the map of July with that of January, and glancing also at Dové's maps of the other months not given here, we may in general terms sum up the changes which the Isothermals undergo in the course of the year, as follows:—1. In Asia, these lines present the largest displacements in a northerly and southerly direction. Their summits, convex towards the pole in summer, become concave in winter.—2. In Europe, the isothermals present the most complex contortions.—3. In America, the concave summits pointing towards the poles, shift, between winter and summer, from the interior of the continent towards the Eastern coasts, and they resume their previous place towards the end of summer or autumn.—4. Europe has no extreme seasons.—5. Asia has cold winters and hot summers; America rigorous winters, a cold spring, a European summer, but a much more beautiful autumn.—If primary causes were alone taken into account, the extremes of heat and cold ought to be found in the middle of the continents and oceans respectively. It is not so. Sundry causes of great importance must therefore modify the action of the primary ones. What these are, will in so far appear from the remainder of this paper.—The mode in which M. Dové determined these monthly means, and so felt authorized to construct his maps, is explained under TEMPERATURE.

(1.) *The Temperature of the Pole*, and *of the Terrestrial Globe and its two Hemispheres.*—Before proceeding to take account of the secondary causes above alluded to, let us attempt to form a notion of the general distribution of temperature over the globe. In the map of January, as indeed in the others also, it will be seen that the fiercest colds rage in the north of Asia and America. If a polar projection were made of these regions for the month of January, it would be found that the two coldest spaces of these continents form a continuous band passing across the pole of the Earth. But the lines occupying these regions of the extreme north are so twisted and strange, that no formula, of a manageable nature, capable of expressing them, need hopefully be looked for:—happily in determining the temperature of an entire hemisphere, the condition of the other regions is of much more importance. Let us rather start from the torrid zone. In this zone, the formula which best expresses, in Centigrade degrees, the decrease of temperature in January as we pass from 0° to 30° N. lat., in the *Eastern Hemisphere*, is

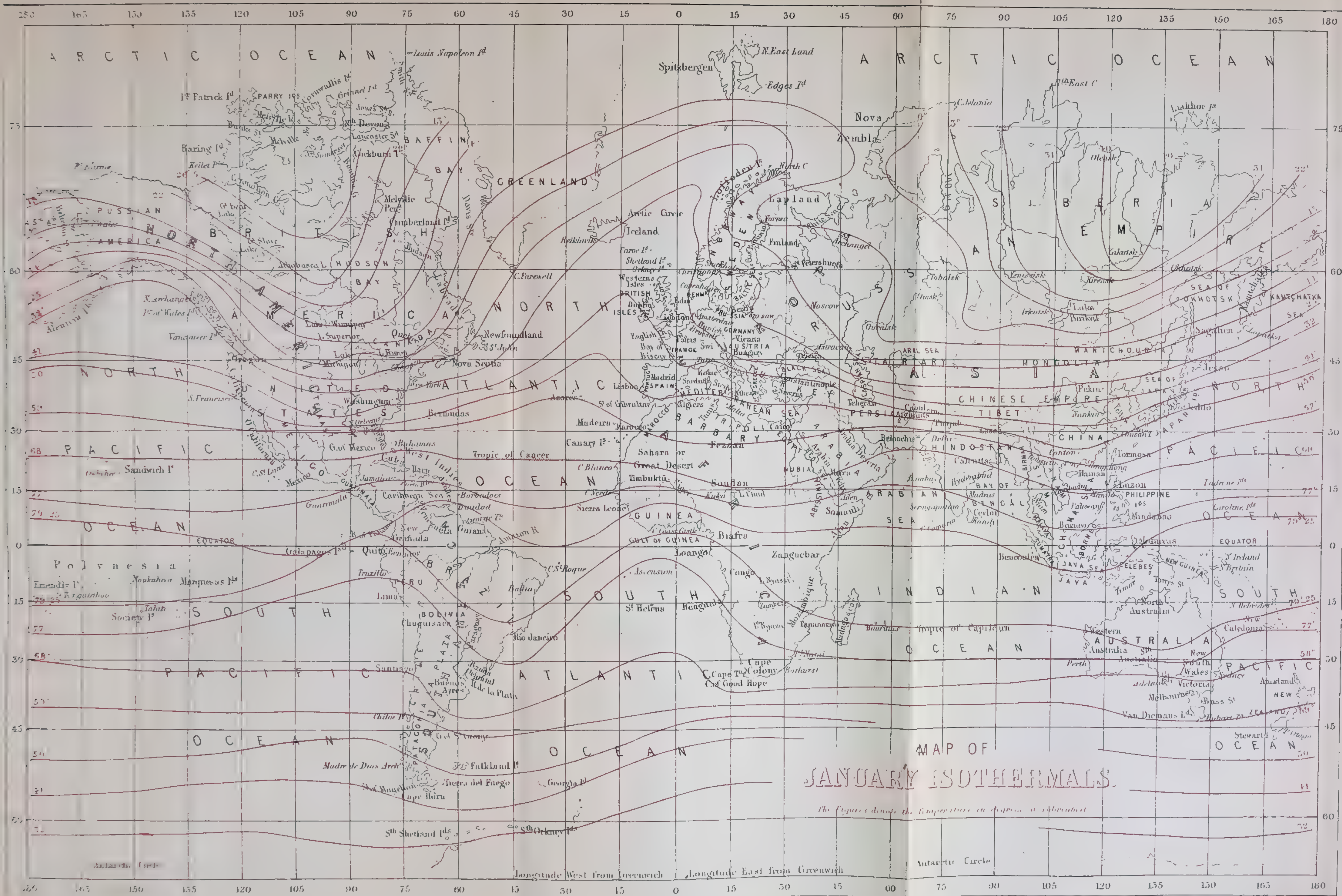
$$t_x = 26{\cdot}21 \cos. 2\,x.$$

where x is the latitude and t_x the temperature corresponding to it. In the Western Hemisphere between 0° and 40°, the approximate representative formula is

$$t_x = 27{\cdot}75 \cos. (2\,x - 7°).$$

No formula can be found that will embrace all degrees of latitude; and between 30° and 40° the errors of the foregoing are sufficiently palpable. The reason is simple. It is between these parallels that the Gulf Stream leaves the coast of America and makes for the Azores, and that in Asia we meet with the plateau which rises from the plains of the Ganges. Hence perturbations in the law of the decrease. The formula which best represents the case for *all* parallels is

$$t_x = -30\ 63 + 56{\cdot}88 \cos.^2 x;$$



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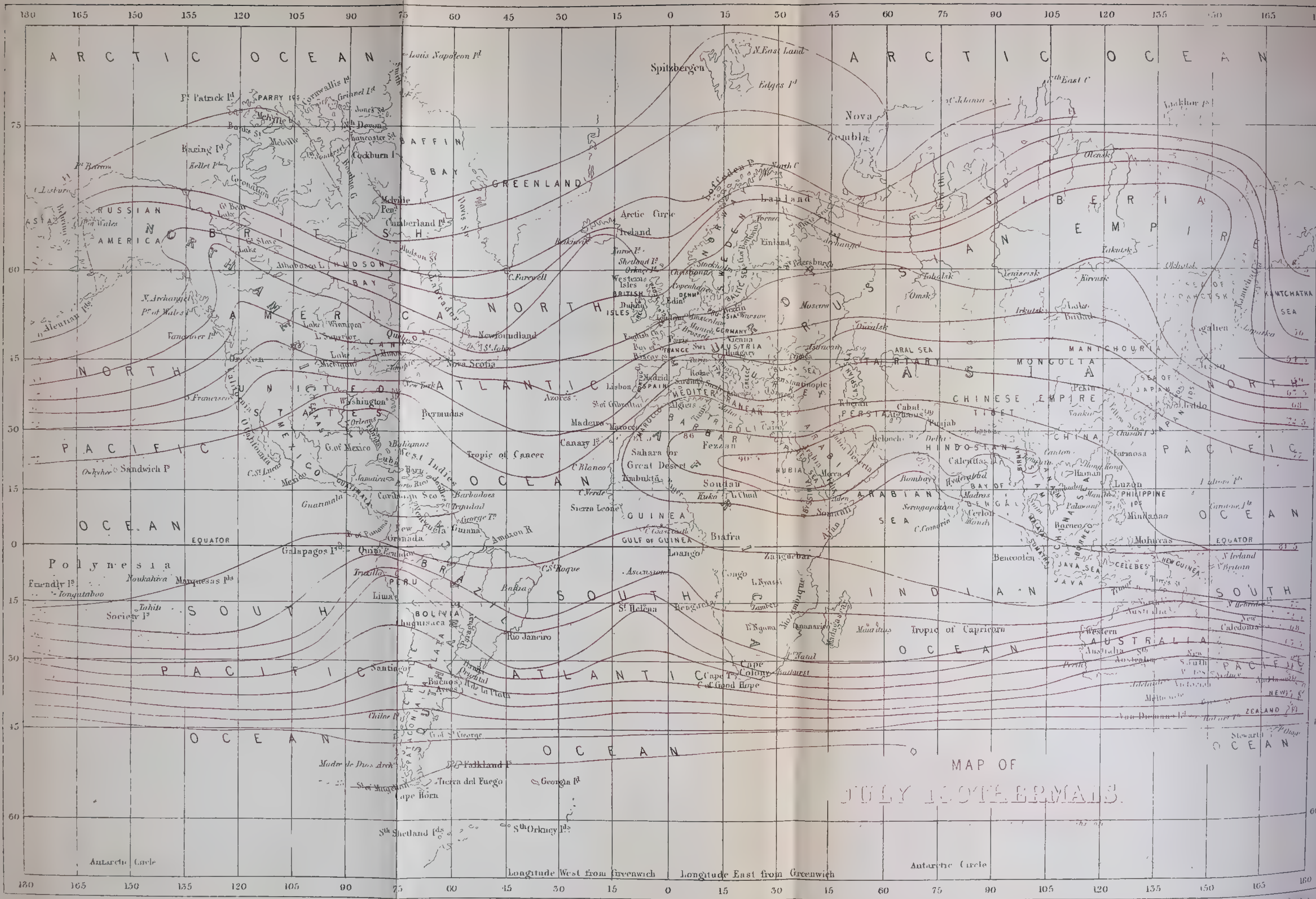

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nd, for low latitudes, the nearest formula is

$$t = -30\cdot0 + 56\cdot25 \cos.^2 x.$$

ccording to this the temperature of the pole, or hen $x = 90°$—would be

$$t_{90} = -30°\cdot6 \text{ Cent.} = -23°\cdot1 \text{ Fahr.}$$

or the Eastern Hemisphere, south of the Equar, and as far as 40° S. lat., the following formula approximative:—

$$t_x = -6\cdot25 + 32\cdot75 \cos.^2 (x - 5°).$$

ové, by applying these formulæ, which he conlers, with all their errors, preferable to the usual ethod, obtained the following results:—

JANUARY,—Northern Hemisphere	49°·3 Fahr.
Southern „	59°·5
Entire Globe,	54°·4 Fahr.
JULY,—Northern Hemisphere,	70°·88 Fahr.
Southern „	53·6
Entire Globe,	62°·24 Fahr.

The temperature of the Earth thus diminishes out 8° Fahr. or 4°·5 Cent. from January to ly. If the mean temperature of the Earth be ken as the mean of the temperature of these two treme months, it is 14°·5 Cent., or 60°·1 Fahr.; r the Southern 13°·6 Cent., or 58°·32 Fahr. For e Northern Hemisphere it is 15°·5 Cent., or 56°·5 hr. The difference of the temperature of the mispheres and of the variations indicated above referable to the great primary cause—their olly different relations to land and water. t how many other complications are involved these differences? To how many modificans in the general system of winds must they e rise,—how greatly must they affect the disbution and well-being of animal and vegetable !

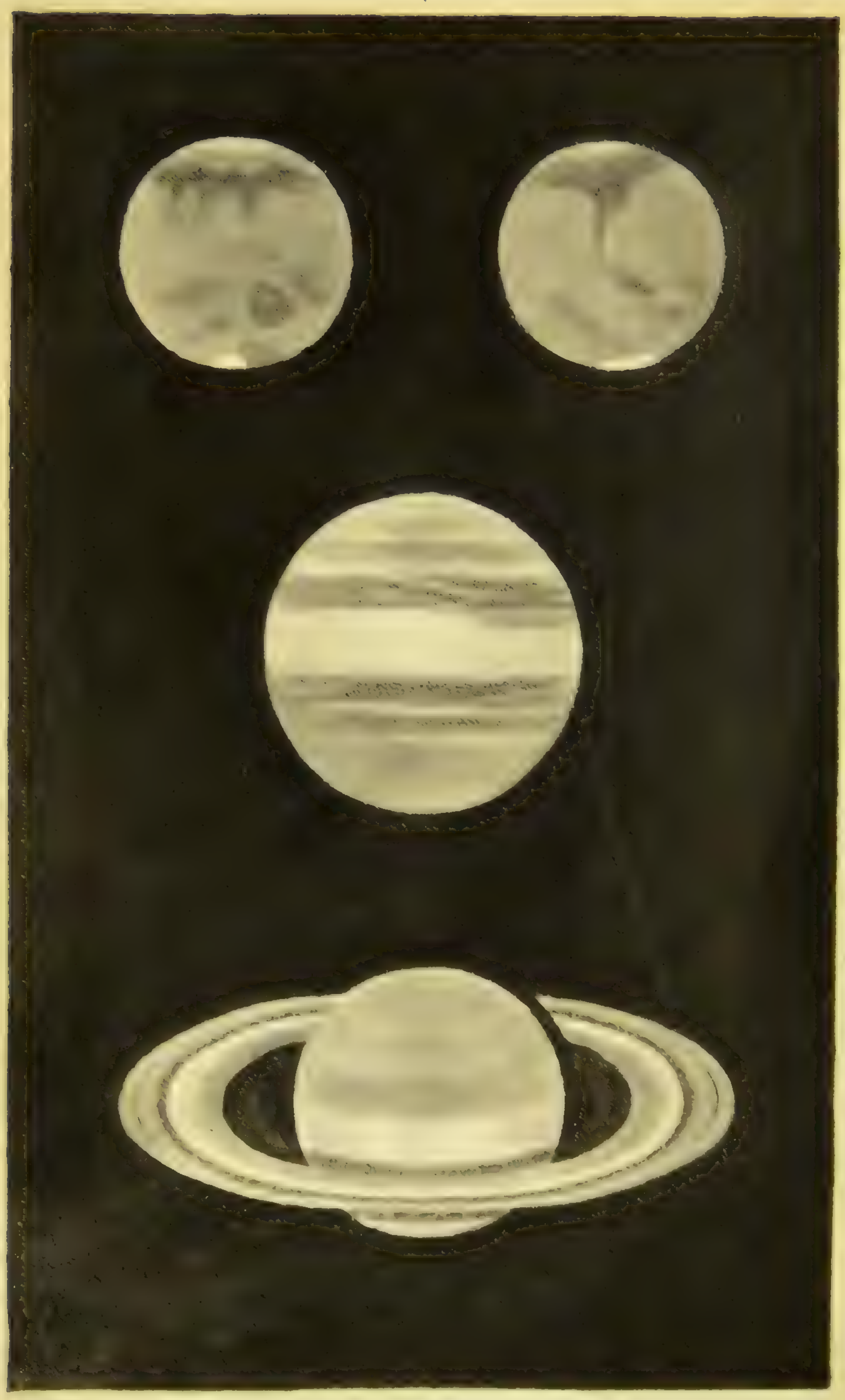
(2.) *The Influence of Oceanic Currents on the thermals.*—The calorific influence of an oceanic rent necessarily depends on the difference of at existing between the coasts it leaves and se at which it arrives. The great Equatorial rent from East to West, has no perturbing ion whatever: influence of this sort can be exted only from those which flow from Equator to le, and *vice versa.* Of these, the two chief ones the Gulf Stream, carrying Equatorial heats thward in the Atlantic, and the current on the uvian coast, discovered by Humboldt in 1802, ich sweeps towards the Equator the cold ters of the Antarctic Ocean. Both of these st the Isothermal lines, and in the same direc; giving them sinuosities that push northrds. It is in the southern atmosphere where southern ices melt in greatest quantity, that influence of the Peruvian current is mainly ; but the mass of waters moving northwards so large, that there are only slight traces of erences owing to the seasons. The northern f of the Pacific Ocean and the southern half the Atlantic present phenomena strikingly analogous to the great Antarctic current. In the southern Atlantic a current arising at the Cape of Good Hope, moves along the African coast towards the Equator, then it traverses that ocean from east to west, following the confines of Brazil from Cape St. Roque, and flowing from north-east to south-west. In the North Atlantic also, there is a current running opposite to the Gulf Stream from Greenland and Hudson's Bay, but this latter acts chiefly in spring. This is joined by another, whose origin apparently lies in the melting of masses of ice carried to the north of Siberia by the unbound rivers of that terrible region, and which gives rise in the first place to what Rennel called the *Stream Current.* This divides into two currents, one of which flows west to Spitzbergen, which turns it to the south in the direction of Greenland; it passes between Greenland and Iceland as far as Cape Farewell. At that Cape it meets the former current,—the one which penetrates from the Icy Sea into Baffin's Bay, through the Strait of the *Fury* and *Hecla*—issuing through *Davis' Straits.* Thus we find on the banks of Newfoundland icebergs brought down by these two currents; but these rapidly melt on meeting the waters of the Gulf Stream.—From these facts it results that the Isothermal Lines which always push forward towards the pole in proportion as the declination of the sun increases, are stopped in their advance along the eastern coast of America. The concave summits of December and January shrink back in the centre of America; and, after June, they are displaced towards the eastern coast. The north of America,—especially the neighbourhood of Hudson's and Baffin's Bays—is the country of cold springs. There vegetation is of the poorest, because the masses of ice absorb, in melting, the heat which should cherish the increase and multiplication of plants. An unhappy conjunction of circumstances neutralizes the beneficent influence of the sun. For instance, the missionaries at Okak inform us that on 1st May, 1837, their garden was covered with snow to the depth of from five to eight yards, and in *August* it snowed anew! These few examples may suffice to show the influence of Oceanic Currents in impressing irregularity on the American Isothermals.

(3.) *Influence of Aerial Currents or Winds on the Isothermals.*—It will be noticed that the influence of the Oceanic currents, as traced in last section, is mainly visible in the Western Hemisphere,—a fact naturally issuing from the narrow and elongated character of the new continent. The eastern half of the globe has a totally different configuration. It cannot from its very nature be much influenced by the waters that wash its coasts; and it is here accordingly that we trace mainly the agency of Winds in modifying temperature.—The plateaux and chains of mountains in the Eastern continent, lying for the most part from East to West, are on that account barriers against the winds that chiefly influence

temperature, viz., the Northerly and Southerly streams. The central plateau of Asia, acting in this manner, prevents the hot winds of India from penetrating into Siberia, and the cool winds of the North from refreshing during summer the burning plains watered by the Ganges: hence the terrible winters of Siberia, and scorching summers of India. These latter, indeed, do to some extent influence the summers of Siberia, which in some places are very warm.—Why, however, do the isothermals, with concavities towards the pole, fall in winter near to its eastern coast, and not in the centre of the Asiatic continent? In other words, why has Europe—the western side of the old World—so high a temperature in winter? The temperature of the winter in our latitudes is so dependent on the direction of the winds, that the cause of the phenomenon in question must be sought for there alone. The predominance of moist and hot, over dry and cold winds, is the cause of the mildness of the European winters. Two propositions establish this:—*First*, Nowhere are moist and hot winds so predominant; and, *secondly*, Nowhere, even when they prevail, do they raise the temperature so much as they do in Europe. This subject cannot be understood without regard to the changes impressed on aerial currents by the rotation of the Earth. The air which rises at the Equator is animated by a rotation much more rapid than that of higher latitudes, hence a change in its direction as it flows northwards or southwards towards the temperate zones. In our Northern zone, the south winds gradually verge towards west winds, and north winds towards the east. The air, therefore, that rises in Africa, blows rather over Asia than over Europe. The cradle of our winds is not in Sahara, but in America. Now, the air which rises under the tropics, and descends to the Earth's surface in higher latitudes, warms them chiefly by getting rid of the latent heat that retained its moisture in the state of vapour. Europe is thus the *condenser* of the Caribbean Sea. The Andes and the rocky mountains arresting the snow, all the vapours of the Pacific Ocean precipitate themselves in the narrow slip of land on the west of these chains. Hence the vast difference of the influence of these apparently corresponding winds in Europe and the greater part of the continent of America,—that, viz., lying to the east of its central chains of mountains.

(4.) *Causes of the intense Cold of Northern America.*—Besides the general phenomena, already partially explained, there starts up the problem as to the fearful rigour of the winters in the northern portions of North America. Why are immense rivers annually frozen in a continent whose coasts are washed by the Gulf Stream? Why have the United States—narrow as that continent is, and, of course, with every point in it near the sea—a continental climate in winter, and a marine climate in summer?—The great lakes of the St. Lawrence cover a space of 94,000 square miles, and other lakes besides, form a continuous chain between Hudson's Bay and the Rocky Mountains, as far as the Arctic Sea. In these masses of fresh water, the cooled layer at the surface always descends, and the lowering of temperature through effect of evaporation takes place exactly as in the sea; but, in the sea, the cold water on reaching the bottom *flows towards warmer regions;* while no such current, removing the cold layer, can take place in enclosed lakes. Fresh water also has its maximum density at 39°·2 Fahr., while the sea freezes less easily on account of the tides that agitate it. The Northern regions of America, with its frozen lakes, is, therefore, a *continental mass during winter;* while, in *summer*, its surface *is divided between land and water.* Hence the contrast. If it is asked, further, why the lakes freeze so soon, why the Hudson, under the latitude of Rome, is bound up from the 15th of December? the reply is, that in Europe south-west winds dominate in winter, and are replaced in spring and summer by northerly ones; while in America the mean direction is north-west in winter, and south-west in summer.

(5.) *Causes of the predominance of Southerly Winds in Europe.*—It is well known that there are two grand opposing aerial currents in our Atmosphere—one from Equator to Pole, and the other from Pole to Equator. These, modified in direction by the rotation of the Earth, originate our main winds. See WINDS. But the air which returns from Pole to Equator is the same as that which flows from Equator to Pole, only it has lost its vapour, and is much colder, and therefore occupies a greatly diminished volume. These counter currents often change their beds; but any point of Europe will be found much more frequently within the course of the large equatorial current, than in that of the comparatively narrow polar one. Hence the predominance of southerly over northerly winds.—To establish the relative frequency of these currents and their reciprocal influence in the course of the year, is now one of the most urgent necessities of meteorology. Dové's maps of the monthly isothermals is a most important contribution towards that end.—Other and more special causes act also upon Europe. The high summer temperature of the Asiatic continent, gives rise to a powerful special ascending current, which diminishes the atmospheric pressure over it, and thereby makes that current a sort of centre of attraction to all neighbouring masses of air. Then the south-east trade wind is driven back and becomes the south-west monsoon. The north-west, as far as the Himalaya and Europe, are, through the same action, visited by cold north-west winds that cool so disagreeably the atmosphere in summer. At the same time east winds prevail on the East coast of Asia, north winds on the coasts of the Frozen Ocean, and the mass of air that rises from Asia, flows away laterally and accumulates over the convex



ummits of the Isothermals. This is the fact in-icated by the curve of Barometrical pressure of he dry air at Sitcka, where the maximum is in ummer; while in countries, with cold spring mes (the arctic region of North America), the aximum of pressure takes place at that season.—uch, then, is something of the periodic changes at affect the isothermals, and whose combination gives rise to the *annual* curves. The student, who is curious regarding this most interesting subject, is referred to the original memoirs of M. Dové, from whose immense repertory the foregoing paper is abstracted.

Isotropic Solid. An *Amorphous* Solid, or one in which the action of the Elastic forces is alike in all directions. See ELASTICITY, § 19.

J

Juno. One of the Asteroids (*q.v.*) discovered y Professor Harding of Göttingen in 1804. or its Elements, see ASTEROIDS.

Jupiter. The largest and most superb planet our solar system. The diameter of this mag-ficent orb, is 92,164 miles, that of the earth ing only 7,926 miles. Its *volume* is therefore 491 times that of the earth; but as its density only ·227, or considerably less than one-fourth e density of the earth, its *mass* is only 338·718 mes greater than the mass of our globe. ravity, at the surface of Jupiter, is nearly $2\frac{1}{2}$ nes greater than the same force at the earth's rface; in other words, while with us, a heavy dy falls through 16·1 feet per second, the same dy would fall, in the same time, at the surface of r companion planet through 39·4 feet. Jupiter's stance from the Sun, varies on account of the centricity of its orbit, from 5·453663, to 951871,—the distance of the earth being unity. e mean distance is 5·202767. The period of sidereal revolution is 4332·5848032 days. e period of his rotation on his axis, only 9 hours, minutes, 26 seconds; so that it is not won-ful that this planet is singularly oblate or ttened at the poles. The compression of the rth is known to be only about $\frac{1}{299}$th part of its uatorial diameter; but the compression of piter is as much as $\frac{1}{17}$th.—It is, of course, to expected that a planet so majestic, should, ough the influence of its gravitation, largely luence the other orbs of this system, and espe-lly those in its immediate neighbourhood. e immense spaces separating Jupiter from most his companion planets, diminish in so far, influence of his great magnitude; although questionably, that influence must extend to m all. One of the most interesting of his cts in this way, is that *long inequality*, as it echnically termed, which exists through the tual relations of this planet and *Saturn*. If, every revolution, or every few revolutions, two planets were to resume their original itions and distances relative to each other, ir mutual perturbations would recur, or go ough their rounds, in comparatively brief cycles. s, however, is nowhere the case, although, in e instances there is an approach to it. In case of Jupiter and Saturn, five periods or olutions of the former are nearly equal to two he latter,—the former taking place in 21,663 days, and the latter in 21,519 days. The perturbations in the main, therefore, recur in the course of very nearly 60 years. But the recurrence is not exact; and this inexactitude gives rise to a superadded or supplementary inequality always modifying the perturbations just mentioned, which also runs its course. The difference between the two foregoing numbers is only 146 days; nevertheless, in that time, Jupiter describes 12°, while Saturn describes only 5°; so that, instead of being in actual conjunction, they are 7° apart. The question is, then, seeing that they separate thus far in about 60 years, how long will they require to return to their original relative position? The period of the long inequality is determined by the reply to this question, in conjunction with other considerations on which we cannot now enter. Its true period is about 918 years.—The influence of this great planet on MARS will be referred to in our notice of Mars.

(1). *Jupiter, Physical aspects of.*— On the surface of Jupiter are several transverse bands parallel to his equator, which is almost parallel to the ecliptic; and darkish spots are also discoverable on his surface. See fig. 1. Herschel attributed

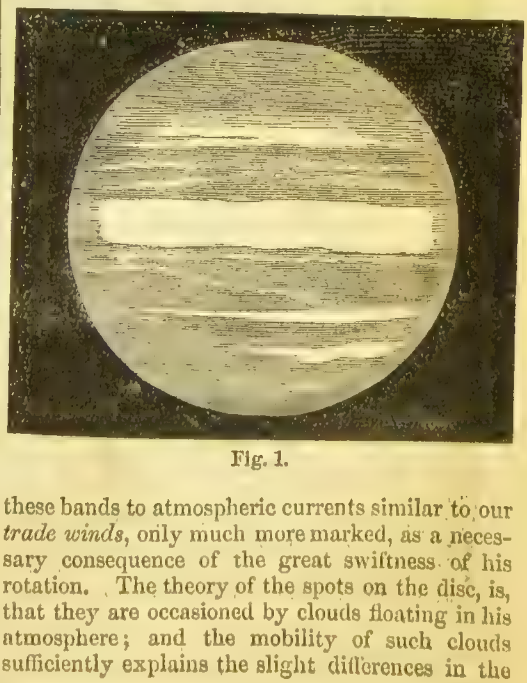


Fig. 1.

these bands to atmospheric currents similar to our *trade winds*, only much more marked, as a necessary consequence of the great swiftness of his rotation. The theory of the spots on the disc, is, that they are occasioned by clouds floating in his atmosphere; and the mobility of such clouds sufficiently explains the slight differences in the

values given for the period of his rotation.—Owing to the fact, that his polar axis is almost perpendicular to the plane of his orbit, there cannot be any appreciable *seasons* in Jupiter, and of course none of those peculiar changes that so diversify and beautify the existence of the Earth.

(2). *Jupiter, Satellites of.*—One of the earliest discoveries of Galileo, after his inestimable invention of the telescope, was the four "Medicean Stars," or the four Satellites of Jupiter. The following table exhibits their mean distances from the centre of the planet, and their periods of revolution. It will be seen that the relations of their distances and periods are exactly expressed by the third law of Kepler:—

	Mean Distances.	Duration of their Revolutions.
1st Satellite	6·05	1 77
2d Satellite.........	9·62	3·55
3d Satellite...........	15·35	7·15
4th Satellite	27·00	16·69

The subjoined figure, drawn according to exact proportion, will represent their relations to the eye. On the same scale, the distance of Jupiter from the Sun would be 28 English feet—the volume of the Sun himself, nearly filling up the space within the orbit of the second Satellite. The eccentricities of the orbits of the two first Satellites are scarcely perceptible, those of the third and fourth exceedingly slight. The planes within which they move, form very small angles with the plane of Jupiter's orbit, on which account they are generally seen arranged nearly in a straight line in the direction of the planet's equator. The inclination of the third Satellite is the largest, and that is only 5′ 3″ from Jupiter's equator; the inclination of the orbit of the second, is 1′ 6″; that of the fourth, 24″, while that of the first, is only 7″.—The perturbations of this interesting system, were early investigated by Laplace; whose Memoir on the subject was one of his first titles to distinction. They have since occupied other distinguished astronomers. They are very curious—especially the relations of the first three. As the foregoing table shows, the relations of the periods of revolutions of these three are peculiar and most simple. The period of the second is very nearly double the period of the first, and the period of the third is also almost exactly double that of the second. From this peculiarity, and the further fact, that the slight differences between these two ratios, and the actual numbers, are likewise related in a very simple way, the following two laws controlling their mutual perturbations, arise:—I. The regression of the line of conjunction of the second and third Satellites is exactly as rapid as the regression of the line of conjunction of the first and second Satellites. And—II. The line of conjunction of the second and third Satellites always coincides with the line of conjunction of the first and second Satellites produced backwards; the conjunction of the second and third Satellites always taking place on the side opposite to that on which the conjunctions of the first and second take place. The student versed in the calculus, will, of course, consult the original chapter in the *Mecanique Celeste.* These bodies are all somewhat larger than our moon: the largest—the third—is 3,573 miles in diameter.

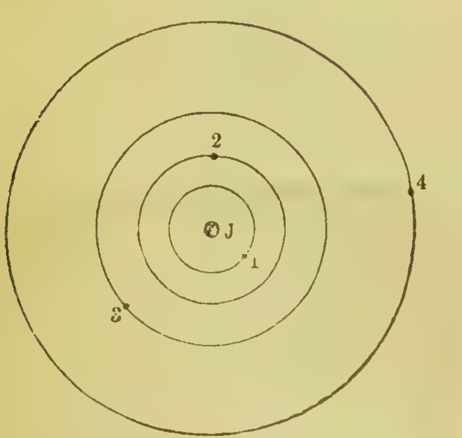


Fig. 2.

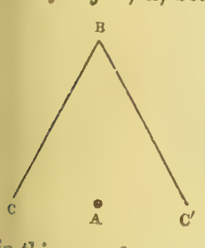
(3.) *Jupiter's Satellites, Eclipses or Occultations of.*—Owing to the fact, that the planes of the orbits of these bodies are nearly coincident with the plane of the planet's orbit, they must suffer eclipse on every revolution; and for the same reason, we must be able on every revolution to see them passing across Jupiter's disc. These phenomena were at one time employed to determine terrestrial longitude (see LONGITUDE), but the practice has long been superseded by the use of much more precise methods. It demands, however, to be mentioned, that it was by aid of the eclipses of the Satellites, that the acuteness of Rœmer first detected the *progressive* character of the propagation of light, and its actual velocity. Occupying himself with the theoretical prediction of these eclipses, he found that the actual occurrence never took place at the calculated time—*it was always* LATER. Still further, this apparent erro varied with the position of the Observer, or of th Earth. When our globe is between the Su and Jupiter, and of course nearest Jupiter, th error is not so great as in the opposite cas viz., when the earth is farthest from Jupiter, on the opposite side of the Sun. And the apparent errors are not capricious. They o cur regularly and by regular quantities; ar this clearly indicates that their origin lies some permanent and regular cause. In a hap moment, Rœmer hit on the conjecture, that *do not see the eclipse when it does take place,* b some time afterwards,—in other words, that *li occupies a certain definite time in travelling.* T idea started, the calculation of the velocity w easily made. Taking that velocity in round nu

bers, or 198,000 miles in one second of time, the Astronomer found that all discrepancies vanished; and from that moment, a new and most important truth entered the domain of Physics. It will be seen in other articles in this dictionary that our whole power to explain the phenomena of Light depends on our possession of this truth. —For discussion of the fact itself, see LIGHT, VELOCITY OF.—It cannot fairly be alleged that we are acquainted experimentally with the velocity of this great agent in the interstellar spaces; but it is taken for granted in all physical Inquiry, that it travels through those remotenesses also, with the same marvellous speed. Rœmer's discovery is thus held to be a cosmical one.

K

Kaleidophon. A very pretty device of Professor Wheatstone's, by which the eye can be made to distinguish the *transversal* vibrations of an elastic thin plate, fixed at one of its extremities. Let the plate carry at its free end, a ball of polished metal, or a hollow glass, silvered within; and let the experiment be made in circumstances calculated to effect, by this ball, the reflection of Light in the form of a brilliant point. When the thin plate is put in vibration, in any way, this fine point of Light appears to describe various curves, corresponding with the musical notes produced by the vibrations.

Kaleidoscope. A beautiful instrument—one of the happiest inventions of Sir David Brewster. It is well known that if two mirrors hang on walls exactly opposite each other, the image of any object between them—of a lamp for example —is repeated in each, an indefinite number of times; forming, if the mirrors are perfectly parallel, a sort of indefinite or vanishing alley of lights. If the mirrors, instead of being parallel and distant from each other, were inclined as below, the image of any object, A, between them, would likewise be repeated, but instead of forming an alley, its images would be disposed around the circumference of a circle whose centre is B, and radius B C, or B C′. Sir David Brewster happily imagined that by the right disposition of two mirrors in this way, figurate objects placed between C and C′ might be made produce symmetrical forms of great beauty and infinite variety; and by fixing two oblong mirrors at their edges as at B, and making other suitable arrangements, he arrived at his famous Kaleidoscope. This instrument, which, in forms more or less rude, found its way into the hands of every one, is so well known that further general description is unnecessary. It is only justice, however, to the eminent inventor to state, that the Kaleidoscopes produced under his patent, must in nowise be confounded with the rough imitations of them that so rapidly spread abroad. The latter were toys. Brewster's Kaleidoscope is an instrument perfected by science. As in every similar circumstance in which he has been concerned, Sir David rigorously investigated and applied all those scientific conditions to which his invention should be subjected, so that it produce perfect symmetry. The following are the leading conditions:—(1.) The angle at which the reflectors are placed must always be a submultiple of 360°.—(2.) When the object whose images are to be viewed is, in all its parts, symmetrically situated with regard to both mirrors, this submultiple may be either *even* or *odd;* but if the object is irregular, and irregularly placed, it ought to be an *even* submultiple.—(3.) There are only two positions in which the object can be placed so that its images form a perfect symmetry. It must either be between the ends of the mirrors, or in contact with these ends.—(4.) The eye must be as near as possible to the angular point.—Sir David added to some of his kaleidoscopes an object-glass or lens; thereby giving the observer the power to view through it, any external or natural object. The instrument was of great use, and at one time of wide application, in all Arts of Design.



Kepler's Laws. Three Laws, or expressions of the Order of the planetary motions, discovered by John Kepler. They are these:—

First. The orbit of every planetary body is an *Ellipse*, in one of whose foci, the Sun is situated.

Second. The *radius vector*, of each planet, sweeps, in equal times, over equal areas. See AREAS.

Third. The *squares* of the times of Revolution of two planets, are in the ratio of the *cubes* of their mean Distances from the Sun, or from the foci of the Ellipses in which they move.

These most important Laws were elaborated by Kepler out of the mass of separate observations—those especially on the planet *Mars*—left by Tycho Brahe. No more instructive lesson, in inductive philosophy, can well be found, than his own narrative of the mode in which he discovered them. We have told, in article GRAVITATION, what is their place and importance, as means towards the discovery of the grand Law of the Material Universe, and as descriptive of its exact Order.—It was unfortunately not the privilege of Kepler, to advance beyond these Laws, or to ascend to the great Physical Theory afterwards unfolded. He was indeed the true discoverer of the *Law of Inertia*, but could not appreciate its bearings on *Celestial Mechanics*. He clung to a doctrine of Vortices; nor would it have been easy for him to devise a more competent Theory.

Lacerta. One of the constellations marked by Hevelius. It is surrounded by Andromeda, Cepheus, Cygnus, and Pegasus. Its largest star, *g* Lacertæ, is of the fourth magnitude.

Lamp. A very essential part of an experimentalist's apparatus. The prime quality of a lamp is the steadiness of its light, and this depends on the uniformity with which the wick is wetted by the oil or other combustible. The first effort to secure this, was by the ring-lamp, in which the oil was contained in a ring of a foot or so in diameter, surrounding the burner, and on a level with the top of the wick. But the level of the oil in the ring sank as combustion proceeded, and the issue was, carbonization of the wick, smoke, and diminished light. This could only be avoided by the troublesome process of continued replenishing. Next we had *Carcel's* lamp, in which the oil is regularly pumped up to a certain level from a reservoir below, by two small pumps kept in motion by clock-work, or by force of a mainspring. These lamps have been great favourites on the Continent, and were made highly ornamental; but their expense, and the inconvenience arising from the necessity of cleaning their mechanism, kept them out of general use. The *Moderator* lamp, quite a recent invention, promises to displace *Carcel's*. In this, as in Carcel's, the reservoir for the oil is below, and it is forced up to the required level by the action of a spiral spring, whose tension requires to be renewed, or the spring wound up, once in twelve hours. They possess all the virtues of the Carcel lamp. They are simpler, cheaper, and free from the more serious inconveniences attaching to their precursors.

Lamp, Monochromatic. A lamp whose flame yields rays of some one homogeneous light. It is of greatest importance in optical experiments to have the power of using light of a definite refrangibility. But the object is difficult of attainment unless by the process of first separating the rays by the prism, and then selecting the special ray we desire. Sir David Brewster long ago proposed to substitute lamps with coloured flames. Here, too, the difficulty is great, as, however strong the colour, the ray seldom manifests absolute homogeneity under the test of the prism. The distinguished philosopher just named obtained a homogeneous *yellow* light, from a lamp fed by spirits of wine diluted with water and heated: and it has been found since that if some of the muriates are thrown in powder on the wick of a spirit lamp the same effect is produced. By using common salt or muriate of soda, in this way, Mr. Talbot obtained a very copious supply of pure or absolutely homogeneous yellow light.

Lamp, Safety. A fine invention of Sir H. Davy's, with the object of lessening danger of explosion in mines. It rests on the principle that flame, to ignite adequate combustible gases, will not pass through fine wire gauze; although the *light* of the flame easily passes through it. If used with ordinary care, this lamp is quite effective for its purpose.

Lantern, Magic. A very simple and ingenious philosophical instrument depending on dioptrical principles. It is sometimes employed for scientific purposes; most commonly as a mere toy. It consists of a box of cubical form, within which a lamp is set. The box has a hole in the middle of one of its sides, where a lens is inserted, as in the ordinary camera obscura, with a vacant space between the glasses of the lens and the end nearest the box, where a slide can be inserted. The lamp is placed so that its light shall be on a level with this hole in the box. The lens consists of an arrangement of two or three glasses capable of scattering rays, which go through them from the box, according to the ordinary dioptrical properties of Lenses (*q. v.*) The slide has coloured figures, the colours being as transparent as possible; and when it is inserted the rays from the lamp passing through the glass slide go on to the lenticular arrangement through which also they pass. If the room be dark, the black wall opposite the mouth will be lighted up by the radiance from the lamp coming through the slide and lens, and the figured glass from its colour absorbing more light than the plain glass beside it, the light which has passed through it will be less, and dark outlines will thus be thrown upon the wall.

Latitude. The latitude of a place is equivalent to the elevation of the pole above the horizon, which altitude could be easily determined were the pole a visible point. But as there is no star exactly at the pole, its position must be determined by observations of stars at a distance from it. There are seven methods of determining this essential element.

(1.) *By Transits of a Circumpolar Star both Above and Below the Pole.*—The best method of determining the latitude of a place, so as to be independent of the declination of the star observed, and also as free as possible from the errors of refraction, is by observations of a circumpolar star at the time of its upper and lower culminations. These observations may be made by means of a mural circle, or any graduated circle.—Let H Z P O represent a meridian, H O the horizon of the place of observation, P the place of the pole, and A B C D the circle described by a circumpolar star in its diurnal motion. The elevation of the pole

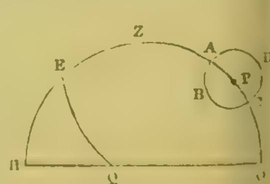


Fig. 1.

P O is equal to half the sum of A O and C O,

corrected for refraction.—Let A and A′ represent the altitudes of a circumpolar star at its upper and lower culminations; also, let r and r' be the refractions corresponding to these altitudes; then

$$\phi = \tfrac{1}{2}(A + A' - r - r'),$$

both altitudes being measured from the north horizon.—If zenith distances instead of altitudes are observed, the co-latitude will be,

$$\psi = 90° - \phi = \tfrac{1}{2}(z + z' + r + r').$$

The refraction is derived from the usual tables.

(2.) *By Simple Meridian Altitudes.*—Let P Z H represent the meridian of the place of observation, H O the horizon, Z the zenith, P the place of the pole, E Q the equator, S or S′ a star on the meridian, S E or S′E its declination (δ), S P or S′ P its distance from the pole (d), which is the complement of δ; the arc E H is the complement of the latitude (ϕ), or $90° - \phi$.—We measure the altitude (A) of the object S or S′, or its zenith distance (z), and correct it for refraction and parallax, if the parallax is appreciable. Then it is evident that

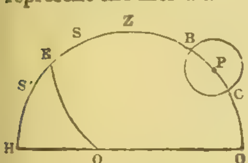


Fig. 2.

$$EH = SH - SE = S'H + S'E.$$

These two equations are included in the same expression by regarding the declination negative when it is south of the equator. Thus,

$$90° - \phi = A - \delta,$$

or $$\phi = 90° + \delta - A.$$

But $$z = 90° - A.$$

Hence $$\phi = \delta + z,$$

for stars which culminate south of the zenith, where δ must have the negative sign when the declination is south.—If the star passes the meridian between the north pole and the zenith, as, for example, at B, then we shall have

$$PO = BO - BP;$$

that is, $$\phi = A - d.$$

But $A = 90° - z$, and $d = 90° - \delta$.

Hence $$\phi = \delta - z.$$

If the star passes the meridian below the north pole, then we shall have

$$PO = CO + PC;$$

that is, $\phi = A + d = 180° - \delta - z.$

Hence we shall have

$\phi = \delta + z$ if the observations be made to the south;

$\phi = \delta - z$ if to the north, *above* the pole;

$\phi = 180° - (\delta + z)$ if to the north, *below* the pole.

(3.) *By Circum-meridian Altitudes.*—The preceding method gives but one value of the latitude, because the star can only be observed at the instant when it crosses the meridian. But where the observer is furnished with an altitude and azimuth instrument, a repeating circle or sextant, he may render any number of observations made on each side of the meridian, and at a short distance from it, equal in accuracy to those which are made at the moment of culmination. For this purpose, he must know the distance (in time) of the star from the meridian at the instant of each observation, and he can compute the correction which ought to be applied to the zenith distance observed.—Let P be the pole, Z the zenith of the place of observation, P Z M a meridian, S a star near to the meridian, M the point where this star crosses the meridian, and P S an hour circle passing through the star. Suppose the zenith distance, Z S, of the star has been measured, and corrected for refraction, and also for parallax, when the sun or a planet has been observed; it is required to compute the zenith distance, Z M, of the star when on the meridian. Now from the figure we perceive that

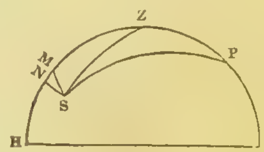


Fig. 3.

$$PS = 90° - \delta,$$

$$PZ = 90° - \phi,$$

$$ZM = PM - PZ = \phi - \delta = (z),$$

the zenith distance of the star on the meridian.—With Z as a centre, describe the arc S N, and the point N will be at the same altitude as S. It is required to compute $MN = x$, the quantity which the star must rise from S, before it reaches the meridian.

$$\cos. a = \cos. b \cos. c + \sin. b \sin. c \cos. A.$$

But $$\cos. A = 1 - 2 \sin.^2 \tfrac{1}{2} A.$$

Hence

$$\cos. a = \cos. b \cos. c + \sin. b \sin. c - 2 \sin. b \sin. c \sin.^2 \tfrac{1}{2} A.$$

Also,

$$\cos. b \cos. c + \sin. b \sin c = \cos. (b - c).$$

Hence

$$\cos. a = \cos. (b - c) - 2 \sin. b \sin. c \sin.^2 \tfrac{1}{2} A.$$

Applying this formula to the triangle P Z S, and representing the angle Z P S by P, we have

$$\cos. ZS = \cos. (PS - PZ) - 2 \sin. PZ \sin. PS \sin.^2 \tfrac{1}{2} P$$
$$= \cos. z - 2 \cos. \phi \cos. \delta \sin.^2 \tfrac{1}{2} P \ldots\ldots(1.)$$

But $$ZS = ZM + x.$$

Hence

$$\cos. ZS = \cos. ZM \cos. x - \sin. ZM \sin. x.$$

But, since x is supposed to be a small arc, we may put

$$x = \sin. x,$$

and $$\cos. x = 1 - \frac{x^2}{2} +, \text{\&c.}$$

Hence we obtain

$$\cos. ZS = \cos. ZM\left(1 - \frac{x^2}{2} +, \&c.\right) - x \sin. ZM$$

$$= \cos. Z - \tfrac{1}{2}x^2 \cos. Z - x \sin Z.$$

Therefore equation (1.) becomes

$$\cos. Z - \tfrac{1}{2}x^2 \cos. Z - x \sin. Z$$
$$= \cos. Z - 2 \cos. \phi \cos. \delta \sin.^2 \tfrac{1}{2}P,$$

or
$$\tfrac{1}{2}x^2 \cos. Z + x \sin. Z$$
$$= 2 \cos. \phi \cos. \delta \sin^2 \tfrac{1}{2}P \ldots\ldots\ldots\ldots\ldots(2.)$$

If we neglect the term containing x^2, and suppose x to be expressed in seconds, we shall have

$$x = \frac{2 \sin.^2 \frac{1}{2}P \cos. \phi \cos. \delta}{\sin. 1'' \sin. Z},$$

which formula is sufficiently accurate, when the hour angle does not exceed ten minutes. If a further approximation is required, it may be obtained as follows:—Divide equation (2.) by sin. Z, and we obtain

$$x + \tfrac{1}{2}x^2 \cot. Z = \frac{2 \sin.^2 \frac{1}{2}P \cos. \phi \cos. \delta}{\sin. Z}.$$

Represent the second member of this equation by B, and $\frac{1}{2}$ cot. Z by A, then

$$x + Ax^2 = B,$$

or
$$x = B - Ax^2.$$

But B is the approximate value of x before found; hence, for a second approximation, we shall have

$$x = B - AB^2;$$

or, supposing x to be expressed in seconds,

$$x = \frac{2 \sin.^2 \frac{1}{2}P \cos. \phi \cos. \delta}{\sin. 1'' \sin. Z}$$

$$-\left(\frac{\sin.^2 \frac{1}{2}P \cos. \phi \cos. \delta}{\sin. Z}\right)^2 \frac{2 \cot. Z}{\sin. 1''} \ldots\ldots(3.)$$

which is the correction to be subtracted from the zenith distances observed near the meridian, for an upper culmination, in order to obtain the true meridional zenith distance.

(4.) *By a Single Altitude, the Time of Observation being known.*—Let Z be the zenith of the observer, P the pole, S a star whose altitude is measured at a known instant of time. Then, in the spherical triangle ZPS, we have given PS $= 90° - \delta$, ZS $=$ Z, and the hour angle ZPS, to find PZ. From S let fall the perpendicular SM upon PZ produced. Then, by Napier's rule, we shall have

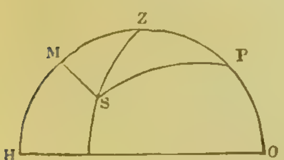


Fig. 4.

$$R. \cos. P = \tan. PM \cot. PS = \tan. PM \tan. \delta.$$

Hence
$$\tan. PM = \cos. P \cot. \delta \ldots\ldots\ldots(1.)$$
$$MZ = PM - PZ = PM + \phi - 90°.$$

Also,

$$\cos. PM : \cos. ZM :: \cos. PS : \cos. ZS,$$

or, $\cos. PM : \sin. (PM + \phi) :: \sin. \delta : \cos. Z.$

Hence

$$\sin. (PM + \phi) = \frac{\cos. Z \cos. PM}{\sin. \delta} \ldots\ldots(2.)$$

Equation (1.) furnishes the value of PM, and equation (2.) furnishes the value of PM $+ \phi$. The difference between these quantities is ϕ, the latitude required.

(5.) *By Observations of the Pole Star at any Time of the Day.*—Let P be the pole, Z the zenith, ZPN the meridian of the place of observation, and S the pole star in any point of its diurnal circle, SBS′. Then we shall have ZP $= 90° - \phi$, ZS $= 90° -$ H, H being the observed height of the star corrected for refraction. Represent the polar distance, PS, by d. Since the arc d is at present less than 90′, the sides ZP, ZS, differ only by a small arc, x, which we propose to calculate.—Let

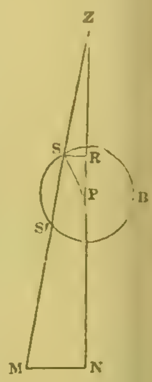


Fig. 5.

ZP — ZS $= x$; that is, H $- \phi = x$,

or
$$\phi = H - x.$$

Represent the hour angle ZPS by P. The spherical triangle ZPS furnishes—

$$\cos. ZS = \cos. PS \cos. PZ + \sin. PS \sin. PZ \cos. P,$$

or

$$\sin. H = \cos. d \sin.(H - x) + \sin. d \cos.(H - x) \cos. P.$$

By substituting the values of sin. (H $- x$), and cos. (H $- x$), and dividing the whole equation by sin. H, we obtain

$$1 = \cos. d\,(\cos. x - \sin. x \cot. H)$$
$$+ \sin. d\,(\cos. x \cot. H + \sin. x) \cos. P,$$

or

$$1 = \cos. x\,(\cos. d + \sin. d \cot. H \cos. P)$$
$$- \sin. x\,(\cos. d \cot. H - \sin. d \cos. P).$$

Let us put

$$a = \cos. d + \sin. d \cot. H \cos. P,$$
$$b = \cos. d \cot. H - \sin. d \cos. P,$$

and we have

$$1 = a \cos. x - b \sin. x \ldots\ldots\ldots\ldots(1.)$$

But

$$\sin. y = y - \frac{y^3}{6} + \frac{y^5}{120} -, \&c.$$

$$\cos. y = 1 - \frac{y^2}{2} + \frac{y^4}{24} -, \&c.$$

Therefore

$$a = 1 + d \cos. P \cot. H - \frac{d^2}{2}$$

$$-\frac{d^3}{6}\cos.\text{P}\cot.\text{H}+,\ \&c.$$

$$b=\cot.\text{H}-d\cos.\text{P}-\frac{d^2}{2}\cot.\text{H}$$
$$+\frac{d^3}{6}\cos.\text{P}+,\ \&c.$$

Let us now assume

$$x=\text{A}\,d+\text{B}\,d^2+\text{C}\,d^3+,\ \&c.......(2.)$$

where A, B, and C represent unknown coefficients independent of d.—Then we shall have

$$\cos.x=1-\frac{\text{A}^2\,d^2}{2}-\text{A}\,\text{B}\,d^3+,\ \&c.$$

$$\sin.x=\text{A}\,d+\text{B}\,d^2+\left(\text{C}-\frac{\text{A}^3}{6}\right)d^3+,\ \&c.$$

Substituting in equation (1.) the values of a, b, sin. x, and cos. x, arranging the terms in the order of the powers of d, and retaining all the terms which contain the first three powers of d, we obtain

$$1=1+\cos.\text{P}\cot.\text{H}\,.\,d-\frac{d^2}{2}-\frac{d^3}{6}\cos.\text{P}\cot.\text{H}$$
$$-\frac{\text{A}^2\,d^2}{2}-\frac{\text{A}^2\,d^3}{2}\cos.\text{P}\cot.\text{H}-\text{A}\,\text{B}\,d^3$$
$$-\ \text{A}\cot.\text{H}\,.\,d+\text{A}\cos.\text{P}\,.\,d^2+\frac{\text{A}\,d^3}{2}\cot.\text{H}$$
$$+\text{B}\,d^3\cos.\text{P}-\text{B}\cot.\text{H}\,.\,d^2$$
$$-\left(\text{C}-\frac{\text{A}^3}{6}\right)\cot.\text{H}\,.\,d^3.$$

Since this equation must be verified by any value of d, the terms involving the same powers of d must cancel each other. Hence, *First.*

$\cos.\text{P}\cot.\text{H}-\text{A}\cot.\text{H}=0$; whence $\text{A}=\cos.\text{P}$.

Second.

$$-\tfrac{1}{2}-\frac{\text{A}^2}{2}+\text{A}\cos.\text{P}-\text{B}\cot.\text{H}=0.$$

Therefore,

$$\text{B}\cot.\text{H}=\cos.^2\text{P}-\frac{\cos.^2\text{P}}{2}-\tfrac{1}{2}$$
$$=\frac{\cos.^2\text{P}-1}{2}=-\frac{\sin.^2\text{P}}{2}.$$

Hence $\quad \text{B}=-\dfrac{\sin.^2\text{P}}{2}\tan.\text{H}.$

Third.

$$-\frac{\cos.\text{P}}{6}-\frac{\text{A}^2\cos.\text{P}}{2}+\frac{\text{A}}{2}-\left(\text{C}-\frac{\text{A}^3}{6}\right)=0.$$

Whence, substituting the value of A already found,

$$3\,\text{C}=\cos.\text{P}-\cos.^3\text{P}$$
$$=\cos.\text{P}\,(1-\cos.^2\text{P})$$
$$=\cos.\text{P}\sin.^2\text{P}.$$

Therefore,

$$\text{C}=\tfrac{1}{3}\cos.\text{P}\sin.^2\text{P}.$$

Substituting these values in equation (2.), we obtain,

$$x=d\cos.\text{P}-\tfrac{1}{2}\sin.^2\text{P}\tan.\text{H}\,.\,d^2$$
$$+\tfrac{1}{3}\cos.\text{P}\sin.^2\text{P}\,d^3;$$

or, multiplying by sin. 1″, in order that x may be expressed in seconds of arc, we have

$$\phi=\text{H}-d\cos.\text{P}+\tfrac{1}{2}\sin.1''\,(d\sin.\text{P})^2\tan.\text{H}$$
$$-\tfrac{1}{3}\sin.^4 1''\,(d\cos.\text{P})\,(d\sin.\text{P})^2...(3.)$$

The last term of this equation never amounts to half a second, and may therefore generally be omitted.

(6.) *By Observing the Difference of the Meridional Zenith Distances of Two Stars on Opposite Sides of the Zenith.*—If we select two stars whose places are well known, one of which culminates to the north, and the other to the south of the observer, at nearly the same distances from the zenith, and within a short interval of time, and measure accurately the *difference* of their zenith distances, the latitude of the place of observation may thence be easily deduced. If we represent the zenith distance of the northern star by z_n, and that of the southern star by z_s; also the declination of the northern star by δ_n, and that of the southern star by δ_s, then we shall have

$$\phi=\delta_s+z_s;$$
$$\phi=\delta_n-z_n.$$

Hence $\quad 2\,\phi=\delta_s+\delta_n+z_s-z_n;$

that is, the sum of the declinations of the two stars (which are given by the catalogue), added to the difference of their zenith distances, gives twice the latitude of the place.

(7.) *By Observations with a Transit Instrument in the Prime Vertical.*—This method supposes the transit instrument to be placed with its supports north and south, so that the telescope, when directed toward the horizon, points due east and west. We must then observe the passage of some known star over the same wires when the telescope is pointing east and west. From these observations we may determine the latitude of the place, or the declination of the star, when either of these quantities is known.—Let P represent the pole of the earth, Z the zenith of the observer, E Z W the prime vertical, which is also the line described in the heavens by the transit; and let the arc S B S′ be the path of a star which culminates a little south of the zenith. Let the times at which a star crosses the field of the transit at S and S′ be noted; and the angle S P S′, which is the difference of those

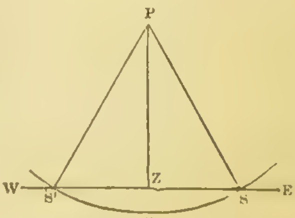


Fig. 6.

times, will be known. Then, in the right-angled spherical triangle P Z S, by Napier's rule,

$$\text{R. cos. ZPS} = \text{tan. PZ cot. PS}.$$

Put $\text{ZPS} = \text{P} =$ half the sidereal interval between the times of east and west transit;

$\delta = 90° - \text{PS} =$ the declination of the star.

$\phi = 90° - \text{PZ} =$ the latitude of the place.

Then $\quad \cos.\ \text{P} = \cot.\ \phi\ \tan.\ \delta;$

or $\quad t\ \ \phi = \dfrac{\tan.\ \delta}{\cos.\ \text{P}}. \ldots\ldots\ldots\ldots\ldots (1.)$

But to accomplish this object in the best manner requires an instrument of a peculiar construction. The instrument should admit of having the level applied to it while the telescope is in the position of observation, and it should also admit of being reversed with ease and rapidity. The figure below represents such an instrument made by Pistor and Martins, of Berlin.

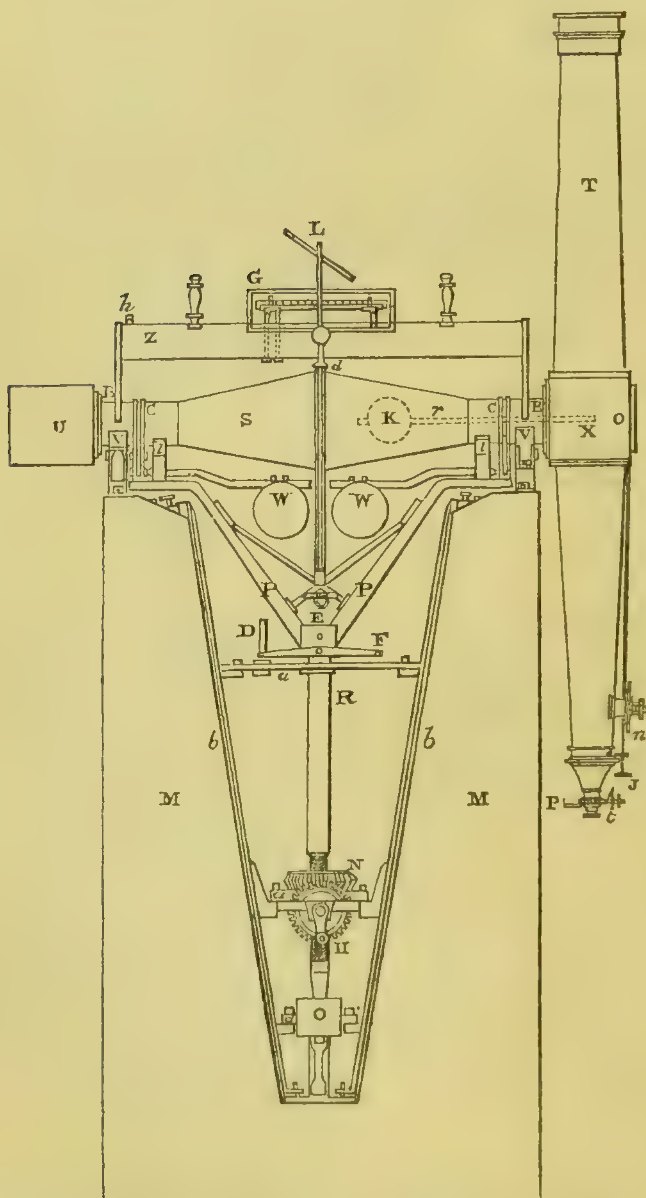


Fig. 7.

—The instrument rests on a block of granite, M M, 6 feet 5 inches high, 3 feet 3 inches from east to west, and 3 feet 7 inches from north to south. This block is cut so as to form two columns 4½ feet high, separated by a cavity which contains the reversing apparatus.—S is the axis of the instrument, terminating in two pivots, B, B, 3·6 inches in diameter; to one of which is at-

LAT

tached the telescope, T, to the other the cylinder, U, which counterpoises the telescope. The telescope is 6½ feet focal length, and 4·8 inches clear aperture.—V, V are the Y's which support the axis, and C, C are friction rollers, with grooves for relieving the Y's. They are regulated by the counterpoises W, W, all of which are carried by the reversing apparatus.—The axis, S, is hollow, and contains a lever, *r*, one end of which expands into a fork, and is firmly secured at X to each side of the telescope tube. To the other end of the lever is attached the counterpoise, K, which transfers the weight of the telescope to that part of the pivot which rests immediately upon the Y's. A similar counterpoise is placed on the other side, to produce the same effect with reference to the cylinder U.—Z is the striding level, which rests permanently upon the pivots B, B during the observations; and L is a mirror for illuminating the level divisions by means of a lamp. The level tube is protected by a glass case, G, and there is a cross level at *h*.—About the middle of the axis, at *d*, is a clamp for slow motion of the telescope, and a screw, with a Hook's joint, at E.—The reversing apparatus, P, P, turns on an inverted cone, working in the hollow cylinder, R, and is strengthened by the cross iron bars, *a*, *a*, *a*, which are supported by the flat iron bars, *b*, *b*.—H is a crank which turns a cog-wheel at N, which, by means of a screw, lifts the hollow cylinder, R, and, by means of the forks, *l*, *l*, lifts the horizontal axis until the pivots, B, B, are sufficiently high to clear the Y's. The telescope is then turned to a zenith distance of about 45°, and is revolved to the other side of the pier. It is prevented from going too far by the arm F, which is so adjusted as to strike the pin D, when the telescope is exactly over the Y's. —*n* is a finding circle for setting the telescope upon a star.—J is the handle of a screw, which moves a slide at O for regulating the illumination of the wires.—*t* is the micrometer head and screw moving the micrometer wire.—*p* is a lever which carries the eye-piece across the field.—In the eye-piece of the telescope are inserted two horizontal and parallel threads, distant 1′ from each other; and also 15 fixed vertical lines, with one moveable one. The transits over the vertical lines are designed to be observed midway between the two horizontal lines.—Having determined the error of level of the axis, the observer directs the telescope to a star while it is yet north of the eastern prime vertical, and observes the transit of the star over each of the wires preceding the middle of the field; the altitude of the telescope being continually changed, so that the oblique transit may be observed over the centre of each wire. When the star has passed the wire next before the middle, he reverses the axis, by which means the telescope will be carried to the opposite side of the pier, and observes the passage of the star, now on the south side of the eastern prime vertical, over the same wires as before, but in the opposite order. He next determines the error of level of the axis. When the star is approaching the western prime vertical from the south, the instrument being still in its second position, he ascertains again the error of level of the axis. Again he observes the transit of the star over the first seven wires preceding the middle of the field; reverses the instrument to its first position, and observes the transit of the star, now on the north side of the western prime vertical, over the same wires. Finally, he ascertains the error of level of the axis in the last position.—The following observations were made by Struve, with the prime vertical transit of the Pulkova Observatory. The numbers in the last column are read from below, upward:—

January 15, 1842. *o Draconis.*

Wires.	EAST VERTICAL. Telescope S. h. m. s.	WEST VERTICAL. Telescope S. h m. s.
I.	17 54 30·7	19 42 51 4
II.	55 8·65	42 13·65
III.	55 44·4	41 38·0
IV.	56 22·25	40 59·85
V.	57 0·6	40 21·7
VI.	57 40·9	39 41·4
VII.	17 58 19·5	19 39 2·7
	Telescope N.	Telescope N.
VII.	18 1 4·0	19 36 17·85
VI.	1 45·5	35 37·0
V.	2 29·8	34 52·35
IV.	3 12·7	34 9·3
III.	3 57·6	33 24·7
II.	4 39·8	32 42·1
I.	18 5 26·35	19 31 55·6
	Level = + 0″·687	Level = + 0″·923

The reduction of the observations is made as follows, each wire being treated separately:—Let N E S W represent the horizon, N S the meridian, E W the prime vertical, P the pole, and A the place of the star at its transit over one of the wires of the telescope. Join P A and N A by arcs of great circles. The projection of each wire on the sky is a small circle, whose pole is the north point, N, of the horizon. If *c* represent the angular distance of one of the wires from the line of collimation, 90° — *c* will be the radius N A of the small circle, when the star is seen on it, north of the prime vertical, and 90° + *c* when the star is south of the prime vertical.—In the triangle P N A, we have

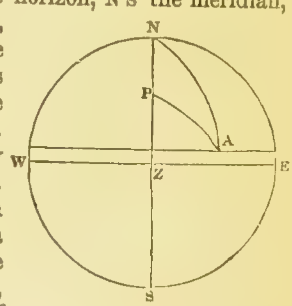


Fig. 8.

$$\cos. NA = \cos. NP \cos. PA + \sin. NP \sin. PA \cos. NPA.$$

Let ϕ = N P the latitude of the place;

$\delta = 90° -$ P A = the star's declination;

t, t' = the hour angles S P A from the meridian, at the two observations over the same wire, in the direct and reversed positions of the axis.

Then, when the star is north of the prime vertical,

$$\cos.(90^\circ - c) = \sin. c = \cos.\phi \sin.\delta - \sin.\phi \cos.\delta \cos. t;$$

and, when the star is south of the prime vertical,

$$\cos.(90^\circ + c) = - \sin. c = \cos.\phi \sin.\delta - \sin.\phi \cos.\delta \cos. t'.$$

Adding these two equations, we obtain

$$0 = 2 \sin.\delta \cos.\phi - \cos.\delta \sin.\phi (\cos. t + \cos. t'),$$

or,

$$\tan.\delta \cot.\phi = \frac{\cos. t + \cos. t'}{2} = \cos.\frac{t' + t}{2} \cos.\frac{t' - t}{2} \ldots\ldots(2.)$$

This formula will furnish the declination when the latitude is known, or the latitude when the declination is known. The latitude of the Pulkova instrument is 59° 46′ 18″. t' represents half the interval between the first transit east and the second transit west; and t is half the interval between the second transit east and the first transit west.—The following is Struve's reduction of the preceding observations, a correction of + 0·09s. being applied to the interval W. — E. for rate of clock:—

	Wire I.	Wire II.	Wire III.	Wire IV.	Wire V.	Wire VI.	Wire VII.
	h. m. s.	m. s.	m. s	m. s.	m. s.	m. s.	m. s.
W.—E. $2t'$	1 48 20·79	47 5·09	45 53·69	44 37·69	43 21·19	42 0·59	40 43·29
W.—E. $2t$	1 26 29·34	28 2·39	29 27·19	30 56·69	32 22·64	33 51·59	35 13·94
$\frac{1}{2}(t' + t)$	0 48 42·53	48 46·87	48 50·22	48 53·60	48 55·96	48 58·05	48 59·31
$\frac{1}{2}(t' - t)$	0 5 27·86	4 45·67	4 6·62	3 25·25	2 44·64	2 2·25	1 22·33
$\cos.\frac{1}{2}(t' + t)$	9·9901167	0871	0642	0411	0250	0107	0020
$\cos.\frac{1}{2}(t' - t)$	9·9998765	9063	9301	9516	9688	9828	9922
$\tan.\phi$	0·2345728	5728	5728	5728	5728	5728	5728
$\tan.\delta$	0·2245660	5662	5671	5655	5666	5663	5670
δ	59° 11′ 39″·00	39″·04	39″·23	38″·90	39″·12	39″·06	39″·21

The mean error of level of the instrument may be applied to ϕ, or we may apply a correction to the declination obtained with a constant value of ϕ. If the inclination of the axis be denoted by I, which is the mean of the two inclinations, telescope N and telescope S, then $\phi + \text{I}$ should be used in place of ϕ. Now we have

$$\tan.\delta = \tan.\phi \cos. \text{P}.$$

By differentiating, supposing P constant, we obtain

$$d\delta \sec.^2\delta = d\phi \sec.^2\phi \cos. \text{P}.$$

Hence

$$d\delta = d\phi \frac{\sec.^2\phi \tan.\delta}{\sec.^2\delta \tan.\phi} = d\phi \frac{\cos.\delta \sin.\delta}{\cos.\phi \sin.\phi} = d\phi \frac{\sin. 2\delta}{\sin. 2\phi},$$

or

$$d\delta = \frac{\sin. 2\delta}{\sin. 2\phi}\ \text{I}.$$

In the preceding observations the mean inclination of the axis was + 0″·805.

The mean value of δ	= 59° 11′ 39″·071
Correction for inclination of axis	+ 0″·814
Observed declination	= 59° 11′ 39″·885

The declination thus found is not correct, unless the telescope is truly adjusted to the prime vertical. Suppose there is an error in the azimuth

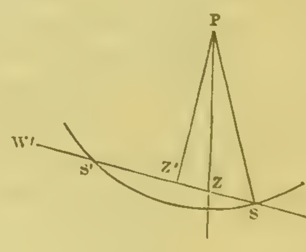


Fig. 9.

of the instrument equal to a or $90^\circ - \text{P Z Z}'$; then, in the triangle P Z Z′,

$$\tan. \text{P} = \frac{\cot. \text{P Z Z}'}{\cos. \text{P Z}} = \frac{\tan. a}{\sin.\phi}.$$

If the error in azimuth be small, we may assume

$$\text{P} = \frac{a}{\sin.\phi},$$

which represents the angle at the pole, between the true meridian and the meridian of the instrument. The instant of the star's passage over the meridian of the instrument is equal to the half

sum of the east and west transits. Thus, in the preceding observation, we have

Wires.	Telescope S.	Telescope N.
I.	18h. 48m. 41·10s.	18h. 48m. 40·93s.
II.	41·15s.	41·25s.
III.	41·20s.	41·07s.
IV.	41·05s.	41·00s.
V.	41·15s.	41·15s.
VI.	41·15s.	40·95s.
VII.	41·10s.	40·97s.
Mean,	18h. 48m. 41·13s.	41·05s.

Mean, 18h. 48m. 41·09s.

The instant of meridian passage requires a small correction for the difference of inclinations of the axis in the two verticals.

Latitude, Astronomical. If a great circle be drawn through a Star from the pole of the Ecliptic, the portion of that circle intercepted between the star and the Ecliptic is its *Latitude.*

Latus-Rectum. Another name for the *parameter* or *focal chord* of a conic section.

Law. A word of frequent and important use alike in Physics and pure Mathematics. It is essential to have it understood, that the term has no *objective* significance, no significance in so far as external events or phenomena are concerned, beyond this,—it merely expresses *an order of sequence*, or the mode in which changes succeed each other. If the succession be very regular, the Law is simple; if comparatively irregular, the Law is more complex. Into the *subjective* meaning and relations of Physical Law, its relations to our notion of FORCE or to the idea of CAUSATION, we inquire not here.—In pure mathematics, the word is generally employed to designate the mode in which the successive terms of a *series* are derived,—each one, from those going before it. Sometimes this mode of derivation is simple, and at other times quite complex. The highest and most grasping expression of Law, according to this view of it, is exemplified in Laplace's idea of *Generating Functions.*

Laws of Motion:—Those fundamental propositions on which the whole of *Statics* and *Dynamics* repose. Various statements have been given of them, and very various theories advanced as to the source of their authority. We have no room for criticism, and shall simply expose therefore, that view of the important subject which seems to us most satisfactory.—It has already been explained under INERTIA, that the so-called *inertia of matter*, is not a physical fact, but a logical hypothesis,—an hypothesis involving no error provided it be understood as such, and which is absolutely necessary so that Dynamical science be *possible.* This Inertia, then, is not a Law of Motion, or a fundamental fact, but a preliminary or *postulated hypothesis.* The FIRST Law of Motion, or the *Law of Inertia*, as distinguished from the Hypothesis of Inertia, was discovered by John Kepler. It is this:—*All motion is naturally rectilinear and uniform*; in other words, *every body acted on by a single instantaneous force, moves constantly in a straight line and with an uniform velocity.* Various but most futile attempts have been made—chiefly through misuse of the Principle of the *Sufficient Reason*—to prove the absolute nature and necessity of this Law, or to establish it as an *à priori* truth; whereas the very circumstance, that—notwithstanding the acuteness of the Greeks, and their especial aptitude as to every form of *à priori* investigation—the discovery of the Law fell so late as the times of Kepler, would seem enough to class that discovery among the fruits of slowly advancing observation. In fact, the discovery of such a principle could not be easy, inasmuch as we discern simple motion nowhere around us. Every body that is moving, is under the influence of several forces, unless we except the case of falling bodies, which again are not acted on by an *instantaneous* force; nor can the natural influence of a single instantaneous force be eliminated from any visible phenomenon, unless by considering the path resulting from that force as the limit or asymptote of the paths actually passed over, or by a process of analysis. A projectile, for instance, takes a parabolic course, because its subjection to the laws of falling bodies combines with the effect of the impulse. The former effect is constant, but the latter may be varied indefinitely. Increase it, and the parabola becomes wider and wider—a larger and larger portion of it approaching to identity with its tangent: if the power of the impulse, therefore, were augmented indefinitely,—in other words, if it were infinite, or so great that gravitation might be supposed relatively to disappear, the path of the projectile would be along the tangent or in *a straight line.* Or, the laws of falling bodies understood, as well as the third Law of Motion, the complex path may be *analyzed*: if the effects of gravitation be then distinguished and separated, the residuum will be, a rectilineal path and an uniform velocity, as due to the instantaneous force or impulse. In many other ways, this great Law of Nature is indicated; but it needed the experience of ages, and the acuteness of such a man as Kepler, to disentangle it.—The SECOND Law of Motion is due to Sir Isaac Newton. It asserts the *universal equality of action and reaction*: that is, if one body strikes another, *it will lose exactly the same quantity of motion as it communicates to the body struck.* (The term, *quantity of motion*, signifies the mass or weight of the body multiplied by the velocity with which it moves.) This, also, is clearly the result of an universal experience or observation; nor have attempted *à priori* demonstrations been more successful in its case than in the former. It ought to be remarked that the famous Principle of D'Alembert, (see D'ALEMBERT, PRINCIPLE OF), by whose aid all problems of *Dynamics* are so readily transformed into mere problems of *Statics*, is nothing but a complete generalization of this Law of Newton's. The principle in question

only states regarding *any number* of connected forces, what Newton's Law asserts regarding *two:* which mode of considering it, saves us from the necessity of placing reliance on certain very unsatisfactory abstract speculations that are often adduced as its foundation.—The THIRD Law of Motion is usually designated the Law of the *Composition of Motion or Forces*, (q. v.), discovered by Galileo. In its pure and simple form, this law merely asserts that any motion common to any system of bodies whatsoever, does not alter the special motions of the different bodies of that system with respect to each other: or, that if a body is moving in one direction, the addition of some new impulse will only superinduce its own effect, without destroying or even modifying any tendency that previously existed. The Law, indeed, instead of being called the Law of the Composition of Forces, should rather be termed the *principle of the independence or co-existence of motions;* a principle of wide reach, of which the mere composition of motions is but a corollary, and that includes within it the famous principle of *the co-existence of small oscillations* (see OSCILLATIONS), which became so powerful in the hands of Daniel Bernouilli. There is less difficulty in recognizing the principle as now stated, to be the result of universal observation, than in the case of our first and second Laws. All phenomena around us indicate its reality. The smooth motion of a ship does not derange the vessel's internal economy: nor are scientific experiments wanting to sustain its authority,—for instance, Laplace showed that the oscillations of the pendulum are the same, whatever the relation of their plane to the direction of the rotation of the Earth. Nevertheless, not only have efforts not been wanting to establish this principle on abstract considerations; but there is, perhaps, no principle in Philosophy on which so much vain logic has been expended, so that it might appear based on mathematical reasoning. The higher analysis even, has been brought into ostentatious play: in every instance, however, something has been assumed quite equivalent to the Principle itself. For instance, Poisson, in his very *mise en equation*, takes it for granted that whatever the angle at which the two independent forces act, they must be compounded, *in the same way*, to produce the resultant; or that the *form* of his *function* cannot vary with that angle. If this assumption be analyzed, the inmost nature of the truth to be established will be found involved in it.—These Laws, then, ought to be held as results of universal Observation.

Leap Year. The true solar year is nearly 365¼ days long. If, therefore, we were to date our time from its commencement, on one year our day would begin, suppose at 12 o'clock at night, next year it would begin at 6 in the morning, next at 12, next at 6 in the evening, and next again at midnight. There would thus be a constant rotation of the commencement of the first day of the year, which would seriously embarrass civil affairs. In our calendars, therefore, to remedy this main and obvious inconvenience, an entire day is introduced into the month of February every fourth year. On every fourth year, February has *twenty-nine* days. That one of any four successive years which is *leap year*, is the one divisible by 4 without remainder. See BISSEXTILE.

Least Squares, Method of. See SQUARES, THE LEAST.

Leda. One of the Asteroids very recently discovered. See table of recent Asteroids in Appendix.

Lens. There are ten different kinds of lenses of the more ordinary spherical form.—They are represented in fig. 1; the light being incident on them in the direction of the arrow—

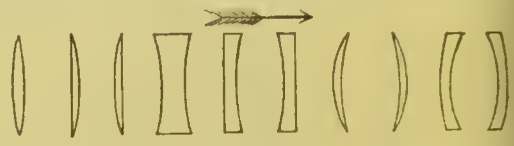


Fig. 1.

No. 1.—The double convex or concavo-convex lens—which may or may not have the spherical surfaces of equal radii.
2.—Plano-convex.
3.—Convexo-plane.
4.—Double concave or concavo-concave.
5.—Plano-concave.
6.—Concavo-plane.
7.—Convex meniscus (moon-shaped).
8.—Concave meniscus.
9.—Convexo-concave.
10.—Concavo-convex.

It is necessary to specify the direction in which the ray comes from the luminous point; because several of these lenses might be interchanged when that is altered. Thus the lenses, No. 7 and No. 8, would be exactly the same, if the ray fell upon them from different sides. So also with 2 and 3, with 5 and 6, and with 9 and 10. It seems, therefore, that the following division includes all the real forms of the lens, and that, when we wish to change, for example, a lens No. 2, into a lens No. 3, we require simply to turn it so that one surface shall be met by the light, instead of the other—

1.—The simple convex (Nos. 2 and 3).
2.—The simple concave (Nos. 5 and 6).
3.—The double convex (No. 1).
4.—The double concave (Nos. 4, 7, 8, 9, 10).

The latter lenses, Nos. 9 and 10, differ from the lenses, Nos. 7 and 8, simply in this, that the inner circle diverges from the outer in one pair, while it converges to it in the other.—The uses of lenses are principally two—to increase the apparent magnitude of objects from which luminous rays come (as described in the articles TELESCOPE and MICROSCOPE); and to collect luminous rays issuing from a body, luminous either in itself or by reflection, into a point to which the eye can be conveniently applied. The former use

hangs very much by the latter, but we shall here confine ourselves to the latter.—In each of the lenses we have enumerated, a ray incident at any point, would pass through one surface, from the air, into the material of the lens; and generally speaking, next through that material, — thus suffering two distinct refractions (DIOPTRICS), before meeting the eye. — To take the simpler cases first, we shall imagine that the lens consists of one curved surface on which the ray is incident, and that the point to which the refracted rays converge is in the interior of the refracting substance. It thus ceases to be material, what may be the exact form of the second lenticular surface; and we have the simple case of a ray incident upon a single curved surface, of given refractive power, at a certain angle, the course of which, through the mass of refracting substance, is required.—We have no species of lens which will concentrate, by refraction, all the luminous rays (even supposing that these rays consist of homogeneous, or equally refrangible light) coming from points placed at various distances from it in space. For *one* luminous point, we can construct *one* surface, so that the rays dispersed from that point and incident upon that surface, shall converge to one point within the refracting mass.—As the positions of points may be infinitely varied, it would be impossible to apply simple geometrical considerations to the full question. One analytical expression might indeed be found applicable to every case; but if we reject analysis, we are reduced, in all of the more complicated problems, to consider special cases included under the general formula, as we have found, for instance in the article DIOPTRICS (which should be read before this article), when considering the course of rays passing through refracting media, and falling at the points of incidence in, and emergence from, the medium in question, upon planes not parallel.—The case which most frequently turns up, is that of luminous rays from the sun, moon, planets, or fixed stars. These bodies are so far removed that we may consider the rays from them as parallel. The problem then is, to find a lens, which shall refract, by a single refraction, luminous rays falling parallel upon it, to a single point in its mass.—An elliptic lens is such an one, that its eccentricity is equal to $\frac{1}{\mu}$ (μ being the refractive index of the material of the lens). In this case the refracted rays converge to the farther focus of the ellipsoid,—the parallel rays falling upon the convex surface of the ellipsoid.—A hyperbolic lens has a similar property. If a pencil of parallel rays fall upon the concave surface of a hyperboloid, whose substance has a refractive index, equal in value to its eccentricity, the rays will be refracted to the other focus of the hyperboloid.—The converse of these two propositions also holds, viz., if rays diverge from the foci, to which, in the cases mentioned, the parallel rays converged, they will be refracted from the medium, as parallel rays.—In this most important case then, ellipsoid and hyperboloid lenses would altogether serve our purposes, and might be used with very great advantage in telescopes; for even although not employed with a view to celestial observations, the objects to which they would be directed are nevertheless ordinarily at such a distance, that rays coming from them might be considered parallel. But it unfortunately happens that lenses of this precise shape are of very difficult construction in any case, and especially so in the larger and finer instruments. The only possible form of construction, is either that of plane or spherical surfaces. The former would produce no effect, however, of use for the purposes of a magnifying instrument. —We are at once led therefore to consideration of the refraction of homogeneous light at *spherical* surfaces. Our *first* proposition is, that the refraction, at a spherical surface, of rays from a luminous point at a distance from its centre $= \mu$ times the radius, is directed to a point, the distance of which from the centre, will form with the distance of the first luminous point from the centre, a rectangle equal to the square of the radius.—Let $\text{P A P}'$ (fig. 2) be such a surface, and let $\text{Q O} = \text{A O} \cdot \mu$ and $\text{Q O} \cdot q' \text{O} = \text{A O}^2$, then the

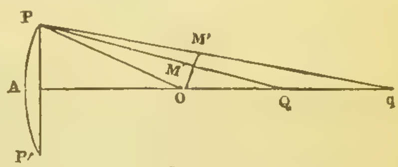


Fig. 2.

refracted rays will be refracted accurately to q' (Q being the luminous point from which the rays diverge).

Since $\text{Q O} \cdot q' \text{O} = \text{A Q}^2$

$$\text{Q O} : \text{O A (or O P)} :: \text{O P} : \text{O} q'$$

Therefore $\frac{\text{Q O}}{\text{Q P}} = \frac{\text{O P}}{q' \text{P}}$ (VI. 16.)

Now $\text{Q O} = \text{A O} \cdot \mu = \text{O P} \cdot \mu$

$$\therefore \mu \cdot \frac{\text{O P}}{\text{Q P}} = \frac{\text{O P}}{q' \text{P}}$$

$$\therefore \mu \cdot q' \text{P} = \text{Q P}; \text{ and } \frac{\text{P} q'}{\text{P Q}} = \frac{1}{\mu}$$

Let $\text{O M}' \text{O M}$ be drawn perpendicular to PQ and $\text{P} q$

$$\text{Then } \frac{\text{O M}}{\text{O M}'} = \frac{\text{Sin O P} q'}{\text{Sin O P Q}}$$

$$\text{But O M} \cdot \text{P} q' : \text{O M}' \cdot \text{P Q} :: \text{P} q'^2 : \text{P Q}^2.$$

Since the first two members of this proportion are equal, respectively, to double the area of the triangles $\text{O P} q'$, O P Q, and since these triangles are proportional to $\text{P} q'^2$, and P Q^2, respectively,

—we have, therefore,

$$OM : OM' :: Pq' : PQ$$

$$\text{And } \frac{OM}{OM'} = \frac{\text{Sin } OPq'}{\text{Sin } OPQ} = \frac{Pq'}{PQ}$$

$$\text{But } \frac{Pq'}{PQ} = \frac{1}{\mu}$$

$$\therefore \frac{\text{Sin } OPq'}{\text{Sin } OPQ} = \frac{1}{\mu}$$

$$\text{Sin } OPQ = \mu \text{ Sin } OPq'.$$

Now O P is the perpendicular to a plane touching the incident surface at P, and O P Q is therefore the angle of incidence. Hence O P q' is equal to the angle of refraction, according to the dioptrical law; and the ray will appear to an eye, within the mass of P P, as coming from q' instead of from Q. This will hold, evidently, with regard to all the rays passing from Q, by parity of reasoning (since nothing in the reasoning, required to postulate that P Q A was any particular angle).—The same proof holds for convex surfaces on which rays are incident (as in fig. 3). Rays

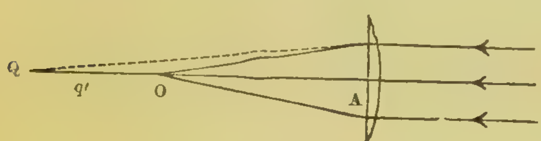


Fig. 3.

which have a focus (or point of convergence), if uninterrupted, at Q (O Q $= \mu$. O A as before), will converge in reality to q' $\left(Oq' = \frac{OA^2}{OQ}\right)$.

Thus imagine a concave surface of glass—the radius of the spherical part of which (O A, fig. 2) is 1 foot. Then O Q $= 1{\cdot}5 \times 1 = 1\frac{1}{2}$ foot.—Rays then emerging from Q will be seen through the glass, as if coming from q', which is determined by the equation

$$Oq' = \frac{OA^2}{OQ} = \frac{1}{1\frac{1}{2}} = \frac{2}{3} \text{ foot.}$$

That is, such rays will appear as coming from a point 10 inches (18—8) nearer the spherical surface. The point Q, however, is here determined for every glass surface, with the same radius and refracting index.—But with any given refracting surface, we have generally to take account of luminous rays coming from other points. The object cannot always be moved to a point corresponding to Q; and we are usually quite as unable to move towards it, or to go so far towards it, as to bring it to the point adapted to a given lens. Generally, moreover, a luminous object consists of a series of points rather than one, and if one be in the position Q, the others, for that very reason, are not there. There will, therefore, be—as this is the only case of accurate refraction to an actual point, with spherical lenses—decomposed light in many cases, and in still more homogeneous light dispersed. To this is owing the formation of the caustic curve (CAUSTIC), and much of the disarrangement which requires achromatic applications.—In the problem just treated, the luminous point could not be at a great distance from the lens, for Q O $= \mu$. O A and μ, in the case of mercury, the most refrangent substance in the table, is less than 6. O Q would therefore be less than 6 O A, and Q A less than 7 O A.—Suppose P P$'$ to grow less and less curved. It would do so, evidently, by O being removed along A O from A; now, if finally, A O came to be an indefinitely great distance, P A P$'$ would be very nearly a flat surface. Q A, therefore, being greater than O A, would also be very large; and rays proceeding from Q might be conceived parallel, as in the case of the solar or astral rays. This peculiar lens, a lens with a plane surface, would therefore have the foregoing proposition applicable to it, for the most usual case of Solar or astral light. Let us apply it.

$$q'O = \frac{AO^2}{QO} = \frac{AO}{QO} \cdot AO = \frac{1}{\mu} \cdot AO$$

$$= \frac{1}{\mu} \cdot \frac{QO}{\mu} = \frac{QO}{\mu^2}.$$

Hence, if Q be indefinitely distant, the focus q' from which the refracted rays would seem to emerge, would also be at a very great distance, since μ^2 could not be greater than 36. A proposition which gives us no very valuable information.—We pass now to consider the case of Solar light incident upon concave or convex spherical lens surfaces, and to seek the focus of accurate reflection, if it may be obtained.—Let the rays S P and S A be solar or astral rays, incident (figs. 4 and 5) upon spherical lenticular

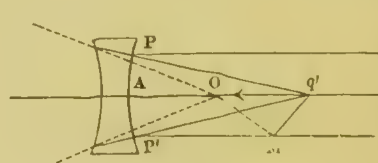


Fig. 4.

surfaces, of which, with air, μ is the relative refractive index. Let q' be the point, as in the former case, where the unrefracted ray (falling upon the surface perpendicularly) meets the refracted ray P q'. Draw P O M through P and O and q' M perpendicular to it, then O P S $=$ P O A $=$ M O q'

now $\text{Sin } MOq' = \text{Sin } i = \frac{Mq'}{Oq'}$; and $\text{Sin } q'PO = \text{Sin } i' = \frac{Mq'}{q'P}$, hence $\frac{\text{Sin } i}{\text{Sin } i'} = \mu$, therefore dividing, $\mu = \frac{q'P}{Oq'}$.

Hence, in order to find the point q', we require to describe upon the base O P, a triangle whose sides shall have the constant ratio μ, and which shall have one of its sides in the direction A O q'. It would not be difficult to show that for each base O P (drawn from O to the various points of the surface) we should have a different point q'. It follows that in this case the ray will not be concentrated to one point, because the same thing happens to the rays below O A$'$, which would therefore intersect each other—if at all—either above O q' or beyond A P$'$, while the corresponding rays above would intersect below O q' or beyond A P.—The rays near A, on both sides, will give q' P and q' A very nearly equal (because P and A become nearly the same point), and O P and O A nearly coincide. Hence, for such rays we have this law $\frac{q'\mathrm{A}}{q'\mathrm{O}} = \mu$. The point q' will therefore concentrate a *thin* pencil of rays. When the light comes from the sun the length, q' A, is called the *principal focal length* of the refracting surface. The focal length varies (being equal to the distance of the point q', which varies as we have seen for each point P, from A), but the length q'A, found by the equation $\frac{q'\mathrm{A}}{q'\mathrm{O}} = \mu$ is that to which all ultimately approach, and therefore has its name. Call q' A $= f'$,

$$\text{then } \frac{f'}{f'-r} = \mu$$

$$\mu f' - \mu r = f'$$

$$(\mu - 1) f' = \mu r$$

$$f' = \frac{\mu r}{\mu - 1}$$

Thus, in a lens of glass, with refractive index 1·5, and radius 1 foot, $f' = \frac{\frac{3}{2} \times 1}{\frac{1}{2}} = 3$ feet.—Precisely the same result is obtained in the case (fig. 5) which we give here, that the student may compare the proof with this figure,

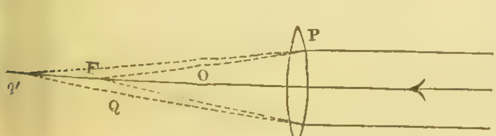


Fig. 5.

in order that he may observe, that nothing depending upon the circumstance of the concavity or convexity being turned towards the luminous pencil, can alter the *length* of the principal focal distance.—We shall now revert to the more general case of the incidence of *diverging* rays upon a spherical medium, and endeavour to get results analogous to those just obtained. Imagine the diverging point situate anywhere, and depending in no degree upon μ. Then

$$\frac{\sin \mathrm{OPQ}}{\sin \mathrm{POQ}} = \frac{\mathrm{QO}}{\mathrm{QP}}$$

$$\frac{\sin \mathrm{PO}q'}{\sin \mathrm{OP}q'} = \frac{q'\mathrm{P}}{q'\mathrm{O}}$$

$$\therefore \frac{\sin \mathrm{OPQ}}{\sin \mathrm{OP}q'} = \frac{\mathrm{QO}}{\mathrm{QP}} \cdot \frac{q'\mathrm{P}}{q'\mathrm{O}}$$

$$\text{but } \frac{\sin \mathrm{OPQ}}{\sin \mathrm{OP}q'} = \mu \qquad \therefore \mu = \frac{\mathrm{QO}}{\mathrm{QP}} \cdot \frac{q'\mathrm{P}}{q'\mathrm{O}}$$

$$\therefore \mu \frac{q'\mathrm{O}}{q'\mathrm{P}} = \frac{\mathrm{QO}}{\mathrm{QP}}.$$

But, for the same reasons as before, if P and A approach very close, we shall have q' P and Q P becoming equal to q' A and Q A

$$\therefore \mu \frac{q'\mathrm{O}}{q'\mathrm{A}} = \frac{\mathrm{QO}}{\mathrm{QA}}$$

Making Q A $= u$, q' A $= u'$ and O A $= r$, we have q' O $= u' - r$, Q O $= u - r$

$$\therefore \frac{u-r}{u} = \mu \frac{u'-r}{u'}$$

$$\therefore u u' - u' r = \mu u u' - \mu u r$$

Divide by $u u' r$, then $\frac{1}{r} - \frac{1}{u} = \frac{\mu}{r} - \frac{\mu}{u}$

$$\therefore \frac{\mu}{u'} = \frac{\mu - 1}{r} + \frac{1}{u}$$

which gives $\frac{\mu}{u'}$, and as μ is known for each special case, u' is determined.—In this case of the concave surface then, we obtain the point from which the luminous rays will appear to diverge. If we take the case of a convex lens, instead of a concave, Q A or u has changed its position relative to the surface, and will, therefore, be marked $-u$ in the application of the formula. Sometimes in this case, the rays, originally dispersing, are made to converge downwards, so as to intersect in one point; sometimes, on the other hand, they still continue to diverge, though less than before. In the latter case the term u' also changes in direction—being on the same side of A with u, as before. In the former it does not, but takes the opposite direction, commencing from A, to that of u. In the first case, therefore, $-\frac{\mu}{u'} = \frac{\mu - 1}{r} - \frac{1}{u}$, or $\frac{\mu}{u'} = \frac{1}{u} - \frac{\mu - 1}{r}$.

In the second case, we have

$$\frac{\mu}{u'} = \frac{\mu - 1}{r} - \frac{1}{u}.$$

Remembering that f' (the principal focal length), which is considered as a fixed point in the system of every lenticular surface $= \frac{\mu r}{\mu - 1}$

we have $$\frac{\mu}{f'} = \frac{\mu - 1}{r}$$

$$\therefore \frac{\mu}{u'} = \frac{\mu}{f'} + \frac{1}{u}$$, in the principal formula just found.—In the case of a converging, instead of a diverging pencil, incident on a concave surface, we have r changing its direction in effect, and the formula becomes

$$\frac{\mu}{u'} = - \frac{\mu - 1}{r} + \frac{1}{u'}.$$

For a plane surface, r is infinite, and

$$\frac{\mu - 1}{r} = o$$

$$\therefore \frac{\mu}{u'} = \frac{1}{u}$$

whence $\mu u = u'$.

For parallel rays incident on a concave surface, Q is at an infinite distance (*i.e.*), u is infinite, and

$$\frac{\mu}{u'} = \frac{u - 1}{r}.$$

—We shall not pursue the subject of the dioptrical effect of single spherical surfaces on homogeneous light further at present.—We pass to consider the effect of the various kinds of lenses, through which there is a second refraction of the luminous rays.—In doing so, it is usual to imagine the thickness of the lens so small that it may be safely disregarded. In an accurate determination of the complete effect, this would require to be taken into account; but unless we employ the principles of the higher mathematics, such a determination is impossible.—We shall not consider here, the effect of a lens upon ordinary white light (ACHROMATISM), but consider, as in the article DIOPTRICS, the light as homogeneous, or of equal refrangibility.—The discovery of the effects of lenses upon light from points at any position, is the most general problem; that upon light consisting of parallel rays is at once the most important and the simplest. The focus of a lens is the point to which all the rays emerging from a given luminous point, actually converge; or, in the case of rays which diverge after refraction, the point from which they seem, to an eye behind the lens, to proceed instead of from the actual luminous point. The principal focus of a lens is the point to which rays incident in a direction parallel to the axis of the lens (the perpendicular through its middle to its two surfaces), and incident upon a small space of the lens, or a small angular space of the circle of which the whole lens is a segment, tend to converge. We shall not give detailed investigations for the various forms of lenses. The principles upon which these are conducted are precisely such as are employed in the investigations already gone over. At the one surface of the lens, the rays are refracted, and pass through its mass in a given direction. We have thus new rays of light, moving in a definite direction—the one just found—and towards a definite point, also found by that investigation. The question is thus simply repeated. The rays, so directed, have to emerge into a rarer medium (or denser, as the case may be) at another spherical surface, the relative refractive index of which and the matter comprising the lens are well enough known; and this problem has been already in effect solved. We aim rather at giving principles than at detailing processes, and shall therefore avoid the repetition of those already indicated. The results will be useful, however, and we give them.—The simplest case is that of a plano-convex lens (fig. 1, No. 2). Rays, as of astral or solar light, incident, parallel to the axis, fall perpendicularly on the first refracting surface. They so fall, therefore, and pass through the substance of the lens, unaltered in direction. We have here, therefore, only one application of the principles of refraction;—we have only to consider the original parallel rays, as coming through their whole course, through the medium of the substance of the lens, and entering the air or water, or whatever other translucent substance the lens may be surrounded by. According to the investigation already given, then we have for that case, making B F $= f$; and the radius O B of the convex lens, $= r$, $f = \frac{r}{\mu - 1}$. If, now, r be two feet, and the lens be made of glass (refractive index 1·5), surrounded by air, we shall have $f = \frac{2}{\frac{1}{2}} = 4$ feet.—In the reverse case, of a convexo-plane lens (fig. 1, No. 3), (the same lens, having its spherical side turned to meet the parallel rays), with the rays parallel as before, we have this result, where A B, the thickness of the lens is taken into consideration, and where B F $= f$ as before

$$\frac{1}{f} = \frac{\mu - 1}{r} + \frac{t}{\mu}\left(\frac{\mu - 1}{r}\right)^2$$

If, as is usually the case, $\frac{t}{\mu}$ be very small, and $\left(\frac{\mu - 1}{r}\right)^2$ very much smaller than $\frac{\mu - 1}{r}$ ($\mu - 1$ being generally less than 1, and r greater than 1), we obtain $\frac{1}{f} = \frac{\mu - 1}{r}$. The same result exactly, is obtained by neglecting the thickness t.—Since therefore $\frac{1}{f} = \frac{\mu - 1}{r}$, $f = \frac{r}{\mu - 1}$ quite as before. It would appear, then, that the two lenses are very nearly of the same value, as far as their focal distance goes. It is readily seen how this must be so. If we imagine t to be small, as we did in the first case, or to vanish as we did in the second, we get the ray strik-

ing upon P Q, at a point so very little below that at which it would have struck, if it had gone on unrefracted, that when it continues its course, it might be easily considered as having continued its course from that point instead of from that at which it does really strike on the plane surface of the lens. When we do take into account the thickness of the lens, if it bear any considerable proportion to the radius of the spherical surface, a different result from the former is undoubtedly produced. Thus, let $r = 2$ feet, $\mu = 1\cdot5$ and $t = 6$ inches, then $\frac{1}{f} = \frac{1}{4} + \frac{1}{3}\left(\frac{1}{4}\right)^2 = \frac{1}{4} + \frac{1}{48} = \frac{13}{48}$ and $f = \frac{48}{13}$ or $3\frac{3}{4}$ feet nearly.

The focal distance is therefore shortened by this rrangement of the glass. In the other, it was uite immaterial how thick the glass might be. No effect was produced upon the focal length. Here the thicker the glass, the greater $\frac{1}{f}$ ecomes, and therefore the less, f, becomes.—There is a considerable effect, moreover, due to this hange of focal length, from the chromatic disersion of the rays of light.—The principal focal ength of a double concave lens is expressed by his formula

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r} + \frac{1}{r'}\right) - \frac{t}{\mu}\left(\frac{\mu - 1}{r}\right)^2$$

where $f =$ B F, r, is the radius of the one, and r' of the other spherical surface, B A being the hickness t. This lens causes the parallel rays to iverge, and the focus here is the point from which ey appear to come.—With a double convex ns, the expression for the principal focal length is

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r} + \frac{1}{r'}\right) + \frac{t}{\mu} \times \left(\frac{\mu - 1}{r}\right)^2.$$

This lens makes, however, the arallel rays converge instead of diverging.—If e thickness be very small, so much so that it ay be neglected, we find the following values for e two cases:—For the double concave,

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r} + \frac{1}{r'}\right).$$

r the double convex,

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r} + \frac{1}{r'}\right).$$

e focal length is therefore the same—the difence of convexity and concavity merely suffic- to determine whether the focus be the point m which the rays will appear to an eye behind the lens to diverge, or *to* which they will appear to converge.—To take an example of the various cases just enumerated, imagine two lenses, the one double concave and the other double convex, the thickness of each of which is two inches—the refractive index 1·5, the radii of the two surfaces, five and six inches respectively. Then, for the double concave lens—

$$\frac{1}{f} = \frac{1}{2}\left(\frac{1}{5} \times \frac{1}{6}\right) + \frac{2}{1\frac{1}{2}}\left(\frac{\frac{1}{2}}{5}\right)^2$$
$$= \frac{11}{60} + \frac{4}{3} + \frac{1}{100} = \frac{11}{60} + \frac{4}{300} = \frac{59}{300}$$
$$\therefore f = \frac{300}{59} = 5\tfrac{1}{12} \text{ inches nearly;}$$

For the double convex lens—

$$\frac{1}{f} = \frac{1}{2}\left(\frac{1}{5} + \frac{1}{6}\right) - \frac{2}{1\frac{1}{2}}\left(\frac{\frac{1}{2}}{5}\right)^2$$
$$= \frac{11}{60} - \frac{4}{3} + \frac{1}{100} = \frac{11}{60} - \frac{4}{300} = \frac{51}{300}$$
$$\therefore f = \frac{300}{51} = 5\tfrac{9}{10} \text{ nearly.}$$

If t, instead of being, as here, two inches, be sufficiently small to be neglected, then

$$\frac{1}{f} = \frac{11}{60}, \text{ and } f = 5\tfrac{5}{11} \text{ inches.}$$

The principal focal length of a convex meniscus lens (No. 7, fig. 1) is expressed by the following formula:—

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r} - \frac{1}{r'}\right) + \frac{t}{\mu}\left(\frac{\mu - 1}{r}\right)^2$$

For a concavo-concave glass (fig. 1, No. 10) we have

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r} - \frac{1}{r'}\right) - \frac{t}{\mu}\left(\frac{\mu - 1}{r}\right)^2$$

The other cases (fig. 1, Nos. 8, 9, 6, 5), have analogous formulæ, according to the various modifications of the circumstances in which the luminous rays are incident at first, and finally emergent from their various surfaces.—We now see how the various principal focal lengths depend upon μ. The problem of achromatism is this,—to arrange several lenses, so that the ultimate point of convergence will be the same for different but known values of μ. For any one given form of lens, in most of the formulæ given, there would be one focal distance corresponding to every value of the refractive index. Hence the violet of a pencil of white light would be sent to one principal focus, and the red rays and those at the other end of the prismatic spectrum would be sent to another. In some of the formulæ indeed, where μ is found both in the numerator and the denominator, two values of it might be found, which would lead to the same principal focal length. But supposing these to be suited to the values of μ for the red and the violet rays, we

should still have the intermediate rays of the spectrum, with different values, so that achromatism would still be unattained. The introduction of two or more lenses remedies the evil so far. Imagine rays coming from one point, in one direction to be incident upon a given lens,—they become separated, so as to appear emergent from different points, one for each different refrangibility of the prismatic rays. If now, these separated rays came from one point upon the second lens, we should have the very same effect as a reseparation of the constituent lights; but falling as they will now do, in different directions upon the lens, that lens may be so arranged that they shall be reconcentrated. With some lenses, for example, the greater the refractive index is, the larger the principal focal length; with others again, the greater the refractive index, the smaller the principal focal length, under certain limitations. Some lenses, as we have seen, throw parallel light falling upon them back, upon a point before them. Others, on the contrary, carry it forward to convergence at a point behind them. Taking these facts, in conjunction with that readily established, that the refrangibilities of any one kind of substance are not proportional to those of any other, for the different rays, as for example,—

$$\frac{\text{Refractive index at B for Crown Glass, No. 13}}{\text{Refractive index at B for Flint Glass, No. 13,}}$$

is not equal to—

$$\frac{\text{Refractive index at H for Crown Glass, No. 13}}{\text{Refractive index at H for Flint Glass, No. 13;}}$$

we may understand the principles upon which practical opticians found improvements in achromatism. Their aim is simply to obtain glasses, which, while serving to make rays, converge or diverge, will make the rays of different refrangibility, converge to or diverge from the same point.—In treating of achromatism we have found ourselves brought into contact with the more general case, of the use of lenses. Although, for example, the rays incident upon the first surface are parallel, they diverge or converge before reaching the second, and fall upon it in all possible directions. We must examine, therefore, if there be any new principle involved in the case of rays diverging from or converging to a point, not so far distant from the lens as to be considered parallel to it, which has not yet been considered.—The case is very much, in fact, the same as before. The consideration of the principles determining the refraction of converging or diverging rays into a medium, bounded by a spherical surface, has already been taken up. This is exactly the case of the incident rays before us. When passing through it, they may be parallel to the axis of the lens, or converging or diverging. If the former, the case of parallel rays, emergent from a medium bounded by a spherical surface, is exactly the same as that of parallel rays incident upon a medium (the substance into which they emerge), which is likewise bounded by a spherical surface, turned in the opposite direction. We have already considered this. If the latter, the case of emergent rays and of incident rays is exactly the same in such circumstances. It follows, therefore, that no new principle is involved in the case just introduced. We prefer to give a few results rather than processes here also.—The following formula expresses the length of the focus of emergent rays, when a small pencil of diverging rays is incident on a double convex lens. Let r be the radius of the first and r' of the second lens, Q the centre of emission of light, and q' the focus of the rays while passing through the lens. Call q the focus of the rays as finally emergent, and let Q A $= u$, q' A $= u'$, q B $= v$, and A B $= t$, then $\frac{1}{v} = (\mu - 1) \times \left(\frac{1}{r} + \frac{1}{r'}\right) - \frac{1}{u} + \frac{\mu\, t}{u'^2}$, or, calling f the principal focal length, we have $\frac{1}{v} = \frac{1}{f}$

$$- \frac{1}{u} + \frac{t}{\mu}\left(\frac{\mu - 1}{r} - \frac{1}{u}\right)^2$$

We have for the other most frequent case of the double concave lens $\frac{1}{v} = \frac{1}{f} + \frac{1}{u} + \frac{t}{\mu} \times \left(\frac{\mu}{u'^2}\right)^2$. The cases of other kinds of lenses may be solved upon the same principles as these. Their formulæ may be deduced from the formula here given, by introducing the appropriate variations for the values of r, r', &c.—For example a convex meniscus (No. 7, fig. 1), may be considered to be a double convex lens, whose radius

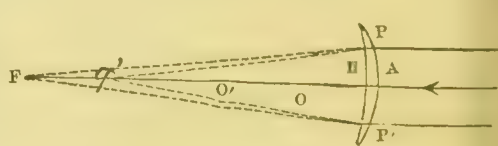


Fig. 6.

r', is negative instead of positive, and where it occurs in the expression we should have simply to add instead of subtracting, and subtract instead of adding. With a convexo-plane lens (No. 3, fig. 1), we should have r' infinite, because the plane may be considered as a spherical surface with an infinite radius. With a plano-convex lens again, we have r negative and r' positive, and so on. All the cases therefore of spherical lenses may be solved by the employment of the foregoing formulæ, by means of the proper substitutions;—one of which is, in fact, deducible from the other, by the same method. The circles of the double concave ar

just those of the double convex *turned*, and those of the double convex have the same relation to those of the double concave. Hence, if in the formula for the one we substitute $-r$ and $-r'$ for r and r', we shall find a formula applicable to the other.—The same method of substitution would enable us to deduce all the cases considered, from the general one of incidence of diverging or converging pencils upon a double convex or concave lens. If the rays be parallel, we have to express that, in the general formula, substituting for the indefinite distance u, the infinite value, and having therefore $\frac{1}{u} = 0$. If parallel rays be incident on or emergent from the various kind of lenses, we have merely to take the two systems of substitution now indicated, together, instead of apart.—Still another case remains for consideration, however much more complex in the mathematical processes necessary for its solution, yet not in the least involving any different physical principle.—It will have been remarked, that whenever we treated of the effects of lenses upon light, the light was either parallel to the axis (the perpendicular to the two lenticular surfaces at the middle of the lens), or emitted from a point in the line of that axis. Now this is by far the most usual case. But no object consists of one point alone, capable of being placed in the axis of the telescope, and except in the case of stars, we have few which appear to consist of a mere point of light. For the sun and moon and planets, and for distant terrestrial objects, and to an extent yet almost inappreciable, for those stars which have a parallax, we must consider light falling upon our lenses in directions other than that of the axis. In observations upon the stars themselves it is frequently necessary (as for double stars), to observe two or more at once, and in this case also, only one can give such rays as we have already treated of. We shall therefore give very shortly the formulæ for pencils of oblique light, falling upon, and refracted by lenticular surfaces.—But we must offer first a definition. The centre of a lens is the point where a line joining the extremities of two parallel radii of its spherical surfaces, cuts the axis of the lens.—One proposition then, of considerable importance in connection with this problem is, that one focus is always in the line joining the centre of the lens with the centre of emission of light. In the case of a telescope, some of the oblique rays will generally be intercepted by the rim of the telescope however, and this result will not be accurately produced.—If the pencil of oblique light be not much inclined to the axis of the lens, the focal length for parallel rays will be in the line named, and at a distance from the centre $= f$, for a double convex or double concave lens—and therefore, since all lenses may be considered as particular cases of these—for all lenses. Hence, we may find the focal length, by describing from the centre, a small arc with a radius $= f$, which will give the focus of all the principal focal lengths for small obliquities.—We have for diverging pencils the following focal lengths:—

$$\frac{1}{q\,\mathrm{C}} = (\mu - 1)\left(\frac{1}{r} + \frac{1}{r'}\right) \mp \frac{1}{\mathrm{Q\,C}}$$

These are the principal cases of ordinary lenses, which come properly under our notice.—Another interesting question, merits notice,—required

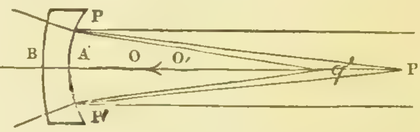


Fig. 7.

the focus of a refracting sphere. We have already considered the focus for a spherical surface, prolonged indefinitely, but in this case we have a twofold refraction, at incidence and emergence, as with an ordinary lens. The expression obtained for it is the following, for parallel rays $q\,\mathrm{O} = \frac{\mu\, r}{2\,(\mu - 1)}$ where $q\,\mathrm{O}$ is the distance of the focus of the refracted rays from the centre of refraction.—The case of a refracting sphere, on which the incident rays are converging instead of parallel, gives expressions precisely analogous to some of those for convex lenses. $\frac{1}{q} = \frac{1}{f} - \frac{1}{\mathrm{D}}$, where q is the distance of the focus of refraction from the centre of the sphere as before, f the principal focal length determined above, and D the distance of the centre of emission of light from the centre of the sphere.—This formula, applied to the case of rays from an infinite distance would give $\frac{1}{q} = \frac{1}{f}$, and $q = f$, as it ought to do. —We have thus seen how lenses operate in making rays from a point converge or diverge, and we have further considered the case of rays falling obliquely on a surface instead of, as usual, parallel to the axis. We required to do this, principally because the objects we usually have to examine through lenses, are not points, but made up of a series of points, forming themselves into surfaces or lines. It is interesting to trace the effect upon the various points which make up these lines together, in order to discover what image is produced by any given lens, of a given object, and at what point or line or surface it is produced. The theory of microscopes and telescopes entirely depends upon the problem now indicated. We can do little more than allude to it. Suppose we desire to find the image produced by a spherical surface, of an object which constitutes a circular arc concentric with the surface. We have found what would be the focal distance of rays coming from any one point of it when the

rays are incident perpendicularly upon the surface; and as the object is a circular arc concentric with the spherical surface, the line from every point to the centre of the spherical surface, is perpendicular to that surface. As, further, the distances of the various successive points from the centre of the surface are the same, and as it is upon this distance that the ultimate position of the focus (or point, to which, the ray is refracted, or, from which, it appears to come) depends, the distance of that focus from the centre of the surface will be the same, for every point, and therefore the arc will appear as a concentric spherical arc also, magnified or diminished in proportion of the distance of one of the foci from the centre, to the distance of the point to which it corresponds from the centre.—The image by reflection of an arc, concentric with the spherical part on which the rays from it directly fall, is also proved to be circular, and the expression for its magnitude, compared with that of the object, is $\frac{2\,OQ - r}{r}$, OQ being the distance of the object from the centre of the lens; and r the radius of the lens which is considered. There are two cases, as the image and the object are placed on opposite sides of the lenticular surface, or upon the same side.

Lenses, Aplanatic. A name given to lenses that practically destroy the effects of spherical Aberration. In no single lens can this aberration be made less than 1·07 of the thickness of the lens; but, by combinations of lenses, it may be much further reduced. Sir John Herschel has pointed out that, by the substitution of two plano-convex lenses with their convexities turned towards each other, the aberration may be reduced to one-fourth of the foregoing amount, provided the focal length of one lens be 2·3 times that of the other.

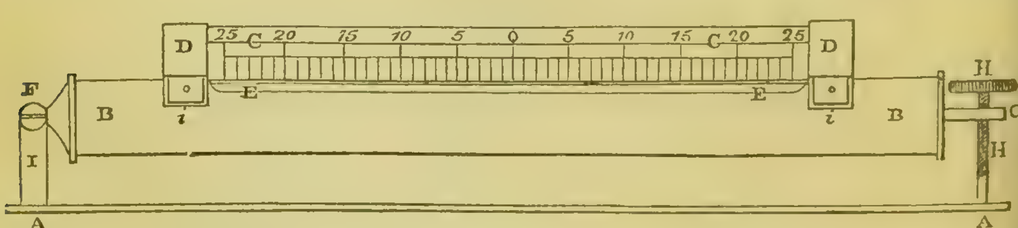
Lenses, Fresnel's. See PHAROS.

Leo. One of the Zodiacal constellations. It commemorates the Nemæan lion which Hercules slew. A line passes nearly through Arcturus, and the bright stars, Regulus (α, Leonis, of the first magnitude), and Deneb (β, Leonis, between that and the second), which are the brightest, and the next brightest of the constellation. A line through Deneb and the Pole Star passes through the lowest of those in the Great Bear (γ Ursæ Majoris). The sign of Leo in the Zodiac is ♌.

Leo Minor. One of the constellations marked by Hevelius. It is surrounded by Leo, Cancer, Lynx, and Ursa Major. It has no large stars.

Lepus. One of the old constellations directly under Orion. Its chief stars α, β, γ, Leporis, are of the third magnitude.

Level. A most important instrument in all practical sciences whose operations are dependent upon a correct determination of the horizontal or vertical point. In the olden times of Practical Astronomy, the vertical point was usually ascertained by the *plummet*, by aid of the contrivance termed Ramsden's *Ghosts;* subsequently the method of direct and reflected observation was resorted to; and quite recently a preference has been given to Bohnenberger's happy conception as explained under CIRCLE. Nothing, certainly, can exceed a long plummet in delicacy; but it is not convenient of application, and its oscillations do not cease soon. It is also frequently requisite to determine a horizontal or level line, when neither of the other methods can be employed. It is necessary, therefore, that we resort to the *spirit level;* nor is this any longer a hardship, on account of the almost wonderful perfection with which our great Artists have succeeded in endowing this instrument. The general character of a *spirit level*, as it is now used, is represented in the subjoined cut. The tube B B is of brass,



moveable at one end on the hinge F, and capable of being moved up and down at the other end by the screw H H. Sometimes the milled head by which this screw is moved, is graduated, so that the observer may know precisely through how much of a circumference the screw has been turned. The glass tube containing the spirit, and in which the *bubble* or empty part is apparent, is placed within the brass tube, the upper portion of which is open, so that the position of the bubble may be seen. Sometimes there is no brass *tube* at all, but only a brass *stand*, on which the glass tube is fixed by its extremities, or, much better, by its *centre*. Attached to the brass tube or stand, and placed right over the spirit level, we have the scale D D, graduated in proportion to the delicacy of the level, and by aid of whose graduations, read off at both ends of the bubble, we gather whether its centre is at *zero*, or by how many divisions it departs from it. Supposing a level well made, the observer adjusts it as follows:—Place it on a *moveable* surface,—its Y's for instance, on the pivots of a transit instrument—and by the screw H, bring the bubble to zero of

the scale. Reverse the level. If the bubble is still at zero, the axis of the transit and the position of the level tube are both alike correct. If, as almost always will be the case, the bubble has departed from zero, ascertain the amount of departure by the scale; divide this by two; attribute half the error to the level, and the other half to the transit; correct accordingly, and again reverse. Both instruments will in this way be speedily freed from error. It also generally happens that the glass tube is not placed absolutely along the line which gives it support, but lies somewhat transversely. This error can easily be detected by giving the level a slight side motion on its Y's; and a transverse screw is always supplied to correct it.—These corrections are easy; it is in the construction of the level that the chief difficulty lies. The bore of the glass tube must be ground to a nicety, so that equal parts of the scale indicate equal angular quantities wherever the bubble is; and instead of the glass tube being straight, it must be an arc of a circle of greater or less radius. The larger the radius, the truer the level: the famous Reichenbach is said to have boasted that he could construct a level with a radius of curvature of *two hundred English miles*, —a feat which, if accomplished, would have enabled the bubble to move over more than five English feet, for a change of one second of space! Without reference to such marvels however, it suffices to know that by aid of the levels of Ertel, Repsold, and some of our own English Artists, an estimate may be safely adventured on of *tenths of seconds* of space: it is only by aid of the Electric recorder that we can expect to approximate such accuracy in our measurement of *space*, through the medium of *time*.—After a level has been constructed with the utmost care, it must be severely tested, and the size of the divisions of a scale suited to it, accurately determined. This is, perhaps, most securely accomplished by placing it on the telescope of a finely graduated circle, when the telescope rests horizontally. A slight motion communicated to the instrument by its micrometer screw, will elevate the telescope one second of space; the displacement of the air bubble can then be fixed; and so for larger quantities.—In sum, the following are the requisites of a good level, and fortunately they are now all attainable.—*First*, the bubble must be long enough, compared with the whole tube, to admit of quick displacement, and yet not too long to admit of its proper elongation at low temperatures:—*Secondly*, the curve must be such, that the sensibility and uniform run of the bubble will indicate quantities sufficiently minute, while those quantities correspond exactly to the changes of inclination, as read on the graduated limb of the instrument of which it forms a part:—*Thirdly*, the bubble must keep its station when the angles are moved a little round the pivots of suspension: —*Fourthly*, the opposite ends of the bubble must vary alike in all changes of temperature; or, in other words, the ends of the bubble must elongate or contract alike in opposite directions, so that the middle point may always be stationary:—*Fifthly*, the angles of the metallic end-pieces must be so nicely adjusted, that reversion on horizontal pivots that are equal, will not alter the place of the bubble:—*Sixthly*, the distance between the two zeros of a fixed scale, when such a graduated scale is used, should be equal to the length of the bubble at the temperature of 60° of Fahrenheit's scale, and should be marked at equal distances from the visible ends of the glass tube. Then as the bubble lengthens by cold, or shortens by heat, its extreme ends in the glass may always be referred to these fixed marks O O on the scale, and will fall either within, upon, or beyond them, according to the existing temperature. The number of subdivisions of the scale that each end of the bubble is standing at, counted from the fixed zero marks, at the instant of finishing an observation, must always be noted, that an allowance may be made for the value of the deviation in seconds, + or —, as the case may require:—*Seventhly*, when the two ends of the bubble are not alike affected by a change of temperature, the scale should be detached and adjusted to the new zero points, by an inversion of the level:—*Eighthly*, when the scale has only one zero at its centre, which is a mode of dividing the least liable to misapprehension, the positions must be reversed at each observation, and both ends of the bubble read in each position; for in this case, if any change has taken place in the true position of this zero, the resulting error will merge in the reduction of the observation. This mode of graduating is generally preferred on the continent.

Levelling. An important branch of general Geodesy, whose object it is to enable the surveyor to draw an exact profile of a district of country. See any great work on Geodesy.

Lever. A solid bar at each end of which a certain amount of force is applied in similar directions, and which is supported on a pivot, or by some fastening between the points of application.—The most intelligible mode of proof for the fundamental proposition in the theory of Levers, is the following:—Let A B be a rigid bar, at the ends of which forces perpendicular to its length

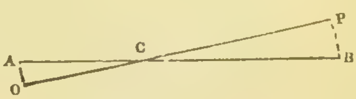


Fig. 1.

are applied, and let it be supported on a pivot, at the point where the bar is divided inversely as these forces,—it is required to show that there will be equilibrium. The maxim on which the proof rests is this: supposing a small disturbance—so small, indeed, as to be less than any amount that may be assigned,—if the work done consequent on the motion at the one end of the lever, be equal to that done at the other, there must be

equilibrium. The grounds of this maxim are manifest. If the work be equal in the two cases, there will be a balance or counterpoise of work, and one arm of the lever cannot have a tendency to move, greater than the other. Assuming this principle, then (the principle of virtual velocities), let us proceed to its application. Suppose a very small displacement of A B, to the position O P, —the same forces still acting. There will be a small curve described—a small circular arc, A O, and another B P. The smaller these become, the nearer they will approach to little lines perpendicular to the end of the lever. In this condition of *limits* we should have two isosceles triangles similar to one another, *i.e.*, A O C and B P C, and therefore, also A O : A C : : B P : B C. Now, A O and B P, are the spaces through which the forces have acted, and as those spaces are so very small that we may consider their direction the same throughout, *i.e.*, perpendicular to the line A B, and, therefore, by hypothesis in the line of action of the forces, we have the work done by F (the force at A) $=$ F $\times$ O A, and that by F′ (that at B) $=$ F′ $\times$ B P. Now, F bears to F′ the ratio of B C to A C. Hence, the proportion which the two amounts of work done in this small disturbance bear one to the other, is B C $\times$ O A to A C $\times$ P B. But, from the analogy already given, A O : A C : : B P : B C, or B C $\times$ O A $=$ A C $\times$ B P; in other words, the proportion that the two amounts of work bear, is that of *equality*, when the lever can only move round the point C, *i.e.*, the point where A B is divided *inversely as the adjacent forces:* there must, therefore, be equilibrium in that case. A case apparently more difficult, readily suggests itself. It is when the lever is acted upon by forces not perpendicular to its line of direction. The line of vertical motion, as we may call it—the very small line through which each extremity of the lever moves, is always, as we have seen, perpendicular to the lever. But here we have a force acting not in the line of motion. The difficulty may be got over by compounding each of the actual forces into two —one in the line of the lever in each case, and the other perpendicular to it. The two forces obtained in the direction of the lever would pull it

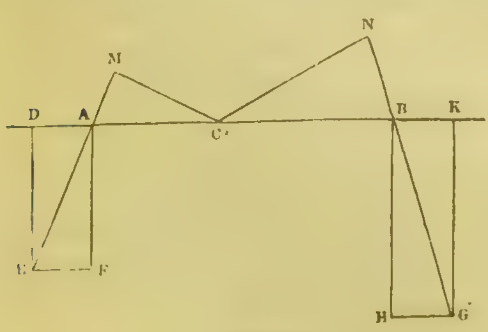


Fig. 2.

in contrary ways, or push it together, compressing it, and therefore would have no power to *turn* it. These forces might, therefore, be struck out, and only a pair left perpendicular to the line of the lever, and in the line of vertical motion. This must reduce them to the last case. It will be evident, that as the original forces E A and B G do not act upon the system in the way of vertical motion; but only the elements, A F and B H, the point C must be at the division of A B, where A C : C B : : B H : A F. This is equivalent to A C : C B : : B G . sin A B G : A E . sin E A B, and as, if we produce E A and G B, and draw perpendiculars from C upon them, we have $\frac{AF}{CM} = \frac{AE}{AC}$, and also $\frac{BH}{CN} = \frac{BG}{CB}$, we obtain E A : C M $=$ B G : C N. Hence, it is a general rule for straight lines, that they are in equilibrium round a point from which the perpendiculars to the lines of direction of the forces are in the inverse ratio of the forces. Sometimes we have a *bent* lever instead of a straight one, the forces lying as in fig. 2. Nothing material is changed by this, for the same laws of equivalence hold. If the figure be like that in fig. 3, we shall have a system which

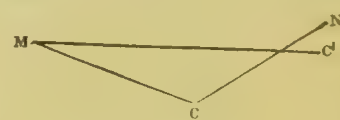


Fig. 3.

will be equivalent to a bent lever, N C M of the former kind. Multitudes of cases, with other modifications of detail, might be introduced; but the principles already expounded must suffice to enable the student to comprehend and solve them all.—The principles upon which we have rested the solution of the problem of the lever, depend upon that of *Virtual Velocities*, and on the use of the doctrine of limits. — The lever is one of the chief mechanical powers. The peculiar mode in which it enables us to modify mechanical processes, will be at once evident.—There are three kinds of lever usually enumerated. We speak of the two forces as the power and the weight; the latter, whether weight or not, being considered the resistance to be overcome; the former, the action which we bring to bear in order to overcome it. The point round which all motions of the lever can alone be made, is called the fulcrum or prop. But it may be a point of suspension, as well as a point on which the lever rests. The three kinds of levers depend on the various relative positions of this prop. Where the fulcrum is in the middle, as in an ordinary crow-bar, one end of which is put below a stone to be raised (*e. g.*, the resistance), and the other weighed on by the body while near the stone, a block of wood is shoved in, round which the lever must turn, the lever is said to be of the *first* kind. If the weight is between the fulcrum and the power, as when we work with a lever fastened at one end to the ground (the fulcrum)

and bearing up a weight somewhere in the middle (the resistance or weight), we have a lever of the *second* kind. The distance between the fulcrum and power, is in this case less than that between the fulcrum and weight, and there is, therefore, always less of power needed, than of weight. In the *third*, the power is between the weight and the fulcrum, as if we lift a bar jointed in the floor—at the free end of which a weight is hung—by a force applied within the bar. We should have in this case always a greater power required than weight to be lifted; and the more so, the nearer to the fulcrum we apply the power. In those instances where we lift a greater weight with a less weight, it is evident that the power moves with a greater velocity and through a greater space. Where, again, we move with a greater power a less weight, we move the power much less rapidly than the last. Hence, that old statement which amounts to an imperfect expression of the principle of mechanical effect, that what we gain in force we lose in time, and *vice versa*. See MECHANICAL POWERS.

Leyden Jar. A very powerful means of procuring electrical condensation, and by its discharge, a brilliant spark and shock. In its best construction, it consists of a glass bottle, coated internally and externally with tinfoil, from which all asperities ought to be carefully removed. If the interior sheet of tinfoil be placed in contact with a positive or negative conductor, it becomes intensely electrified in the same sense as the conductor, while the external portion of the bottle takes on the opposite form of electricity in equal intensity. An approximate contact being established, a discharge and spark occur, proportionate in violence to the intensities of the opposite sides of the jar. The explanation at one time in acceptance, was a very absurd one. It was fancied that the electricity was *bottled up* within the jar, and that the discharge was simply its escape! But the phenomenon is one of pure *induction*, and the instrument belongs to the class under which the *Condenser* is arranged. See CONDENSER for a detailed account of such agencies of *Induction*. —By construction of the Leyden Jar, the electric battery is formed—an apparatus by which the most tremendous of electric effects may be manifested. We believe that the most gigantic battery ever made, is at present in the *Pantecknicon*, in Leicester Square, London.

Libra. One of the old constellations of the Zodiac, surrounded by Scorpio, Ophiucus, Virgo, Centaurus, and Lupus. In the more ancient figuring, Scorpio occupied 60° of the Zodiac—two spaces—his claws occupying one sign, and his body another. The Latins assigned him only one, and placed Libra, or the balance, where the claws of the Scorpio had been; α Libræ, and β Libræ, are of the second, and γ Libræ, of the third magnitude. The star β Libræ, is the vertex of an isosceles triangle, the other angular points of which are Arcturus and Spica, (α, Virginis). Its zodiacal sign is ♎.

Libration. The moon revolves in her axis in very nearly the same time as she revolves round the earth. The consequence is that we see always the same face turned towards us. If we carry a ball painted half white and half black round in a circle, keeping always the same face turned to the centre of the circle, it will be found that there has been one complete revolution of the ball round the axis when the revolution round the centre is accomplished. So, if there be such a revolution in the ball at uniform rate, and if it move round the earth uniformly it will always turn the same hemisphere to us. The irregularities in this are called librations. If the motions of the moon, both orbital and rotatory, were quite uniform, there would be no irregularity; but as they are not so, the moon comes sometimes as it were before us and thrusts a part of its forward hemisphere to our view; sometimes it is behind in its rotatory motion and thrusts therefore a part of its backward hemisphere in view, a part of the forward one falling back away from us. Besides, our Satellite does not rotate on an axis perpendicular to its orbit. The consequence is that at one time we see over one side of its poles, and again over on the opposite side. At one time, for instance, its south and at another its north pole becomes visible. Then, this axis itself is by no means constant in its positions in space, but librates or oscillates just as one sees a spinning top describe a sort of cone with its axis in space. In consequence of this, we see more of the axis at one time than at another, that is, more of the circumpolar regions. As a result of these two distinct kinds of libration—due to the non-uniformity of the orbitual and rotatory motion—and to the deviation of the axis from the perpendicular to the orbit, and from its own regular position—we see at different times a few degrees all round the edge more than the regular visible hemisphere of the moon. This, in its largest sense, is *Libration.*

Light. The physical science of Light, or OPTICS, has for its arduous purpose, to trace and refer to Law, all the complex phenomena of that great Agent or Force, through which the external Universe becomes visible to Man. Under many separate articles in this Dictionary, the various classes of these phenomena, are treated with the care due to them, and in as great detail as the space at our command could permit. The student is referred to CATOPTRICS, DIOPTRICS, RADIATION, REFRACTION, DOUBLE REFRACTION, DIFFRACTION, INTERFERENCE, POLARIZATION, SPECTRUM, DISPERSION, ABSORPTION, COLOURS, &c., &c. We intend at present merely to state, and compare—by the test of a few of the singular changes undergone by light—the theories which have been proposed regarding this remarkable agent. What then is Light? or rather, how and by what instrumentality, is that in-

fluence propagated which enables visible objects to affect the eye? Two replies have been offered to this inquiry, of an entirely opposite kind. One form of answer, is the basis of the Newtonian theory of light, or the theory of *Emission:* according to Newton, material particles of impalpable tenuity are sent forth from the luminous body, traverse space with the immense velocity of about 198,000 miles in one second of time, and—on arriving at the eye—enter the pupil and impinge against the retina. The *second* theory is that of *undulations*, or vibrations. When a stretched wire is struck at one end, the vibration, due to the elasticity of the wire at the end of it that is struck, immediately propagates itself along the wire, by a succession of corresponding vibrations: now, it is clear that in such a case there is no transmission or movement of *translation* of the material particles of the wire. They simply vibrate up and down; and all that is transmitted is this motion or vibration. In the same manner, if a stone be thrown into a quiet pool, we instantly perceive that a series of circular waves is propagated from the centre of disturbance: neither in this case, however, is there transmission of particles, for if any light body be placed in the way of these waves, it is not carried on by them as if they formed a current, but only rises and falls with the crests and troughs of the waves. Exactly thus with regard to the propagation of sound. Sound creates no wind or current in the atmosphere, but only a series of *pulsations* of varying amplitude. Like the theory of *Emission*, that of *Undulation* has its fundamental hypothesis. It is this. Throughout all space, there is diffused an impalpable *Ether* or medium, of nearly infinite elasticity, and therefore capable of being affected by undulations that propagate themselves with almost miraculous swiftness: this Ether is thrown into vibration by a luminous body, just as the atmosphere can be thrown into pulsations by a sonorous body; and, as these vibrations or light-waves reach the eye, they affect it with the sensation of sight. Further, the elasticity of this Ether is uniform through every region of space that is not occupied by ponderable matter,—hence the velocity of light is uniform. This uniformity does not hold, however, in the interior of bodies. The same Ether is there, but its density and elasticity are not the same; these vary with the molecular constitution of the bodies; and within the greater number of crystalline masses, the elasticity of the ether changes with the *direction*. The question cannot fail to arise, What is the nature of these supposed vibrations—are they like those of *sound*—along the line of propagation, or like those of a stretched cord perpendicular to that line or radius? The answer will be found under UNDULATORY THEORY; where we shall show how much of the resources of that theory depends upon the reply given to the problem now started:—it is sufficient here that the student obtain a distinct conception of the general basis of that theory.—The strict logician may probably be disposed to challenge the legitimacy of either mode of speculation. Both theories involve a *physical*, not a mere *formal* hypothesis, concerning the reality of which, no direct physical proof can ever be attained. In the one case we have these streams of impalpable *particles*; in the other an impalpable and therefore unknowable *Ether*. The logical province of an hypothesis is to shape or guide inquiry towards what may afterwards be established by unquestionable induction; and in this way *formal* hypotheses, or hypotheses regarding *laws*, have eminently subserved and accelerated the march of discovery. But hypotheses as to the existence of *substances*, which, from their very nature, must ever elude detection, although not new in the history of physics, have certainly never hitherto achieved a permanent place in pure science. Passing, however, from these general considerations, and amply acknowledging the services of both theories —whether they shall turn out provisional or otherwise—in pushing forward our acquaintance with the facts and laws of Physical Optics, we hasten to establish something concerning their comparative merits.

(1.) *Comparison of the theories, through their relation to the rapidity of the propagation of light, and its rectilinear course.*—It is incontestible that the enormous velocity of the propagation of light, must stagger the adherent of the theory of Emission. We can conceive a medium or Ether of vast elasticity, through which propagation by waves might take place with any degree of swiftness; but assuredly it is next to impossible to imagine material molecules, of magnitude so small, that countless millions of them moving at the rate of 198,000 miles in an hour, can meet in the focus of a lens without communicating the slightest mechanical impulse to any body placed there, or even acting on each other as they cross and recross. But further still, it is demonstrable that light issues from all bodies, whatever their size—and whether self-luminous or shining by reflection—with precisely *the same velocity*. Now, the force which propels them from the surface of any orb, must, if they are material, be modified by the attraction of the orb; so that as the celestial bodies vary so much in size, we ought to expect the velocity of the light issuing from them to vary likewise. According to the computation of Arago, a fixed star of the same density of our sun, but 250 times larger, would utterly destroy the notion of an emitted luminous particle; on which account the orb would remain for ever invisible. In apparent counterbalance of the foregoing preponderance of probability on behalf of the undulatory hypothesis, it seems, on a *prima facie* view of the case, that the doctrine of emission can alone rightly explain the rectilinear propagation of this great agency, and the phenomena of shadows. When no obstacle occurs,

this propagation along straight lines is indeed perfectly compatible with the idea of advance by vibrations; but as waves pass round the corners of obstacles, it would appear that the intervention of any obstacle ought—if the undulatory theory be true—to interfere with the law of rectilinear propagation, and that shadows should not exist. This seeming or *prima facie* presumption on behalf of Newtonian theory, disappears however, when the facts are closely observed and their significance analyzed. *Shadows do not exist*, according to the common apprehension of them. Let A be a luminous point, and B an opaque obstacle, it is commonly imagined that the space within the truncated cone B B′ C C′ is bereft of light or in total darkness; while the real case is very much the opposite. As explained at length

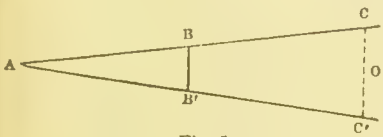


Fig. 1.

under DIFFRACTION, fringes of alternating light and dark spread out within C C′ to a considerable space; and Arago, by an ingenious experiment, showed that if B B′ is an opaque circular disc of small dimensions, the very centre, O, of the presumed shadow is always *a bright spot.* The theory of emission has grappled indeed with this serious difficulty, but by no means successfully. It seeks to explain these fringes by *Inflexions*, or by supposed attractions and repulsions between the stream of light-particles and the edges of the obstacle B B′, or the edges of the slit through which the luminous beam is introduced. The substitution of the mirrors of Fresnel in diffraction experiments utterly destroyed the latter form of the hypothesis; and the supposed action of the edges of B B′ on the particles of light that pass it, requires so many new suppositions to endow it with any degree of probability, that it cannot be said to present serious claims on the assent of the rigorous Inquirer. Now, the existence and entire characteristics of all such fringes—interior as well as exterior—are firmly comprehended by the theory of undulations (see DIFFRACTION); as likewise the necessity of shadows. These light-waves do turn round obstacles, not certainly without diminished intensity; even as the pulsations of sound are propagated round obstacles but with feebler intensity. But Fresnel and others since his time have—with their hands on the wave-theory—demonstrated that all the portions of those lateral waves which do not go to the production of fringes, are destroyed through effect of *Interference.* The theoretical shadow is, in fact, the shadow that actually exists.

(2.) *Comparison of the rival Theories in their relations to the Reflection and Refraction of Light.*—It will be necessary to exhibit, in the first place, the mode in which the fundamental laws of reflection and refraction are explained by these two theories. In terms of the theory of *Emission*, the explanation is very simple. Reflection of light, according to this view, is merely the rebound of a perfectly elastic substance from a plane, on which it has impinged. Let A B be the

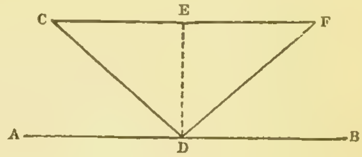


Fig. 2.

reflecting plane, and C D the direction and measure of the velocity of the impinging ray. The motion of the particles constituting the ray may be decomposed into two—the horizontal motion C E, and the vertical motion E D. The horizontal motion cannot be effected by the impact, and will therefore be represented by E F = C E, after impact. The vertical motion E D must after impact be represented by an equal and opposite vertical motion D E, in consequence of the perfect elasticity of the ray; so that the actual path of the reflected ray will be the resultant of D E and E F, or D F. Hence $\angle$ F D E = $\angle$ C D E; in other words, the *angle of reflection is necessarily equal to the angle of incidence.* The fundamental law of *Refraction* is also deduced, on the ground of the emission hypothesis with every facility. The law is this: *The sine of the angle of incidence has to the sine of the angle of refraction, always the same ratio for the same medium.* The process of deduction, however, rests essentially on this, that when an incident ray leaves a less refringent medium to enter one of higher refringency, *its vertical velocity is* AUGMENTED.—The same laws are deducible from the theory of *Undulation.* Suppose $m\,n$ the front of a light wave about to impinge on the reflecting surface A B, and meeting it first at m. Each portion of that wave must, as it encounters the surface A B, become the centre of a new set of spherical waves, which will travel backwards with their former velocity, since the *medium* and of course the elasticity of the Ether has

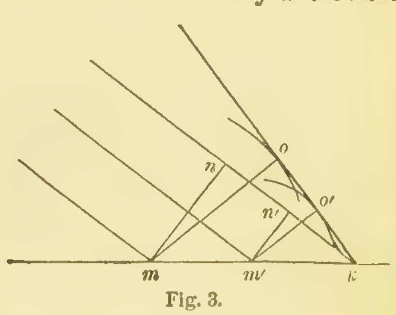


Fig. 3.

not been changed. When n therefore has travelled as far as k, the front of the wave flowing from m, must be in the circumference of a circle whose radius $m\,o$ is equal to $n\,k$; and in the

same way when n' has travelled to k, the waves from m' must be in the circumference of a circle whose radius $m\,o' = n'\,k$. Now the surface which at each instant *touches* all these circles is the front of the reflected wave; and since $m\,o$ and $m'\,o'$ are proportional to $m\,k$, $m'\,k$, this surface is the tangent plane passing through k. Further, since $m\,o = n\,k$, and the angles at n and o are right angles, we shall have $\angle\, n m k + \angle\, o k m$, which signifies, as before, that the angle of incidence is always equal to the angle of reflection. Like the foregoing simple demonstration, the application of the undulatory theory, to the laws of refraction, is due to Huyghens. As before, suppose $m\,n$ the front of the wave pressing towards the line A B, the boundary between two media. When the portion n reaches k, the parts m and m' will have become the centres of waves pro-

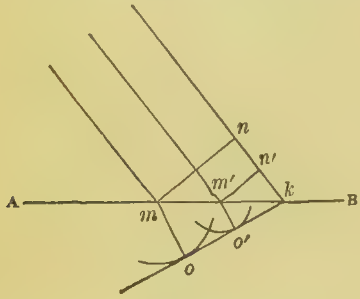


Fig. 4.

pagated within the new medium. The proportions which $m\,o, m\,o'$—the radii of these new waves —bear to the distances $n\,k$ $n'\,k$ will evidently be the ratio of the velocity of propagation in the two media; and the tangent to the circles $k\,o'\,o$, will, as before, be the front of the new wave. But sin. $n\,m\,k$: sin. $m\,k\,o + n\,k : m\,o$, that is, the ratio of the sine of the angle of incidence to the sine of the angle of refraction is for the same two media equivalent *to the constant ratio of the velocities of the refracted and the incident waves.* It is scarcely necessary to draw attention to the fact that when the light-wave passes from a rare to a dense medium, or more properly from a less to a more highly refringent one, *its velocity must be* DIMINISHED, in consequence of the decreased elasticity of the portion of the Ether within that medium. As to this part of the question, then, the two theories are in direct conflict.——There are two points of highest importance to which attention must be drawn, if the relative values of the conflicting theories are to be tested by their powers to explain the phenomena of reflection and refraction. (1.) The simple act involved in either phenomenon separately, and its laws, appear indifferently within reach of both. But there is a complex case, to which the doctrine of Undulation alone satisfactorily applies. *When a ray is refracted, part of it is reflected also.* Whence this double effect? That the Wave-theory recognizes not only its possibility, but its necessity, a glance at fig. 3 will amply suffice to make manifest;—m and m' being centres of disturbance from which waves must emanate, it is clear that one wave will pass backward into the old medium with its former velocity, forming the wave of reflection, while another must pass into the lower medium, with its new velocity, forming the wave of refraction. But if the Newtonian doctrine be correct, why do not the whole of these light corpuscles pass into the new medium?—why are some accepted only, while others are driven back? Can the same medium act differently on the same set of corpuscles—attracting a portion of them, and repelling another portion? The difficulty now adverted to, gave rise to one of the most ingenious, but at the same time one of the most artificial and unsatisfactory portions of the theory of emission,—Newton's theory of *fits.* According to this illustrious inquirer, these light-molecules, as they traverse space, are found in two opposite conditions or states. In the first of these states or *fits*, they are disposed to permit themselves to be readily *repelled*,—this is the *fit of easy reflection:* in the second state they readily yield to *attractive* influences—this is the fit of *easy transmission.* But since it may be expected that in every mass of such molecules, some will be in one state or fit, and some in the contrasted one, it follows that the acts of reflection and refraction may or even must co-exist. To explain the origin of these opposite fits, Newton felt it necessary to introduce further the hypothesis of an Ether, whose vibrations were propagated with a velocity greater than the velocity of light. This ether attaches itself, so to speak, to the light-corpuscles; forcing them into some one of the two foregoing states, according as its vibrations concur with, or counteract the original progressive movement. Newton even computed the elastic force required for this Ether. Surely his followers might long have seen, that all the difficulties of the system of Undulations were thus added to the almost insuperable difficulties of other kinds inherent in the theory of Emission. —(2.) The second important point is this:—the two theories, as we have seen, are in conflict as to a question of fact. In the theory of *emission*, the velocity of the light molecules is *increased*, when they pass into a more highly refringent medium. According to the doctrine of Undulations, the propagation of the light-wave, under the same circumstances, is *retarded.* With which does the truth lie? A question as to substantive fact and to be answered by direct experiment. In (1.) I. of INTERFERENCE, an experimental reply, founded on the position of the Fringes of *Diffraction*, has already been given: but, quite recently, the fact has been determined, without regard to any theory or speculation whatsoever, by MM. Foucault and Fizeau. The methods and results of these very able physicists, are fully explained under our next article,—LIGHT, VELOCITY OF. Suffice it to state

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here that these results have finally and conclusively decided the crucial question on the side of the Theory of Undulations.

(3.) *Dispersion of Light, according to the rival Theories.*—It is well known that when a beam of solar light, or other compound light, falls obliquely on a refringent substance, that ray is not only refracted, but separated into parts having different colours; a separation rendered extremely palpable by the prism, and manifesting itself in the SPECTRUM. This fundamental fact is named the dispersion of Light. Both theories take account of it in the manner peculiar to them. According to the doctrine of Emission, the compound ray consists of molecules of different natures, and which, through effect of that difference, are attracted *more* or *less* by the refringent medium. The vertical components of their velocities are therefore variously altered on entering that medium, and on emerging from it they necessarily pursue different paths,—are separated or dispersed. With the Undulatory Theory, on the other hand, the fundamental proposition is this:—a Light-wave is not homogeneous, but consists of a number of different waves of different lengths, each having a rapidity of vibration peculiar to itself; and from this proposition, the necessity of dispersion is, as we shall soon discern, readily deduced. Now, it may appear, at first sight, that both suppositions are gratuitous,—simple additions to the fundamental hypothesis, framed to suit the new case, or to explain the new phenomenon. There is, however, a memorable logical difference between them. It will be recollected that, in the corpuscular theory, the conception of *fits* was fabricated to meet one special class of phenomena; and, in the same way, we have now the notion of different kinds of molecules with different chemical or physical affinities introduced for the benefit of another class. It is, on the contrary, the distinguishing characteristic of the Wave-Theory, that the modifications required for explanation of some novel facts, have, in so far as has appeared hitherto, been suggested by other facts, or classes of phenomena of quite a different nature. Of this peculiarity, and the signal advantage bestowed by it, there is no better instance than the present one. If Grimaldi's fringes—or the alternation of bright and dark bands, when a ray of homogeneous light is employed in the fundamental experiment of *diffraction*—be rightly explained by the general principle of *Interference*, then the *coloured* fringes, or the resolution of the solar beam into *spectra of diffraction*, demonstrates beyond a doubt that the lengths of the undulations, causing the different colours, are different. Nay, this spectrum of diffraction yields accurate measures of the lengths of these various waves. The table now subjoined offers in round numbers the facts regarding these heterogeneous undulations or waves, as deduced from measurements of this spectrum, and other considerations:—

COLOURS.	Length of the Undulations in Parts of an Inch.	Number of Undulations in an Inch.	Number of Undulations Per Second.
Extreme red, ...	0·0000266	37640	458,000000,000000
Red, ...	0·0000256	39180	477,000000,000000
Orange, ...	0·0000240	41610	506,000000,000000
Yellow, ...	0·0000227	44000	535,000000,000000
Green, ...	0·0000211	47460	577,000000,000000
Blue, ...	0·0000196	51110	622,000000,000000
Indigo, ...	0·0000185	54070	658,000000,000000
Violet, ...	0·0000174	57490	699,000000,000000
Extreme violet, ...	0·0000167	59750	727,000000,000000

The question regarding the composite nature of the light-wave being thus determined, to a large extent independently, the question recurs, in what manner does this bear on the phenomenon of *dispersion?* Now, so long as these various waves were supposed to be propagated with *equal velocities*, it mattered not that they had different lengths and different periods of vibration: the student will gather from section (2) of this article, that their refracted paths must have coincided as well as their incident paths, and that there could be no dispersion. But it remained the received opinion, that the velocity of propagation did not at all depend on the length of the wave, but only on the elasticity and density of the Etherial Medium. At last the veil was raised from before the difficulty, by the powerful analysis of M. Cauchy. The theorem, that the propagation of light-waves must, within the same medium, be independent of the lengths of these waves, is an approximation only, and issues from the mathematical fiction that *the sphere of the action or vibration of the separate molecules is infinitely small, compared with the length of the wave.* This fiction is convenient, and enables the analyst to evade difficulties; nor is it objectionable so long as general problems concerning the propagation of waves alone are before him. But in matters of ultimate delicacy we cannot rest with approximations; and at the instigation of his friend

Coriolis, M. Cauchy recommenced the investigation, and took account of terms previously neglected. The result is, that these waves have different velocities, and, therefore, that they must be *dispersed* on entering a refringent medium obliquely. Not only does theory evolve this satisfactory result, but, in its details, it quadrates with phenomena:—the numbers deducible from M. Cauchy's series, corresponding far within the limits of inevitable error, with the corresponding numbers according to the actual measurements of Frauenhofer. It were wrong to omit notice of the great services of Professor Powell, as to this interesting question. By him chiefly, were Cauchy's results brought to the testing form of numbers. Having obtained from the general expression of the French analyst, a formula showing the relation between the refractive index of a ray, and the length of a wave or the colour of light, he showed the entire correspondence between its theoretical consequences, and the facts recorded by the great optician of Munich for ten different media, as well as those obtained subsequently by M. Rudberg for ten other cases of crystals.—The reader is referred to Professor Powell's own very lucid treatise, as well as the celebrated *Memoire sur la Dispersion* by M. Cauchy. Professor Kelland of Edinburgh has also written an interesting paper on the same subject, in Cambridge *Philosophical Transactions*, vol. vi.—Our very least conclusion must be in the words of Dr. Whewell. "The result of such calculations shows very satisfactorily that there is not in the fact of *dispersion*, anything which is at all formidable to the Undulating Theory."—It is scarcely requisite to remark, on the analogy between the general theory of colour, and the received doctrine concerning the nature of sound. Only, the limit or range of the *eye* is much more limited than that of the *ear*. As to colour, the ratio of the extreme vibrations that affect us, is only that of 1·58 : 1, whereas the ear can readily perceive and appreciate, a sound, its octave, its double octave, its treble octave, &c., &c. No doubt that beyond either terminus of the visible spectrum, vibrations fall, too slow or too rapid to be apprehended by the Eye.—In other articles of this dictionary—especially POLARIZATION, REFRACTION DOUBLE and CONICAL, and above all in the article UNDULATORY THEORY, several expositions will be found, cognate to the foregoing;—the latter article being wholly occupied with discussions concerning the intimate nature of these supposed vibrations, and of the medium within which they are imagined. Enough for present purposes has here been said.

Light, Equation of. The allowance to be made for the time occupied by light in traversing a *variable* space.

Light, Velocity of. In our notice of the Satellites of Jupiter (see JUPITER) it is mentioned how certain apparent irregularities as to the occurrence of these Eclipses, led the Astronomer Rœmer to the memorable discovery that Light has a definite velocity,—travelling through the space between the Earth and that Planet at the rate of 198,000 miles in one second of time. This same velocity appears to obtain everywhere within the sphere of our Solar System, and there are grounds for the presumption that it holds good, or very nearly so, through all the remoter interstellar spaces. But although this important question seemed settled with every attainable satisfaction, another remained, of greatest moment in Physical Optics,—the question, indeed, on which a right decision between the two opposing Theories of Light, appeared to depend,—viz., does Light move with the same velocity through different media; and, if not, can we ascertain and accurately state the difference of its varying velocities? Considering the amazing average velocity of light, it might well have seemed adventurous if not hopeless, to search a definite measure of small variations in that velocity; nevertheless the difficulty rather stimulated than repelled Arago, to whom we unquestionably owe the impulse that has resulted so auspiciously. It has already been explained, how this philosopher and Fresnel attempted the solution by measurements of displacements of the fringes of diffraction; but Arago was stirred towards the effort to obtain a direct and wholly *untheoretical* measure, by the success that attended the labours of Professor Wheatstone in relation to the velocity of Electricity. The principle of the turning mirror, applied by Wheatstone (see ELECTRICITY, VELOCITY OF), took strong hold on Arago's imagination; and in connection with M. Breguet he had devised a mechanism very similar to Wheatstone's, capable of *virtually* turning a mirror three thousand times in a second, and from whose action he fondly expected the coveted results. Unhappily, dimness of sight—one of the few infirmities of his age—overtook the illustrious French Physicist, before his projected experiments could be realized: but he lived to know that his ideas had been successfully carried out (and in a way much more efficient than by the plan he had proposed) by his ingenious countrymen Foucault and Fizeau. The investigations of these inquirers were conducted apart, and depend on different principles. (1.) The researches of M. Foucault rest on an ingenious contrivance, by means of which—through effect of a combination of spherical and plane mirrors—he made the image of an object coincide with the object itself, so that both could be seen at one and the same time; the image being formed by successive reflections of the direct rays, and *after these rays had traversed a considerable space.* One essential portion of the apparatus was a plane mirror, so placed, that although its inclination were in any way changed, the coincidence of the image with the object would not be disturbed. Nay, if the transit of Light were an *instantaneous* act, this mirror might even be made to rotate on its centre with any degree of velocity and still no disturbance would take

place. Light, however, not being instantaneous in its transmission, it is easy to see that the very *rapid* rotation of this mirror might be made the means of separating the image from the object, because the ray issuing from the same point might come to be reflected by it in two different positions, on account of the time occupied in the ray's passage through the long devious route caused by several reflections. On this simple principle Foucault constructed that ingenious and altogether adequate mechanism, described at full length in his own memoirs, and in the last edition of Pouillet's *Physique*. He immediately applied it to detect the difference of the velocity of Light when passing through air and water. The original *object* was a small square diaphragm crossed in the middle by a fine platinum wire. In this case he made two images of the object coincide, and examined them through an ordinary eye-piece. When the mirror was at rest the two images appeared as under. The bright one —the image through the air—overlapping the centre of the darker one, or of the image derived from the ray passing through water. The platinum wire will be noticed passing through the centre of both. The mirror was then put in rapid rotation; and the images appeared as below. Both were displaced, showing that light has a definite velocity, or takes time to travel; but the image formed by the ray passing through the tube of water was *displaced the most:* thus proving beyond a doubt that light travels more swiftly through air than through water; signally confirming Arago's deductions from the displacement of the fringes of diffraction; and settling or ever this cardinal point of difference between the ival theories of Light. Foucault further computed he amount of the difference, and thereby determined the relative indices of refraction of the two media.—(2.) The principle of the apparatus of Fizeau is very different. He also produced an image of an object by rays which, by aid of several reflections, had been made to pass through long distances; but instead of a rotatory mirror he employed a toothed wheel, which had been made to rotate with immense rapidity by M. Breguet. Placing this wheel so that its teeth should coincide with the focus of the returned rays forming the image, he caused it to rotate until no image could be seen—until in fact the image was eclipsed: and a part of the apparatus informed him of the velocity of the wheel's rotation at the moment when that end was gained. It is very evident that this velocity must be a direct index of the velocity of Light;—had Light passed from point to point instantaneously, there could have been no eclipse. Fizeau immediately entered on the same course of investigation as Foucault, and with the same results. But he has further started on a new career,—investigating the effects of the *motion* of the medium through which the light-wave passes, on the velocity of its propagation. These effects in the case of *flowing* water are palpable, or rather considerable and perfectly regular, so that for the first time science has obtained, through decisive experiment, a knowledge of the effects of the motion of ponderable matter on the velocity of light.—It cannot be doubted, that from the sources now indicated, we may gather most important and unexpected facts regarding the constitution and relations of these—at present—rather hypothetical *Ethers.*

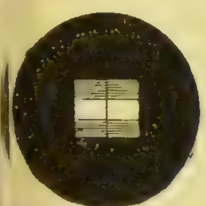

Fig. 1.

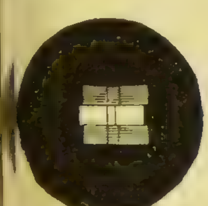

Fig. 2.

Lightning. A spark of electricity, disengaged during an electric discharge, either from cloud to cloud, or between the clouds and the earth. It is easy to distinguish these two kinds of lightning. If the lightning joins two clouds at unequal heights, the sky is irregularly illumined: but, if it passes from a cloud to the earth, we observe a narrow fork of dazzling light, quite limited, and bordered by a sort of halo. Arago distinguishes three kinds of lightning.—(1.) Zig-zag flashes, that frequently bifurcate or trifurcate at their extremity: they may sometimes divide into a greater number of parts.—(2.) *Sheet Lightnings*, presenting themselves as lights that illumine the outlines of the clouds: these are most common when the air is very moist, and they do not appear to have very great *tension.*—(3.) *Ball lightnings* or *globes of fire:* these move slowly from the clouds to the Earth, and are visible for several seconds. M. Arago demonstrates, that lightnings of the first and second classes do not last for the millionth part of a second:—their length or continuity, therefore, depends on the principle of the persistence of images on the retina.—Lightning generally moves from the clouds to the earth, but on occasion, the earth is the originator of the discharge, and the lightning seems to move upwards. The writer of this notice, was once witness of a remarkably brilliant and continued discharge of this kind, while crossing Shap-fell.—The colour of lightning is generally a dazzling white: sometimes, however, it verges towards violet. We know that the colour of the ordinary electric discharge varies with circumstances. See Electrical Egg.—When lightning falls to the surface of the Earth, it naturally follows the best conductors, chiefly attaching itself to metals. This rule, however, is not absolute. The absolute rule is, that it follows the *line of least resistance.* It will leave, therefore, a long but tortuous good conductor, for a less perfect one, that, by its comparative shortness, offers in reality a less obstructed route. If lightning meets with bodies that are bad conductors, it pierces them, breaks them and scatters them

about with irresistible force: instances of these its most disastrous effects, are unhappily too numerous.—This meteor is assuredly the most imposing of any in the Heavens; and it long passed, and in many places, or rather, with the ignorant in all places, it passes still, as a sign of celestial wrath. Listen to the wisdom of old Lucretius:—

> "Postremo, cur sancta Deûm delubra suasque
> Discutit infesto præclaras fulmine sedes
> Et bene facta Deûm frangit simulacra, suisque
> Demit imaginibus violento vulnere honorem?
> Altaque cur plerumque petit loca? plurima quo plus
> Montibus in summis vestigia cernimus ignis?"

For something on the *odour* of lightning, and the electric odour in general, see OZONE.

Lightning Conductors. The disasters often due to powerful Atmospheric-Electrical discharges led very early to the question, whether means for security could be devised. The points at one time in doubt as to *Thunder-rods*, are now well-nigh settled. They ought to be pointed at the top, connected at the bottom with some layer of the earth considerably below the surface, and every metallic portion of the roof to be protected, ought to be joined with the conductor. These rods or metallic strips should likewise be of considerable dimensions—a thin rod, as Faraday has shown, presents so much resistance, that lateral discharges of a destructive kind, are not prevented by it.—At best, however, the Thunder-rod is only a protection within a very few feet of its position, unless it be literally connected by sufficient strips of metal, with the other parts of the roof. In proof, as quoted by Sir William Snow Harris, the mainmast of H.M.S. Endymion was protected, the foremast not: and a flash of lightning striking the latter, shivered it to atoms.—To the excellent Inquirer just named, we owe our best arrangement of *Marine lightning conductors.* A continuous rod, applied to the ship's mast, is impracticable, because of the necessity of elevating or lowering the masts; nor is a chain altogether suitable, inasmuch as every transition from link to link—unless they are pressed together by extreme tension—is a break, to a certain amount, in the chain's conductibility. Sir William overcame the difficulty, and imparted all the advantages of fixed conductors, to those used at sea, by a very simple and ingenious contrivance. He inlays a slip of copper along the entire length of each mast; and these slips are so arranged, that whatever the elevation or depression of the mast, the perfect metallic contact between the adjacent ones, is maintained. The lower portion of the conductor terminates in the sea.—No doubt now remains as to the entire efficacy of this excellent contrivance; experience has pronounced its verdict, for many ships have thereby been preserved from destruction. We desire most earnestly to see it adopted as universally by our Mercantile Marine, as it has been by the Navy. Old prejudices still exist however, and men are even yet found to allege that such conductors are dangerous, because they *attract* the lightning. No conductor *attracts* lightning; it merely furnishes it with a safe road to the earth, when it strikes a ship or a building.

Limb. The edge of a planet is called its *limb*, and so also the edge of any circle of an astronomical instrument.

Limits, Doctrine of. The principle upon which the processes of the integral calculus, and most of those of the differential calculus depend is this, that when we are unable by the application of ordinary means to arrive at any conclusion regarding a certain object—as, regarding the exact length of a curve—we may suppose that object replaced by another, which can be conceived to come quite indefinitely near to it; and apply our reasoning to that instead. Thus, to take the case of a curve, whose length we want to know, we may substitute for the curve A B, a polygonal line A D B, which is very nearly of the same length, and calculate the length of that line. Evidently this will not give quite the result we want, but we may take still smaller sides—as, for example, the polygonal line A C D E B. If we can calculate the length of this, it is evidently nearer to that of the curve, though still not exactly equal to it. If we suppose points taken between A and C, C and D, &c., all joined successively with A, C, &c., we shall evidently have a closer and closer approach. The smaller each little side of the polygon becomes, the larger manifestly will be the total sum, and the nearer to the ultimate value we desire.

Fig. 1.

Now, if we suppose the little sides to be quite indefinitely, or, indeed, infinitely small, it is manifest that our result will be indefinitely or infinitely near to the one which we desire, and we can therefore take it without practical error, if it be indefinitely near—without any error, if infinitely so—for that which we have been seeking.—The purpose, then, of the doctrine of limits is, that by it we may be enabled to substitute in our reasonings concerning matters which we cannot accurately grasp, others which we can; and that having concluded our processes regarding these latter, and obtained results regarding them, we may then apply, instead of the general values of this latter, which have been employed, those special values which come immeasurably or infinitely near the original subjects of thought; and so, obtain special results absolutely true for these limiting cases, and therefore indefinitely near the truth—so near that the error shall be capable of being made less than any given quantity, however small.—The doctrine of limits may be readily understood by aid of the calculus:—Suppose we have an unknown quantity x, and two functions of it, $F(x)$ and $f(x)$, that is, two quantities whose algebraical expression con-

ains x, and whose value, therefore, depends on the ·alue that may be assigned to it. Then, let it ·e supposed that we have two definite values of

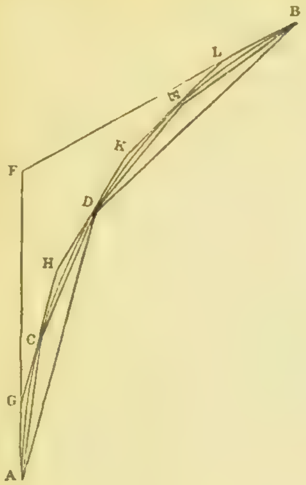

Fig. 2.

ıe variable x, which differ by h; suppose x and $+ h$, it is required to find the value of the ·tio which the difference of $F(x+h)$ and $F(x)$ ·ars to that of the difference of $f(x+h)$ and (x). This is quite a general functional pro-em, and, so far, has nothing to do with the ·ctrine of limits. But, suppose that we want know what this value will become when h be-mes indefinitely or infinitely small. This is a oblem where limits are introduced, and one here the rules established upon the doctrine of :nits, in that branch of mathematical science rmed the differential calculus, at once come our aid. The true logical foundation of is calculus is unquestionably the doctrine of mits; nor have our theories regarding it any gnificance independently of it. See the article LCULUS; also Dr. Whewell's excellent treatise the *Doctrine of Limits*, or the preface to De organ's *Calculus*.

Liquid is that condition of each internal part a body, which consists in tending to preserve lefinite volume, and resisting change of volume, d in offering no resistance to change of figure. :It is known that most substances, and believed at all substances, are capable of assuming the uid condition under suitable circumstances.—ıe property of offering no resistance to change figure, is common to the condition of *Liquid* d *Gas*, and constitutes the *fluid* condition. ıe liquid condition is distinguished from the seous by the property of tending to preserve a inite volume, whereas a gaseous body tends to pand indefinitely.—It follows from the property not resisting change of figure of the internal ·ts, that a liquid mass can neither resist nor ert a pressure in any direction except perpen-ular to its surface, and that the pressure ex-ed at a given point in a liquid mass, between the two portions of liquid on either side of any surface passing through that point, must necessarily be perpendicular to that surface, and of equal intensity, in what angular direction soever the surface traverses the given point. This principle is the foundation of those branches of mechanics which are called *Hydrostatics* and *Hydro-dynamics* respectively—The *compressibility* of a liquid, that is to say, the fraction by which the volume of a liquid mass is diminished by the application of a pressure of a given intensity to its external surface, is for all substances very small. See ELASTICITY, §§ 4, 5. With respect to resistance to compression, every liquid is *perfectly elastic*. See ELASTICITY, § 8.—Every liquid possesses a certain amount of *tenacity* or *direct cohesion*, whereby its parts resist separation by being directly torn asunder.—This cohesion has been proved to be the result, in whole or in part, of an attractive force between the particles of the liquid, which acts at appreciable, though exceeding small distances; in consequence of which there exists, at the external surface of every liquid mass, a layer or film of liquid, of unknown, but exceedingly small thickness, which is of somewhat less density than the internal mass of liquid, and consequently in a state of tension. This superficial tension is the force which sustains a hanging drop; and its amount may be computed from the weight and dimensions of the largest drop of the liquid which can hang. It causes the surface of every isolated mass of liquid (such as a falling drop), or cavity in a mass of liquid (such as an air-bubble), to contract to the smallest possible dimensions, and consequently to assume the figure of a sphere. It also causes the surface of every isolated jet of liquid to tend to assume a form of circular section, or to oscillate about such a form. It modifies the form of the surface of every mass of liquid, by rounding more or less the corners which would otherwise be angular.—Cohesion also exists to a greater or less degree between liquids and solids; and the combined effects of this force, and of the superficial tension due to the cohesion of the liquids themselves, constitute what are known as phenomena of *capillary attraction*.—It is by reason of this tendency of the external film of a liquid mass to assume a definite figure, viz., the sphere, that in defining the word "Liquid" at the beginning of this article, non-resistance to change of figure has been predicated of the internal parts of a liquid body only, and not of the whole mass. The superficial tension, however, is so feeble, that in all hydrostatical investigations relative to masses of liquid much exceeding the dimensions of a drop, it may be neglected.—Intermediate between the liquid and the solid conditions, are the *viscid*, *plastic*, and *gelatinous* conditions, in which the power of resisting change of figure is possessed imperfectly by the internal parts of the mass. ELASTICITY, § 8. The sliding of particles of liquid over each other, or

over a solid surface, is resisted by a force known by the name of *friction*, although its laws are different from those of the friction of solid bodies. Liquid friction, or resistance, is independent of the pressure, and is nearly proportional to the square of the velocity of sliding, and to the area of the surface over which sliding takes place. The mechanical energy which disappears in overcoming this kind of resistance, is replaced by an equivalent quantity of heat. See HEAT, MECHANICAL ACTION OF, §§ 1, 2. The laws of liquid resistance form the foundation of that branch of Hydro-dynamics which relates to the flow of liquids from orifices and through channels.—It is known of most liquids, and believed of all, that at certain temperatures, called their respective *freezing points*, they assume the solid condition, which they retain at all lower temperatures. Some substances undergo an increase of volume in the act of freezing, and all produce certain quantities of heat, which have to be abstracted, or the freezing will not proceed. HEAT, MECHANICAL ACTION OF, § 32. Equal quantities of heat are made to disappear by the act of melting.– The volume of a given liquid mass depends on its temperature: rise of temperature being in most cases accompanied by *expansion*, and the rate of expansion, with rise of temperature, being greater at higher temperatures, and less at lower. In the case of water, the rate of expansion, gradually diminishing as the temperature becomes lower, disappears altogether at 39°·2 Fahr., at which temperature water attains its greatest density. From this temperature down to its freezing point, water undergoes expansion with *fall of temperature*. Whether a similar phenomenon takes place, for any other substance is not known.—The *specific heat* of liquids increases slowly with rise of temperature, and appears to be connected with the rate of expansion, though by what precise law is unknown. For most substances, the specific heat is different in the solid, liquid, and gaseous conditions. Rise of temperature increases the resistance of liquids to compression (ELASTICITY, § 5), and diminishes their cohesion. It is known of most liquids, and believed of all, that for each temperature of a given substance, there is a certain minimum pressure on its external surface, which is necessary to its existence in the liquid state, and under which the communication of additional heat to the liquid mass, makes it boil, or emit bubbles of vapour from its interior. There is also reason to believe, that all liquids under all circumstances emit vapour from their surfaces, and are surrounded by an atmosphere of their own vapour. See HEAT and VAPOUR. Liquids possess, in some instances, a greater or less capacity for *absorbing* particular gases. This absorbing power is increased by pressure, and diminished by rise of temperature.—Liquids have also, in some instances, the power of *dissolving* particular solids, or other liquids, to a greater or less extent. The *diffusion* of dissolved solids or liquids through the dissolving liquid, proceeds according to definite laws.—Liquids are, in general, slow *conductors of heat*. The transfer and diffusion of heat in them takes place chiefly by *convection;* that is, by the circulation of currents.—Liquids are also, in general, slow *conductors of electricity*, though not so much so as certain solid bodies. When a liquid is capable of being electrically decomposed, an electric current may be transmitted through it by *electrolysis*.—No liquid possesses the property of *double refraction;* but some liquids, such as turpentine, and solutions of sugar, *rotate the plane of polarization* of a ray of light. Several liquids (such as the solutions of sulphate of quinine, and of the colouring matter of leaves), possess *fluorescence*, or the property of absorbing light of a higher degree of refrangibility, and emitting in its stead light of a lower degree of refrangibility. —Liquids are capable of assuming a feeble *magnetic* or *diamagnetic* condition by induction.—The liquid condition is the most favourable of all to *chemical action.*

Loci Plani; Locus. The locus of a point, is the line generated by that point when it moves according to some determinate law: in the same manner the locus of a line moving also according to determinate law, will be some determinate surface.—The investigation of *loci*, was a favourite pursuit of the ancient geometers,—one of the most interesting works of the great Apollonius, being his treatise *De Locis Planis*, or an inquiry into such loci, as can be referred to the straight line or the circle. The treatise itself is lost; but as Pappus has described it pretty fully, it became an ambition with our best modern geometers to restore it. Fermat made a successful attempt, which he communicated to his friends in 1637. He was followed by Schooten in 1659; but no one entered into the true spirit of the methods of Apollonius, previous to Robert Simson, who, in 1746, gave to the world his *Apollonii loca plana restituta*. The preface of this remarkable work especially merits attention, because of the nice appreciation it contains of the old geometrical analysis.—The nature of the problems occupying this work of Apollonius, will be understood from the following enunciations of three of them.—(1.) Suppose that from any point P several pairs of lines, PA, Pa; PB, Pb; PC, Pc; &c., are drawn making constant angles APa, BPb, CPc, &c., and that the ratio of PA to Pa is the same as that of PB to Pb, &c.; or suppose that the rectangle PA × Pa, is equal to the rectangle PB × Pb, &c.,—then, if the points A, B, C, &c., have a *plane locus*—that is, if they lie either along a straight line or a circular arc—the points a, b, c, will also have a plane locus, in the former case they will lie along a straight line, in the latter they may be connected by a circular arc.—(2.) If from the extremities of a straight line two lines AP, BP, be drawn, having to each other a ratio of inequality,—this point P, and all others

, ϖ, &c., n which lines A p, B p, A ϖ, B ϖ, hav- ig the same ratio, can terminate, will have as neir locus the circumference of a circle.—(3.) If om any number of points, A, B, C, D, E, &c., there e drawn lines to any number of other points, , p, ϖ, &c., so that the sums of the squares of ae lines A P, B P, C P, &c., be equal to the sums f the squares of the lines Ap, Bp, Cp, &c., and) on with regard to ϖ, &c., then the points P, , ϖ, &c., must all be in the circumference of a ircle whose centre and diameter may be deter- iined.—This latter proposition enunciates a very urious property of the circle, and touches closely n the subject of the Centre of Gravity.—The udent is further referred, as to problems con- erning Loci, to Sir John Leslie's *Geometrical nalysis.*

Locomotive Engine. See STEAM ENGINE.

Logarithm. A name given to those auxi- iary numbers—the first conception of which we we to Lord Napier, or Neper, of Merchistoun— y whose aid difficult arithmetical operations are greatly abridged. It has now, however, a ider meaning, and belongs also to Symbolical lgebra.—The nature and Theory of Logarithms, well as the mode of computing them are easily rrived from the exponential Equation

$$\varepsilon^x = y,$$

here x is the logarithm of the number or quan- ty y, and ε any number or quantity whatever, rmed the *base* of the peculiar logarithmic sys- m. Take two numbers and their corresponding garithms,—

$$\varepsilon^x = y$$
$$\varepsilon^{x'} = y'$$

ultiplying these two equations, and dividing e one by the other, we have

$$\varepsilon^{x + x'} = y\,y'$$
$$\varepsilon^{x - x'} = \frac{y}{y'},$$

other words, *the sum of the logarithms of two antities is the logarithm of the product of these antities, and the difference of their logarithms the logarithm of their quotient.* Again, ex- acting the n^{th} root of both sides of the fore- ing exponential equation, and raising both sides the n^{th} power, we obtain

$$\varepsilon^{\frac{x}{n}} = \sqrt[n]{y}$$

$$\text{and } \varepsilon^{nx} = y^n:$$

the logarithm of a quantity divided by n is logarithm of the n^{th} root of the quantity; d the same logarithm multiplied by n, is the arithm of the n^{th} power of the quantity.—It ll be seen at a glance how extensively the in- ition of these numbers and the use of the tables y constructed, must go to simplify and abbre- te all numerical processes.—There are two or ee minor arithmetical propositions worthy of being noticed. If the quantity y be $= 1$, its logarithm is o, whatever the base e, because,

$$\varepsilon^o = 1.$$

Again, if $y = o$, the logarithm must be $-\infty$, because

$$\varepsilon^{-\infty} = \frac{1}{\varepsilon^{\infty}} = o.$$

The logarithms of numbers between 1 and 0, there- fore, must be negative, whatever the base ε: those greater than 1, must be positive; while logarithms of negative numbers do not exist, or are imaginary. We are supposing that the base ε is greater than 1 or positive. It may be any number whatsoever. In the Neperian system it is 2·718281, for a reason that will afterwards appear: in the tables commonly in use, and which are much more convenient, it is 10: this change was introduced by Briggs.—Let us now present the *series* that represent the relations of loga- rithmic quantities. The following exponential development is well known:—

$$a^x = 1 + \mathrm{A}x + \frac{\mathrm{A}^2 x^2}{1 \cdot 2} + \frac{\mathrm{A}^3 x^3}{1 \cdot 2 \cdot 3} + \text{\&c.}$$

let $x = \frac{1}{\mathrm{A}}$ and we have

$$a^{\frac{1}{\mathrm{A}}} = 1 + 1 + \frac{1}{1 \cdot 2} + \frac{1}{1 \cdot 2\,3} + \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} + \text{\&c.}$$

if A be 1, we have the quantity ε, or the base of the Neperian system; that is

$$\varepsilon = 2 + \frac{1}{1 \cdot 2} + \frac{1}{1 \cdot 2\,3} + \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} + \text{\&c.}$$

which series when summed is approximately 2·718281: hence

$$e^x = 1 + x + \frac{x^2}{1 \cdot 2} + \frac{x^3}{1 \cdot 2 \cdot 3} + \text{\&c.}$$

Since $a^{\frac{1}{\mathrm{A}}} = \varepsilon$, it is clear that

$$\varepsilon^{\mathrm{A}} = a;$$

that is, A is the Neperian logarithm of any other base a.—

Reversing the foregoing series, we get in the system of Neper

$$\log\,(1 + y) = y - \frac{y^2}{2} + \frac{y^3}{3} - \frac{y^4}{4} + \frac{y^5}{5}\,\text{\&c.}$$

and

$$\log\,(1 - y) = -y - \frac{y^2}{2} - \frac{y^3}{3} - \frac{y^4}{4} - \frac{y^5}{5}$$

by combining which, we obtain the singularly converging series,

$$\log(y+1) = \log y + 2\left(\frac{1}{2y+1} - \frac{1}{3(2y+1)^3} + \frac{1}{5(2y+1)^5} +, \&c.\right)$$

by aid of which, tables of numbers can easily be computed. It is plain that it is only necessary to compute the logarithms of *prime* numbers: those of other numbers can be determined from one another.—We have said, however, that the term logarithm has now a much wider significance than the mere arithmetical one; it is a *symbolical* term. The complete solution of the exponential equation is not confined within the foregoing strait limits. If

$$\varepsilon^x = y$$

x may have an infinity of values, whatever y may be: that is, y has an infinity of logarithms. It is known that

$$\varepsilon^{\theta\sqrt{-1}} \quad \cos\theta + \sin\theta.\ \sqrt{-1}$$

make $\theta = 2\,m\,\pi,$

then $\varepsilon^{2\,m\,\pi\sqrt{-1}} = 1,$

multiply by $\varepsilon^{x} = y$

and $\varepsilon^{x + 2\,m\,\pi\sqrt{-1}} = y.$

In other words, $x + 2\,m\,\pi\sqrt{-1}$ is the complete or symbolical logarithm of y. Denoting this complete expression by L, and the arithmetical Neperian logarithm by l, the foregoing expression may be written

$$L\,y = l\,y + 2\,m\,\pi\sqrt{-1},$$

or again $L\,z = l\,z + 2\,n\,\pi\sqrt{-1}.$

Multiply and divide, and we obtain

$$L\,y\,z = l\,y\,z + 2\,(m+n)\,\pi\sqrt{-1}$$

and $L\dfrac{y}{z} = l\,\dfrac{y}{z} + 2\,(m-n)\,\pi\sqrt{-1}$

The logarithms of *negative* quantities have symbolical representatives, although the logarithms of negative numbers are imaginary. For instance, let $\theta = (2\,m+1)\,\pi$, m being a whole number, then, substituting as previously in the original equation,

$$\varepsilon^{(2\,m+1)\,\pi\sqrt{-1}} = \cos(2\,m+1)\,\pi + \sin(2\,m+1)\,\pi,\ \sqrt{-1} = -1$$

Whence $L - 1 = (2\,m+1)\,\pi\sqrt{-1}$

or generally

$$L - x = lx + (2\,m+1)\,\pi\sqrt{-1}$$

From the value of $L - 1$ a curious symbolical result is deducible, viz.:—

$$(2\,m+1)\,\pi = \frac{L-1}{\sqrt{-1}},$$

or if $m = o$

$$\pi = 3{\cdot}1416 = \frac{L-1}{\sqrt{-1}}.$$

For further and full information as to general or symbolical Logarithms, we refer the student to the writings of Professor de Morgan.

Logarithmic Curve: Logarithmic Spiral. The equation of the former is

$$y = \log x.$$

The polar equation of the spiral is

$$\log r = v$$

the pole being at the eye of the curve. An interest attaches to this latter from the demonstration by Newton, that if the force of gravity had varied as the inverse *cubes* of the distances, the planets would have receded from the sun, and that their paths would have been Logarithmic Spirals.

Longitude Astronomical. If a great circle be drawn through a star from the pole of the Ecliptic, the portion of the Ecliptic intercepted between the first point of Aries, and its section by that great circle, is the longitude of the star.

Longitude Terrestrial. The difference of the longitude of two places, is the angle formed by their meridians, or the portion of the Equator intercepted by those meridians. The absolute Longitude of any place is a similar arc of the Equator, between the meridian of the place, and some other meridian or great circle that has been arbitrarily selected as the *first* meridian. In all our English maps, and in those of several other countries, the first meridian is the one passing through the Observatory of Greenwich; in France they reckon from the Observatory of Paris; and in North America they now sometimes begin with the meridian of Washington.—It is easy to see that the determination of the difference of Longitude of two places, is equivalent to the determination of the difference of *time* at the two places. As the Earth rotates on its axis through fifteen degrees every hour, it is clear, that a place whose meridian is fifteen degrees west of that of another, will have, as its time, *eleven* o'clock A.M. when the Sun culminates at the latter, or when it is *twelve* o'clock there; while at a place as far to the east, the clock must point to *one* P.M. If, therefore, we can find these differences of *times*, the problem regarding Longitude is solved. Now, since it is easy to find the absolute time at any one place (see TIME),—say at St. Helena, the observer at St. Helena will have found his longitude when he ascertains the corresponding time at Greenwich. This is accomplished by astronomical observations depending on a simple principle. Changes of position among the celestial bodies, or other noted events, whose advent can be calculated beforehand, are incessantly occurring in the sky. Let us suppose the times of the occurrence of these events at Green-

wich, to be computed for several years to come, and that an accurate list of such occurrences and their Greenwich time, are published;—an act really done in our admirable National Ephemeris —the *Nautical Almanac*. An observer at St. Helena notices one of these events, and marks the St. Helena time of its occurrence: from the *Nautical Almanac* he learns the corresponding Greenwich time;—hence, his Longitude.—The application of this method would be easy, if the events in question took place at *the same absolute instant of time* at the two places. But this is seldom the case—never indeed, in regard to those events, which can be observed with greatest precision. The different position of the two observers impresses different effects on the apparent places of the important celestial bodies, through variations of refraction and parallax; and it sometimes happens, that before an event observed or predicted for one meridian, can be observed at another, the bodies concurring to produce that event, have moved in the sky. To get rid of the influence of such changes, all accurate methods of determining Longitude by the foregoing principle, are encumbered accordingly with long corrections or calculations. We shall offer one specimen of them under OCCULTATION, in our exposition of BESSEL's method; but our limits prevent our going farther. It is with no ordinary earnestness that we refer the reader desirous of studying the whole subject thoroughly, to the recent volume on *Practical Astronomy* by Professor Loomis of New York—by far the best work of the kind at present existing in the English language. The astronomical events usually employed in the determination of longitude, are these—(1.) Eclipses of Jupiter's Satellites.—(2.) Eclipses of the Moon.—Neither of these can afford satisfactory accuracy.—(3.) Eclipses of the Sun.—(4.) The method of Lunar distances —commonly used at sea.—(5.) Occultations, *q.v.*—(6.) Transits of the Moon, or better still, the method of Moon culminating stars.—There are, however, two direct modes of comparing *times* independently of celestial observation, that have now risen into so high importance in Geodesy, that it is necessary to treat them at some length. We are again indebted to Professor Loomis. They are as follows:—

(1.) *Longitude Determined by Transportation of Chronometers.*—The manufacture of chronometers has recently attained to such a degree of perfection as to afford the means of determining the difference of longitude of two stations, not too remote from each other, with a precision superior to that of most other methods. The following are the essential steps of this method:—The time is accurately determined at one station, Greenwich, for instance, and the chronometer is carefully compared with the transit clock; hence the error of the chronometer on the meridian of Greenwich is known. The chronometer being carried to a second station, for example, the Observatory of Paris, is compared with the transit clock at that place. Thus the error of the chronometer on the meridian of Paris is known; but its error on the meridian of Greenwich at the same instant is known, if its *rate* be known, and the longitude is the difference of these two errors. In grand chronometric expeditions, it is customary to employ a large number of chronometers, from twenty to fifty, or more, as checks upon each other.—The most serious difficulty in the application of this method consists in determining the rate of the chronometers during the journey, for chronometers generally have a different rate when transported from place to place, either by land or by sea, from that which they maintain in an observatory. When it is proposed to determine the difference of longitude of two stations with the greatest accuracy, the error of the chronometers should be determined at the commencement of the expedition, at the first station; the same thing should be done at the second station; then, as soon as possible, the chronometers should be brought back to the first station, and their error determined anew. The chronometers should thus be transported back and forward a considerable number of times.—Let us designate the eastern station by A, the western by B, and the west longitude of the place B from A we will designate by ω. We will suppose that at the time t, at the place A, the error of one of the chronometers was a; that on its arrival at B, at the time t', the error was b; and again, on its return to A at the time t'', the error was a'. If we regard a day as the unit of time, and represent the mean daily rate of the chronometer during the journey by m, we shall have

$$m = \frac{a' - a}{t'' - t};$$

whence we may conclude that

$$\omega = a + m\,(t' - t) - b,$$

or

$$\omega = a' - m\,(t'' - t')\,b.$$

Each chronometer will afford an independent determination of the value of ω; and in order to detect any irregularity in the rates of the chronometers, they should be compared daily with each other throughout the entire journey.—Since chronometers almost invariably indicate a different rate, according as they are travelling or at rest, if the observer remains for several days at the station B, the error of the chronometers should be determined immediately upon arrival, and again before departing from B; and the interval of rest should not be included in the determination of the value of m. Suppose we have determined the chronometer errors

$$a,\ b,\ b',\ a',$$

corresponding to the times

$$t,\ t',\ t'',\ t''',$$

where a and a' are supposed to have been obtained at the place A; b was the error on first arriving at B, and b' the error on departing from B.—Then the interval of time embraced in the two journeys is

$$(t' - t) + (t''' - t''),$$

and the change in the error of the chronometer for the same time is

$$(a' - a) - (b' - b).$$

Hence we have

$$m = \frac{(a' - a) - (b' - b)}{(t' - t) + (t''' - t'')}.$$

It is indispensable to the accuracy of the results thus obtained, that the time be obtained at each station with the greatest precision. Struve recommends that the time be determined with a transit instrument, by observations of stars near the zenith, inasmuch as a slight deviation of the transit instrument from the plane of the meridian does not affect the time of passage of a zenith star. It is necessary, however, to know the inclination of the axis with the greatest accuracy; and the axis should be reversed upon its supports during each series of observations, so as to eliminate the effect of unequal pivots and of collimation error. In order to eliminate the effect of any error in the right ascensions of the stars employed, the same stars should, if possible, be observed at both stations. For this purpose, a catalogue of all the stars which pass near the zenith, and of a magnitude sufficient to be observed without inconvenience, should be prepared beforehand, and a copy furnished to each observer. If the places of any of the stars are too imperfectly known, they should be carefully observed with the instruments of some large observatory.—The comparisons of the chronometers should all be made by observing the *coincidence* of beats. If we undertake to compare two clocks which beat seconds of the same kind of time, unless they happen to tick at the same instant, there is a fraction of a second which must be estimated by the ear. This estimation is extremely difficult, and practised observers will differ among themselves by a quarter of a second, and sometimes even more. When, however, the two clocks happen to tick together, there is no fraction of a second to be estimated; and a practised ear will detect any deviation from coincidence in beats amounting to 0·01s. Now a sidereal clock gains upon a solar clock one second in about six minutes; and if two such clocks are placed side by side, they must tick together once in every six minutes. In order to compare two such clocks, we notice their movements, and wait until the beats sensibly coincide, when we know that their difference amounts to an entire number of seconds, which is readily discovered. Chronometers generally make two beats in a second; so that between a clock which beats seconds of sidereal time, and a chronometer which ticks half-seconds of solar time, there must be a coincidence every three minutes. Chronometers are sometimes made to tick 13 times in 6 seconds. Such a chronometer, regulated to mean time, makes 121 ticks in 56 seconds of sidereal time; that is, the coincidences between such a chronometer and a sidereal seconds-pendulum would occur every 56 seconds. Moreover, the intervals between the ticks of the chronometer is 0·4628s. sidereal time; and 13 of these intervals are equal to 6·016s. sidereal time; 54 are equal to 24·991s.; 67 are equal to 31·007s.; and 121 are equal to 55·999s.; that is, in the course of 56 seconds there are five coincidences within the limits $\pm$ 0·02s. Such a chronometer affords the means of comparing by coincidences with great rapidity; a consideration of no trifling importance where 80 chronometers are to be compared daily. Chronometers are frequently made to beat five times in two seconds, which gives a coincidence at every 36 seconds with a half-second sidereal chronometer.—It is also indispensable to the accuracy of the results that the personal equation of all the observers employed in obtaining the time should be carefully determined. See PERSONAL EQUATION. This correction is the most difficult to obtain satisfactorily, especially as the Personal Equation is not always a constant quantity, but is liable to vary with the physical condition of the observer. It is the opinion of Mr. Airy, that when a tolerable number of chronometers is used for a moderate distance, and in good observing weather, the *variation* of the personal equation is the error to be most apprehended.

(2.) *Longitude Determined by the Electric Telegraph.*—The difference of the local times of two places may be determined by means of any signal which can be seen or heard at both places at the same instant. When the places are not very distant, the explosion of a rocket or the flash of gunpowder may serve this purpose. Six or eight ounces of powder at night makes a good signal at a distance of twenty-five to thirty miles; but for a distance of ten miles, two or three ounces are sufficient, if the observers are provided with telescopes.—But the electric telegraph affords the means of transmitting signals to a distance of a thousand miles or more with scarcely any appreciable loss of time.—Suppose there are two observatories at a considerable distance from each other, and that each is provided with a good clock and a transit instrument for determining its error; then, if they are connected by a telegraph wire, they have the means of transmitting signals at pleasure from either observatory to the other, for the purpose of comparing their local *times*. The signal is given at either station by pressing a key, as in the usual mode of telegraphing; and the observer at the other station hears the click caused by the motion of the armature of his electro-magnet. Four different methods of comparison have been practised:—The *first* method

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is the most obvious one, and consists in simply striking on the signal key at intervals of ten seconds; the party at one station recording the time when the signals were given, and the other party recording the time when the signals were received. After about twenty signals have been transmitted from the first station to the second, a similar set of signals is returned from the second station to the first. This mode of comparison has but one serious imperfection, and this is, that it requires the fraction of a second to be estimated by the ear. The party giving the signals strikes his key in coincidence with the beats of his clock, so that at this station there is no fraction of a second to be estimated; but at the other station the armature click will not probably be heard in coincidence with the beats of the clock, and the fraction of a second is to be estimated by the ear. Now this fraction cannot be estimated with the accuracy which is demanded in this kind of comparison. It is found that observers generally estimate the fraction of a second too small when using the ear alone, unassisted by the eye. This error is greatest at the middle date between two clock beats, and is found to vary from 0·06 to 0·18 of a second with different observers.—This evil suggested the *second* method of observation, which relies on the coincidences of a mean solar and sidereal clock or chronometer. The following is the method pursued:—After transmitting a few signals by the former method, so as to determine the difference between the local times of the two stations within a small fraction of a second, the party at the first station commences striking on his signal key every econd, in coincidence with the beats of his mean olar chronometer, and continues to do so for ten r fifteen minutes without interruption. The party at the second station compares the armature click of his magnet with the beats of his idereal clock, and watches for a coincidence, and ecords the time when the coincidence takes lace. When he has obtained two or three coicidences, which generally requires from ten to fteen minutes, he breaks the electric circuit, in der to notify the first party to stop beating. e then commences beating seconds by striking s own signal key in coincidence with the beats his sidereal clock; and the party at the first ation compares the armature clicks of his magt with the beats of his solar chronometer, and atches for a coincidence. When he has obined three or four coincidences, which generally quires ten or twelve minutes, he breaks the ctric circuit, in order to notify the other party stop beating.—The comparison of times at the o stations is now complete.—A *third* method comparing local times is by telegraphing nsits of stars. This method has been praced lately, in the following manner:—A list of ith stars is selected beforehand, and furnished each observer. When everything is prepared observation, the one astronomer points his telescope upon one of the selected stars as it is passing his meridian, and strikes the key of his register at the instant the star appears to coincide with the first wire of his transit. He makes a record of the time by his own chronometer, and the other astronomer, hearing the click of his magnet, records the time by his own clock. As the star passes over the second wire of the transit instrument, the first astronomer again strikes the key of his register, and the time is recorded of both observations. The same operation is repeated for each of the other wires. The first astronomer now points his telescope upon the next star of the list, which culminates after an interval of five or six minutes, and telegraphs its transit in the same manner. In about twelve minutes from the former observation, the first star passes the meridian of the second astronomer, when there, points his transit instrument upon the same star, and strikes the key of his register at the instant the star passes each wire of his transit. The times are recorded at both stations. The second star is telegraphed in a similar manner. The same operations are now repeated upon a second pair of stars, and so on as long as may be thought desirable.—The chief objection to this method is, that it involves the estimation of fractions of a second, as in the usual mode of transit observations; that is, it involves the personal equation of the observers.—The *fourth* method of comparison obviates this evil in some degree, by printing the signals upon a cylinder or a fillet of paper. There must be a clock at one of the stations for breaking the electric circuit every second, as described in TRANSIT INSTRUMENT; and there must be a register at each of the stations for recording the beats of the clock and any other signals which may be required. When the connections are properly made, there will be heard a click of the magnets at each station simultaneously with the beats of the electric clock, and the registers will all be graduated into second spaces. The method is not limited to two stations, but any number of stations may be compared at the same time. The mode of observation is the same as described in the preceding article, except that the observations are all recorded by the operation of machinery. If one astronomer strikes the key of his register as the star passes successively each wire of his transit instrument, the dates are printed not only upon his own register, but also upon those at any number of other stations. When the same star comes over the meridian of another station, the observer there goes through the same operation, and his observations are printed upon all the registers. Thus we have any number of registers all graduated into equal parts by the ticking of the same clock; and, upon these, we have printed the instants at which the star was seen to pass each wire of the transit telescopes at the several stations. These observations furnish the difference of longitude of

the stations, independently of the tabular place of the star employed, and also independently of the absolute error of the clock. The observers next read their levels, and reverse their transit instruments. A second star is now telegraphed successively over each meridian, and so on as long as may be desired.—This method of observation is so accurate as to furnish a tolerable measurement of the velocity of the electric fluid. If the fluid requires no time for its transmission, then the signals given at either station ought to be similarly printed at all the stations; and the fraction of a second registered upon any one scale should be identically the same as upon every other. But if the fluid requires time for its transmission, these fractions will be different.

Lunar Cycle. See CYCLE, where the Metonic Cycle is described.

Lunar Distances. The designation of a favourite method of determining the longitude at sea. Its principle is this:—In consequence of the rapid motion of the moon in her orbit, she is rapidly varying her apparent distance from any fixed star. But as the motions of our satellite are perfectly known, it can be calculated beforehand how far she is distant from any set of stars at a given moment. Accordingly, in the *Nautical Almanac*, a large table is supplied, in which lunar distances are given—*in Greenwich time*—for short intervals, through the entire year. Supposing, then, a navigator in possession of his time at sea, should observe the moon's accurate distance from a noted star at that time, on referring to the *Nautical Almanac* he finds the Greenwich time corresponding to that distance; and the differences of these times is his longitude. The art of observing lunar distances is easy of acquirement, but it needs practice and care, as well as a thorough knowledge of the special instruments used (see SEXTANT), and a right command of them. After the observations are made, calculations or reductions are necessary to "*clear* the distance." The cause is, that the distance observed is not the *true*, but the *apparent* distance only,—the altitudes of the bodies being both affected by refraction,—and in the case of the moon always, and if the other object is a planet, in the case of both, also by parallax. The operation is a little troublesome, but it offers no difficulty in principle,—consisting simply of the resolution of two spherical triangles.—Details in any practical work on *Navigation*.

Lunar Observation. An observation of the moon's distance from a star, for the purpose of finding the longitude of a place by it. See LONGITUDE. This distance is called the *Lunar Distance*.

Lunar Theory. It is our intention to offer, in this paper, a general account of the principal Inequalities affecting the motions of our Satellite, and of their causes. The method adopted is that of Mr. Airy in his treatise on Gravitation,—a method admirable for its lucidity and simplicity, and which is quite adequate to explain the physical conditions of this complex problem. In every inquiry touching on the subject of planetary perturbations, there are two portions altogether distinct,—one is occupied with the discovery of the nature of a perturbation and of its physical cause, while the other is devoted to the discovery of the exact amount of that perturbation. This latter inquiry can be conducted through the instrumentality of Analysis alone; but geometrical methods are quite adequate to the former. The thorough Astronomical student will refer to such treatises as that of Pontécoulant: we invite the attention, in this place, merely of those who desire a general but distinct conception of the nature of the Celestial Mechanism.—A few elementary considerations must be premised. The following are the effective elements of the system whose relations we are about to examine—that system, viz., of the Three Bodies, the Earth, Moon, and Sun. For the present, we place out of sight the action and even existence of the other Planets:—

	Earth.	Moon.	Sun.
Relative distances......	0·	·0025 —	1· —
Relative masses........	1·	·0114 —	354936

It is to be noticed further, that the orbit of the moon is not in the plane of the Ecliptic, but inclined to it at an angle of $5° 8' 47''·9$. The three bodies, in short, lie in relations to each other, indicated in the subjoined diagram:—

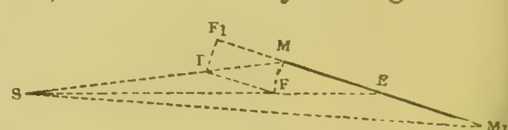

Fig. 1.

It is well known that if the Moon were affected only by the attraction of the Earth, our Satellite would describe an ellipse, and be wholly subservient to Kepler's Laws; but since comparison of the foregoing elements shows that the attraction of the Sun, alike over Moon and Earth, must be very great, a new element of vital importance is manifestly introduced. It is not, however, the total force with which the Sun acts upon the Moon, that is the *disturbing* force. If our Luminary acted with *equal* energy on the Earth and Moon, the mutual relation of these two bodies would not thereby be changed. But it acts *unequally*, on account of the varying distance of the Moon: when our Satellite is at M, for instance, the Sun attracts it more than he attracts the Earth; and when at M_1 the reverse is the case. Now it is this inequality of action, or *the difference of the attractive effect of the Sun, on the Moon and Earth*—an inequality varying with the Moon's place—that alone can act as a disturbing cause, and which alone, therefore, we have at present to scrutinize. Still further, the absolute force of the Sun over the Moon, represented in magnitude and direction by M F, may at all times be decomposed into two forces, M F and M F_1,—the first,

at right angles to the plane of the Moon's orbit; and the second, in the direction of that plane. This resolution is of considerable importance, in the interest of simplicity of exposition; as it enables us, when treating of the effects of the most influential of these components, to place out of sight all consideration of the Inclination of the Lunar Orbit. In what follows of this article we shall take no further notice of the perpendicular force, or the force depending on that Inclination. Suffice it briefly to state, that it is this force which causes the Motion of the Moon's Nodes, one revolution of which is performed in 6798·279 days; and that similar actions give rise to the motions of the nodes of all the planetary orbits. See NODES. It were wrong to omit reference to a beautiful method, contrived by a most ingenious geometer, Mr. Elliot of Edinburgh, by which this description of perturbation can be exhibited as the result of experiment. If a metallic ring be put in rapid rotation, in a plane somewhat oblique to a horizontal circle within which it rotates, the application of a perpendicular force, through instrumentality of a magnet, will at once bring out the phenomenon of the motion of the nodes.—The ground thus, in so far cleared, the question with which we have to do is this:—

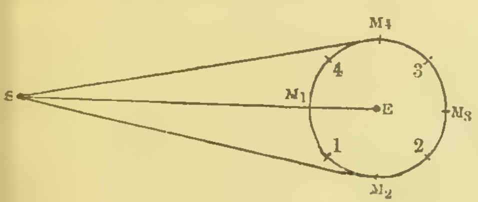

Fig. 2.

The Moon revolves round the Earth in an orbit M_1 M_2 M_3 M_4; in what way must the Sun—supposing that luminary to be always in the same plane with the Earth and Moon—disturb that orbit? We shall treat the problem under two heads—taking account in the first place of those lunar perturbations which are independent of the elliptical character or eccentricity of the moon's orbit, and next of those which involve that eccentricity. And we shall then refer to a few chief perturbations of the Moon, arising from other causes.

I. INEQUALITIES THAT DO NOT DEPEND ON THE MOON'S ECCENTRICITY.—These inequalities are three;—the *annual equation*, the *variation*, and the *parallactic inequality*.

(1.) *The Moon's Annual Equation.*—Reverting to fig. 2, and to the vital consideration that the Sun's disturbing effect is the difference of his effects on the Earth and Moon, let us first examine the most palpable and general result of that disturbing influence. It cannot fail to be evident that when the Moon is at M_1 or M_3 that influence must be at its maximum, simply because the difference of the distances of the two bodies from the Sun is there the greatest possible. Now, the nature of that influence is entirely the same in both positions of the Moon,—in the first, the Moon is, so to speak, drawn away from the Earth; and, in the second, the Earth is drawn away from the Moon: in other words, the natural connection between the two bodies is enfeebled, or the *Earth's attraction over the Moon is virtually diminished.* At the intermediate positions, M_2 and M_4 on the other hand, the case is quite altered. Had the *direction* of the Sun's force on M_2 and M_4 been parallel to its direction on E, that luminary could have impressed no disturbance on the Moon when she is in quadrature, on account of the equality of the distances of the Earth and its satellite in those positions, from the Sun. But the *directions* are different: M_2 S is an oblique force or strain; and it clearly has a tendency, because of that obliquity, to bring the Moon down to the line E S, or to cause it *approach* the Earth. When the Moon is in *quadrature* therefore, the Sun exerts a disturbing force which virtually *increases* the power of the Earth's attraction over that body,—the reverse of what we have found to hold when the Moon is at M_1 and M_3, or in *syzygy.* But as the effort of the Sun to draw the Moon towards the Earth, in one part of its orbit, is exercised only through the obliquity of its pull or strain, while the contrary effect is the result of a disturbing force much more powerful, that, viz., due to his unequal distance from the two bodies, it is evident that *upon the whole*, this disturbing force *diminishes the Earth's attractive influence over the Moon.* Now, the central force which retains any body in an orbit, and the periodic time in which that body revolves, are in closest relationship: a permanent alteration of the one, will issue in a permanent alteration of the other. If, therefore, the action of the Sun as first described had been permanent and regular, the Moon would have moved with an angular velocity differing from what she would have had otherwise, but that angular velocity would have been permanent and have caused no inequality in the Moon's motions. But the Sun's disturbing action *is not permanent.* Its energy or effect necessarily varies with the distance of the system of the Earth and Moon, from their common luminary; and owing to the elliptical character or eccentricity of the Earth's orbit, that distance has an annual variation. The magnitude of the Sun's disturbing force is, in fact, proportioned to the inverse of the cube of his distance from the Earth; so that it is considerably greater when the Earth is in perihelion than when it is in aphelion. Hence an irregularity or inequality of the Moon's angular velocity—an inequality that runs through its changes in the course of a year. The effect is, that our satellite is sometimes behind her mean place in the heavens, and sometimes in advance of it; the greatest deviation amounting to 10′. This inequality was first discovered by Tycho Brahe from observation; and it is called the Moon's *Annual Equation.*

(2.) *The Moon's Variation.*—In the foregoing section we have spoken of the disturbing force, in very general terms only. It will be necessary now, however, to scrutinize it more minutely. We have found that at syzygies the force is wholly in the direction of the radius vector and *negative*, in other words it diminishes the earth's attraction over the moon. At these two points the disturbing energy arises wholly from the inequality of the distances of the Earth and Moon from the Sun; and as this inequality necessarily exists for every point of the moon's orbit, excepting the two points of quadrature, there must—(merely putting out of view these *two points*)—be an energy of the same negative kind, and arising from the same cause, operating on our satellite—whatever be her position in her orbit. Again—at quadrature, as we have seen, a disturbing force, operates also in the direction of the radius vector, but *positive;* that is, drawing the moon closer to the earth, or virtually augmenting our planet's attraction. Now, as this springs out of the obliquity of the pull effected by the Sun, a *positive* force of this sort will be energetic, wherever the Moon is in her orbit, if we merely except the two positions of syzygy. The action actually exercised along the radius vector will, of course, be represented by the difference of these two forces; and a little consideration will show that the following must be the case. At points M_1 and M_3, the negative force is pure and simple; and at points M_2 and M_4, the positive force is pure and simple. Between M_1 and M_2, and between M_2 and M_3—in short, about the middle of every quadrant, there will be a point 1, 2, 3, or 4, where these positive and negative forces exactly balance, or where no disturbing force will be exercised in the direction of the radius vector at all. During her motion through arcs 4, 1, and 2, 3, the moon's gravity to the earth is diminished; during her motion through arcs 1, 2, and 3, 4, that gravity is increased: at points 1, 2, 3, and 4, it is not disturbed, but remains what it would have been had the action of the sun not existed. This, then, is a complete view of the forces acting, positively or negatively, in the direction of the radius vector. —But the force just scrutinized is only a part or single component of the disturbing energy of the Sun. There is another component of it wholly different, to which we must now direct attention. The oblique pull is not wholly in the direction of the radius vector, but has a second and equally important element. For the sake of distinctness, let us further diminish the apparent distance of the sun, or further exaggerate the diagram. M_1, M_2, M_3, M_4, is the moon's orbit as formerly, and S the position of the sun. Take in that orbit two points, so that S *a* E be an obtuse angle, and S *b* E an acute angle at *a* and *b*; and let us fully analyze the oblique pull S *a* and S *b*. Resolving the pull *a e* into two rectangular forces, we obtain the negative force *a d* in the direction of the radius vector, and another force *a e* at right angles to that radius, or a tangential force pulling the Moon when at *a* in the direction *a e*. At the point *b*, the Moon is also influenced by two forces,

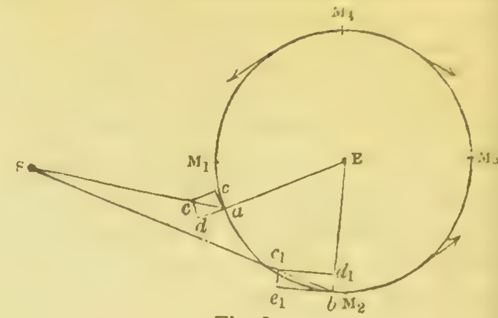

Fig. 3.

one *b d*, in the direction of the radius vector or positive, and another *b e*, also tangential, and pulling the moon in the direction of the tangent. At some point intermediate between *a* and *b*,—the point, viz., where the angle answering to S *a* E or S *b* E is neither obtuse nor acute, but a right angle, there will be no force but this tangential force, which then attains its maximum. And if these resolutions be effected in every quadrant, the following will be seen to be true;—where the forces in the direction of the radius vector have their maximum—whether these be positive or negative, viz., at syzygies and quadratures—the tangential force is at zero; and at the intermediate points 1, 2, 3, and 4, where the forces in the direction of that radius have evanesced, the tangential forces attain their maximum. It will readily be seen, too, on graphically effecting these resolutions, that, like the forces augmenting or accelerating gravity, these tangential forces change their character or sign. When the moon passes from M_1 to M_2 the tangential force is contrary to its motion, and therefore retards that motion; between M_2 and M_3 it is an accelerating force, between M_3 and M_4 a retarding one, and again an accelerating force between M_4 and M_1: or, more briefly, *as the Moon passes from syzygy to quadrature she is retarded by the tangential force; but when she passes from quadrature to syzygy she is accelerated by that force.* —A careful perusal of the foregoing exposition will enable the student to understand completely the action of the disturbing force of the sun, under the relations we have specified:—let us proceed to scrutinize the special influence of this *tangential force.*—Now, without entering at present on considerations we shall afterwards advert to, we think it may be concluded that,—as the tangential force retards the motion of the moon in its transit from M_1 to M_2, and also in its transit from M_3 to M_4, while it accelerates between M_2 and M_3, and also between M_4 and M_1,—*a disturbance or new inequality in the Moon's angular motion*, must hence arise. To reach the root of this inquiry, one requires to advert to the effects of this class of disturbances, *on the shape of the Lunar Orbit;* but this would only authorize us all the more (see ORBIT) to aver that when our satellite is at

M_1 or M_3—the extremities of its periods of acceleration, its actual angular velocity must be greater than its mean angular velocity, while for the opposite reason, it must differ from the mean angular velocity by defect, when at M_2 and M_4 This new Inequality is the *Moon's Variation.* It reaches the amount of 32′, by which the moon's true place is sometimes behind and sometimes before the mean place. The *Variation* was likewise discovered by Tycho Brahe at Uranburg.

(3.) *The Moon's Parallactic Inequality.*—In treating the two Inequalities just investigated, no distinction has been made between the disturbing forces as they act on the two sides of the moon's orbit, viz., on the side next the sun, and the side opposite the sun. But it will be clear on a simple inspection of the foregoing diagrams that these forces are not equal in intensity; on the side of the orbit nearest the sun their intensity is the greatest,—in other words, the moon is more disturbed when she is in conjunction than when she is in opposition. If, therefore, when computing the value of the Annual Equation and the Variation, we use mean values of the disturbing force, we must so that the representation of the real case be complete, introduce some new element which shall be equivalent to an effective increase of that mean force at conjunction, and its diminution at opposition. Now, this amounts to supposing that at and near conjunction, there is a new small force drawing the moon from the earth, and another acting tangentially, accelerating the satellite before conjunction, and retarding it afterwards; while at or near opposition corresponding forces of the opposite kind act likewise. The values of these forces known, their effects on the angular motion of the Moon can readily be ascertained; and their effects constitute the *Parallactic Inequality.* This inequality is certainly small in comparison with the Variation, with which it stands especially connected. Its period is that of a lunar synodial revolution, or one lunation; and it only impresses an effect of 2′ on the longitude of our Satellite. But it seems the more interesting, as we may infer from its amount the proportion of the Moon's distance to the Sun's distance from the Earth:—the magnitudes of the other perturbations depending solely on considerations of eccentricities and periodic times; neither of which involve the proportion of distances.

II. Inequalities involving the element of the Eccentricity of the Lunar Orbit.—These inequalities are also three, viz, the general *progression of the Moon's perigee;* the *irregularity of the motion of the perigee;* and the *alternate increase* and *decrease of the eccentricity of the orbit.* These two last inequalities, form, when combined, the *Evection.*

(1.) *The general Progression of the Perigee.*—That peculiar effect of the Lunar action we are now to consider, arises from the application of a disturbing force, positive or negative, in the direction of the radius vector, as well as of a tangential force, now accelerating now retarding—to a body moving in an ellipse, around another in the focus of that ellipse. It is inconsistent with our limits, to go over the whole course of investigation demanded by this form of the problem; we shall therefore only indicate its method, and state the results.—First, then, what would be the effect of a disturbing force in the direction of the focus, when the revolving body is in perigee, or perihelion—in other words, nearest the focus around which it revolves?

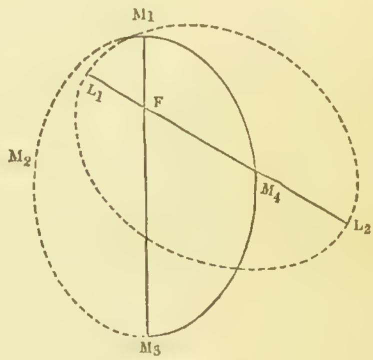

Fig. 4.

Suppose M_1, M_2, M_3, M_4, to be the elliptic orbit of the Moon, or any other planet, and as that body approaches M_1 its perigee, let a disturbing force directed *towards* F suddenly act on it. It is sufficiently plain that this will cause the path of the body to cut the line M_3 M_1 at *an acute angle*, and that the place where it would cut the line through F at a right angle, will be farther onwards, say at the point L_1: L_1 then, instead of M_1, is the *point of perigee*, so that the orbit has virtually twisted round and taken the position of the new dotted ellipse. At the next revolution, if the same force continues to act, the point of perigee will move still farther forward,—in other words, the constant agency of such a force will cause the *perigee*, or the *line of the apses to progress.* If the disturbing force be a *negative* one, or directed from the body in the focus, under the same circumstances, the *line of the apses will regress.*—Turning to the point of apogee, or the point M_3, it will be easy to show, in the same simple way, that the action of similar forces there must have precisely the opposite effect,—a negative force acting at M_3 will cause the *line of apsis to progress*, and *vice versa.* Now, we have seen (*a* and *b*, I.), that when the lunar orbit is in such a position with respect to the sun as below,

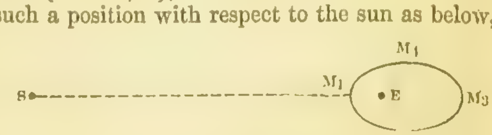

Fig. 5.

forces are acting at perigee and apogee exactly of the kind supposed above. Therefore, the line of apses is affected by opposite tendencies;—the

disturbance at M_1 causing it to *regress*, and the disturbance at M_3 causing it to *progress*. If those tendencies were equal in amount, the orbit would remain steadfast. They are, however, in the direct proportion of the distances, M_1 E, and M_3 E: the *progressive* tendency must therefore prevail, so that it may be averred that on the whole, under the foregoing circumstances, *the perigee, or the line of apses, necessarily progresses.* It must not be forgotten that there is also a *positive* disturbing force, a force drawing the Moon towards E at M_2 and M_4: how, then, does this force act? In the circumstances just treated, this force has no effect whatever on the line of apses. It acts effectively, indeed, in the direction of the line in each of the two positions, M_2 and M_4; but the effects are *opposite;* and, as in the present case, the forces are clearly equal, their result or balance will be *zero.*—It were easy to pursue the inquiry through all details, or in the two cases when the line of apogee passes through the Sun, and when the line of quadrature passes through the Sun. In such investigation, however, nothing new would arrest us;—suffice it then to state broadly, as the general result, that although the motion of the perigee must be irregular, it progresses upon the whole, in consequence of the action of the disturbing force in the direction of the radius vector.—Let us turn now to the effects of the *tangential disturbing force*, on this motion of the perigee. Without attempting to give the various steps of the inquiry—easy though they are—and referring the student for every satisfaction to Mr. Airy's remarkable treatise on Gravitation, we simply record as their results, that when the Moon is near perigee, the force in question causes the line of apses to progress, while when our Satellite is near apogee the reverse holds. But for the reason given in the former case, the latter action prevails;—hence, on the whole, the action of the disturbing tangential force, constrains *the perigee or line of apses to regress.* The practical question is, then, whether the general *regression* resulting from the tangential force, or the general *progression* resulting from the force in the line of the radius vector, predominates? The latter predominates. Independently of the calculation of absolute quantities, Mr. Airy has succeeded in showing very clearly that, from the nature of the forces in operation, the perigee must progress; and he has further traced the causes of the incongruity between the ascertained amount of that progression, and its theoretical amount as deduced in the famous Lunar Theory of Newton. This *general* or *average progression* of the Lunar Apses, has been known from the earliest times. In each revolution of the Moon, its perigee, alters its position forward along the Ecliptic, by about 3°; so that it describes the entire circle, or 360°, in about nine years.

(2.) Although the line of the apses has the foregoing regular average, or general rate of progression, it cannot escape notice that as this average is the mere balance of opposite individual tendencies, which are constantly varying in their relative amounts, nothing akin to actual regularity can be attributed to that motion of progression. The average of the year, in fact, is struck among the inequalities of the months: and it differs, *plus* and *minus*, from the phenomena belonging to the separate months, quite as much as the mean temperature of the year differs from the mean temperature of the various seasons. The position of the perigee therefore, must be an oscillating one: indeed, it sometimes progresses and sometimes regresses for months together. But as the angular motion of the Moon is swiftest in perigee, and depends, therefore, on the position of perigee, we have here the manifest source of another angular Inequality, depending on this *irregularity in this Motion of the Perigee.*

(3.) *The alternate Increase and Decrease of the Eccentricity of the Lunar Orbit.*—Variation in the position of the line of the apses does not alter the shape of the Earth's orbit. It must, as we have seen, alter her angular velocities when these are recorded in connection with the Longitudes of our Satellite; but the orbit, nevertheless, is only *translated*, or rather made to rotate around its focus,—the shape of that orbit remains the same. If, however, the two constituents of the solar disturbing force should be found to alter the shape of the Orbit likewise, we should evidently have a new inequality. According to the value of the eccentricity of the ellipse in which a body moves, is the variation in its angular velocities during a revolution,—in agreement with Kepler's *Law of Areas.* Now, the eccentricity is changed by both elements of the disturbing force. And these elements concur in evolving the following proposition:—*When the Sun is in the line of the Moon's apses, the eccentricity does not alter: after this, it diminishes, till the Sun is at right angles to the line of apses; then it does not alter: and after this it increases till the Sun reaches the line of apses on the other side.* The amount of this variation of the eccentricity exceeds one-fifth part of the mean eccentricity: the eccentricity being sometimes thus much increased and thus much diminished. For details again, see Airy. Now, as this irregularity and the one referred to in the subsection immediately preceding, both depend on the position of the Moon's perigee with respect to the Sun, they may be combined, and their conjoint amount arranged in tables under one Index. This composite result is the Moon's *Evection:* an inequality discovered by Ptolemy, and amounting sometimes to 1° 15′; *i.e.*, the Moon's longitude is, by the influence of these effects, as represented by *Evection*, sometimes in excess and sometimes in defect of its mean value, by the large angle now specified.

III. The Inequalities in the Lunar Motions just discussed, are the more important results of the disturbing action of the Sun; and their physical causes have been evolved during the long prose-

ution of the famous *Problem of the Three Bodies.* The list, as given, must not however be considered as exclusive. The Moon is disturbed in many other ways, although by quantities comparatively slight. Three of these residual perturbations are of so much interest in their bearing on the general mechanism of the Heavens, that, notwithstanding the length of this article, we feel it right to describe them.

(1.) *Inequalities depending on the Oblate figure of the Earth.*—It is well known that our globe is not a perfect sphere, but rather an ellipsoid, whose shortest diameter is the polar. See EARTH, FIGURE OF. Now, so close are the sympathies of the celestial orbs, that even this comparatively slight deviation from the spherical form (the equatorial diameter exceeds the polar by only the $\frac{1}{30}$th part), is the origin of definite and palpable modifications in the motions alike of the Moon and the Earth. Our strictly Telluric system owes to that Equatorial protuberance one of its most interesting and fertile peculiarities,—fully described under PRECESSION. These motions of our polar axis, designated by the terms *Precession* and *Nutation*, are due in great part to the action upon it of the Moon; and in return—in virtue of the Law of the Equality of Action and Reaction—the oblateness of the Earth affects the Moon with Inequalities. They exist, and have been observed and measured, alike in our Satellite's longitude and latitude: and, strange to say, from the *observed amounts* of these perturbations, the degree of the Earth's oblateness may be determined quite as accurately as by any method depending on terrestrial measurements. Deduced from the terms referred to, the compression of our globe amounts to $\frac{1}{305}$: nearly the mean of the different values obtained by other methods.

(2.) *The Moon's Secular Acceleration.*—The inequality itself was first detected by Dr. Halley. In comparing the dates of modern eclipses with the best authenticated dates of ancient ones, this astute inquirer, discerned that the moon's mean revolution is now performed in a shorter time than at the epoch of the Chaldæans and Babylonians. And as this diminution appeared to have been uniformly increasing, there was no alternative but to infer a gradual acceleration of the Moon's period, or, what is the same thing, a gradual lessening or drawing in of her orbit. The significance and physical cause of this *secular acceleration* remained unknown until the times of Laplace. It had naturally attracted great attention, and various hypotheses were offered to account for it. According to one class of Inquirers, it was due to the effect of a retarding or resisting medium, diffused through the interplanetary spaces: according to others, peculiarities in the action of gravity unknown before, could alone explain the anomaly. The discovery of the simple and remarkable truth was reserved for the illustrious Frenchman. On referring to our account of the *Annual Equation*, (*a* I., *supra*), the student will see that the mean effect of the Sun's central disturbing force is to diminish the Earth's attractive power over the Moon; and that this effect is greater when the Earth is at perihelion than when we are at aphelion. The greater the difference, too, between the Earth's distances from the Sun at these two positions, the greater will this mean disturbance be,—provided always that our planet's mean distance from the Sun remains unaltered. But this amounts to saying, that if the eccentricity of the Earth's orbit should increase, this disturbing force of the Sun would increase along with it; and, conversely, *if the eccentricity of that orbit should diminish, the Sun's disturbing force over the Moon, would, in so far as this action is concerned, diminish also.* Now, in consequence of the perturbing action of the other planets of our system, upon the Earth, the eccentricity of our orbit, is subject to a gradual change, one that is oscillating like all the other perturbations, but whose cycle of alternate diminition and increase spreads over a vast period. And at present *this eccentricity is in its declining phase.* From the earliest periods of history, then, the Sun's power to diminish the Earth's attractive force over the Moon has been growing less and less; and this is equivalent to a *gradual increase of the attractive power.* As an inevitable consequence, the moon must through ages have been gradually approaching the Earth, and her mean revolution accelerating. One most interesting result came out of this solution by Laplace. The change of the Earth's eccentricity is *periodic:* the Moon's departure from her mean place must, therefore, be periodic also; so that the acceleration which has endured so long in nowise threatens the stability of the system.—The whole of this curious subject has recently been reviewed by Mr. Adams of Cambridge. He has completed Laplace's exposition by pointing out and supplying omissions, fatal to a determination by this method of the actual amount of the acceleration. And this was accomplished by his taking into view the influence of the primary step in this series of perturbations, on the Sun's disturbing *tangential* force.

(3.) *Perturbations due to the Planet Venus.*—Every planet in our system disturbs the Lunar Orbit and Motions as certainly as the Sun does. But the effects thence arising are too small to be appreciable except in one instance. Venus is the planet nearest our terrestrial system, and its magnitude is considerable. The reciprocal influences of Venus and the Earth give rise to a long inequality in the motions of both orbs; but it was reserved for Mr. Airy, in 1847, to ascertain that through what may be termed an indirect effect of this inequality, an inequality of long period is also impressed upon the Moon. Soon after this achievement by our Astronomer Royal, Professor Hansen, of Seeberg, further determined a separate disturbance of our Satellite due to the same planet, and arising out of a remarkable numerical

relation between the anomalistic motions of the Moon, and the sidereal revolutions of Venus and the Earth. The periods of these two inequalities are 239 and 237 years respectively, and their coefficients are 23·2″ and 27·4″. To use the words of Sir John Herschel,—"their discovery may be considered as a practical completion of the Lunar Theory, at least for the present astronomical age, and as establishing the entire dominion of the Newtonian Theory and its analytical application over that refractory Satellite."

It were wrong to conclude this article without earnestly referring the student to Newton's own *Lunar Theory*, as one of the finest pieces of geometrical composition and physical deduction extant. A brief and instructive analytical review of the subject in its modern state, has recently been given by Mr. Godfray.—Some general reflections on the character of our Celestial Mechanism as a whole, will be found under PERTURBATIONS.

Lunation. The time between two successive new moons.

Lune. The area included between the arcs of two circles that intersect each other. The lune of Hippocrates is famous as being the first curvelineal space, whose area was exactly determined.

Lupus. (The Wolf). One of the ancient constellations. It is properly part of the Centaur, and was carried in his hand towards Ara (the Altar). It is now represented as transfixed by his spear. Its principal star, α Lupi, is of the third magnitude, and its second is between that and the fourth.

Lutetia. One of the Asteroids. For Elements, see ASTEROIDS.

Lynx. One of Hevelius' constellations directly in front of Ursa Major. It has no star above the fourth magnitude. The head of the Lynx is situate between Ursa Major and Capella.

Lyra. One of the ancient constellations. It is surrounded by Cygnus, Aquila, Hercules, and Draco. The star Vega, or α Lyræ, is of the first magnitude. The line between the pole star and it, is nearly perpendicular to one through the pole star, and the middle of Cassiopeia and Ursa Major. The stars, β and γ, Lyræ, are also brilliant, and of the third magnitude. A celebrated *ring nebula* is in this constellation.

M

Machines are bodies, or assemblages of bodies, which transmit and modify motion and force. The word "Machine," in its widest sense, may be applied to every material substance and system, and to the material universe itself; but it is usually restricted to works of human art, and in that restricted sense it will be here employed. A machine transmits and modifies motion when it is the means of making one motion cause another; as when the mechanism of a clock is the means of making the descent of the weight cause the rotation of the hands. A machine transmits and modifies force when it is the means of making a given kind of physical energy perform a given kind of work; as when the furnace, boiler, water, and mechanism of a marine steam engine are the means of making the energy of the chemical combination of fuel with oxygen perform the work of overcoming the resistance of water to the motion of a ship. The acts of transmitting and modifying motion, and of transmitting and modifying force take place together, and are connected by a certain law; and until lately, they were always considered together in treatises on mechanics; but recently great advantage in point of clearness has been gained by first considering separately the act of transmitting and modifying motion. The principles which regulate this function of machines constitute the *theory of pure mechanism;* a branch of *Cinematics*, or the science of Motion considered in itself by the aid of Geometry, without reference to its cause. The principles of the theory of pure mechanism having been first established and understood, those of the *theory of work*, which regulate the act of transmitting and modifying force, are much more readily demonstrated and apprehended than when the two departments of the theory of machines are mingled. The establishment of the theory of pure mechanism as an independent subject has been mainly accomplished by the labours of Mr. Willis, to whose work on that subject the reader is referred for its details.

Outline of the Theory of Pure Mechanism.—The general problem of the theory of pure mechanism may be stated as follows:—*Given, the mode of connection of two or more moveable points or bodies with each other, and with certain fixed bodies; required, the relations amongst the motions of the moveable points or bodies.*—The theory of pure mechanism is applicable to those cases only in which those dimensions and figures of the bodies constituting the machine upon which the transmission of the motion depends, do not vary to an appreciable extent with the forces applied during the action of the machine. Hence, the only case in which the theory of pure mechanism is applicable to the transmission of motion by means of a *mass of fluid*, is that in which the density of the fluid does not appreciably vary during the action of the machine; in which case the solution of the before-stated problem is as follows:—Let a mass of fluid of invariable volume be enclosed in a vessel, two portions of the boundary of which (called *pistons*) are moveable inwards and outwards, the rest of the boundary being fixed. Then, if motion be

ansmitted between the pistons by moving one wards and the other outwards, it follows, from ie invariability of the volume of the enclosed uid, that the velocities of the two pistons at ich instant will be to each other in the inverse tio of the areas of the respective projections of e pistons on planes normal to their directions of otion. This is the principle of the transmission motion in the *hydraulic press* and *hydraulic ane*. When the body by means of which motion transmitted is of invariable length, but vari- le figure, as when one point transmits motion another by means of a flexible cord passed and pulleys, the velocities of the said points at y instant are inversely proportional to the pective sums of the cosines of the angles made th the direction of motion of each point by e several plies of the cord which are directly nnected with it. When the body by means of ich motion is transmitted is of *invariable figure*, well as of invariable dimensions, the problem generally of a more complex character, and re- ires subdivision. The simplest case is that in ich the thing required is the relation amongst the tions of the different points of one solid body. is is generally determined by the mode of its nnection with some fixed body. When the posi- ns and motions of any three points in a rigid id body, not being in one straight line, are deter- ned, the positions and motions of all points in t body are also determined; and in order that positions and motions of all the points of a id body may be determined, three at least of points, not in one straight line, must have ir positions and motions determined. The tion of a point is determined by three things; *form of its path*, and the *velocity and direc-* of its motion at each point of that path; *velocity* may be made to include *direction* by being understood that one direction along the h is to be considered as positive, and the op- te direction as negative. If three points at t in a moveable rigid body, not in one straight , be so connected with fixed bodies as to re in parallel, equal, and similar paths, with al velocities at each instant, then all the points the moveable body will move in parallel, al, and similar paths, with equal velocities, any line drawn between any two points of body will remain unchanged in direction ng the motion. This constitutes a motion of *le translation*. In actual machines most of movements of simple translation take place in ght lines, the paths of the points of the mov- piece being determined either by fixed guides, y combinations of link-work called *parallel ons*. *Rotation* is a relative movement of the ts of a body, such that a line drawn between of them changes its direction. An *axis of ion*, in the general and geometrical sense, *y line in the body* whose *direction* is not ged by the rotation; a property possessed by nes parallel to a certain direction. In a re- stricted sense of the word *axis*, which ought rather to be specified as *fixed axis*, it means that line whose *position*, as well as its direction, is un- changed by the rotation; and this, in a rotating piece of mechanism, is generally the centre line of a shaft or axle turning in fixed bearings. There cannot be more than one fixed axis in a rotating body, and there may be none whatsoever. In the latter case it is desirable, for the sake of simplicity, in analyzing the motions of the points of the body mathematically, to refer them either to the axis of rotation which traverses the centre of gravity of the body, or to that axis of rotation whose path of translation is the shortest. A *plane of rotation* is any plane in which the rapidity of the change of direction of a line drawn between two points is a maximum; every plane perpen- dicular to the direction of the axes of rotation has this property. The *angular velocity* of rotation is the angle through which any line in a plane of rotation changes its direction in an unit of time: and is necessarily the same in all parts of the same rigid body; so that the condition of rota- tion with a given angular velocity is one which- if it exists in a rigid body as a whole, exists in. each of its parts, how small soever. Angular velocity may be expressed in degrees per hour, minute, or second, or in turns per hour, minute, or second; for theoretical purposes, however, the best mode of expresssng it is in *length of arc to the radius unity* per second; and thus it is to be understood when not otherwise specified. The motion of any point, A, relatively to any other point, B, in a rotating rigid body, is one of *re- volution* or *translation in a circular path* round the axis passing through B, with a linear velo- city equal to the product of the angular velocity into the perpendicular distance of A from the said axis; and the motion of B relatively to A is exactly the same with that of A relatively to B. The linear velocities of any two points in a ro- tating rigid body relatively to the axis passing through a third point, are to each other in the ratio of the respective perpendicular distances of the first two points from the said axis; and this is the principle of the modification of motion by the *lever* and the *wheel and axle*. The motion of a rigid body, when it is neither one of *simple trans- lation*, nor of *rotation about a fixed axis*, may be analyzed for mathematical purposes into a motion common to all the points of the body with an assumed axis of reference, and a rotation about that axis. For the sake of convenience, it is in general advisable to assume for an axis of refer- ence, either the axis traversing the centre of gra- vity of the body, or that axis whose proper motion is the shortest or the most simple. When one piece of a machine, A, is connected with another piece, B, and B with the fixed framing, C, so that A has a given motion relatively to B, and B a given motion relatively to C, then the motion of A, relatively to C, is found by the *geometrical adding*, *putting together*, or *taking the resultant*

of, the two given motions. If each of the two sides of a parallelogram represents in direction and length the direction and velocity of a motion of translation, the diagonal will, in like manner, represent the resultant motion. If the two sides of a parallelogram represent in direction the axes, and in length the angular velocity, of two motions of rotation, the diagonal will, in like manner, represent in direction the axis, and in length the angular velocity, of the resultant motion. When motion is *transmitted from one piece of a machine to another*, that whose motion is the cause is called the *driver*, that whose motion is the effect, the *follower*. The *connection* between the driver and the follower may be—1. By *rolling contact* of their surfaces, as in *toothless wheels*; 2. By *sliding contact* of their surfaces, as in *toothed wheels*, *screws*, *wedges*, *cams*, and *escapements*; 3. By *wrapping connectors*, such as *belts*, *cords*, and *gearing-chains*; 4. By *link-work*, such as *connecting rods*, *universal joints*, and *clicks*; 5. By *reduplication of cords*, as in the case of ropes and pulleys, already discussed. The various cases of the transmission of motion from a driver to a follower are further classified, according as the relation between their *directions* of *motion* is constant or changeable, and according as the ratio of their *velocities* is constant or variable. In every case except that of reduplication of cords, the principle of which has already been given, there is at each instant a certain straight line, called the *line of connection*, or *line of mutual action* of the driver and follower. In the case of rolling contact, this is any straight line whatsoever traversing the point of contact of the surfaces of the pieces; in the case of sliding contact, it is a line perpendicular to those surfaces at their point of contact; in the case of wrapping connectors, it is the centre line of that part of the connector by whose tension the motion is transmitted; in the case of link-work, it is the straight line passing through the points of attachment of the link to the driver and follower. The line of connection of the driver and follower at any instant being known, their relative velocities are determined by the following principle: —*The respective linear velocities of a point in the driver, and a point in the follower, each situated anywhere in the line of connection, are to each other inversely as the cosines of the respective angles made by the paths of the points with the line of connection.* Some of the consequences of this principle are the following. When the driver and follower both rotate round fixed axes, it is essential to rolling contact that the point of contact and the two axes should be in one plane; and in this case the respective angular velocities of the driver and follower are to each other inversely as the perpendiculars let fall from the point of contact on their respective axes. In order that two toothed wheels may have the same relative motion from the sliding contact of their teeth, which they would have if toothless, from the rolling contact of their pitch lines, each pair of tooth-surfaces, in the driver and follower respectively, which are intended to work together, should be described by rolling the same curve of any figure, on the same side of the respective pitch lines of the driver and follower. The relative velocity of a driver and its follower is sometimes made capable of being changed at will, by means of apparatus for varying the position of their line of connection; as when a pair of rotating cones are embraced by a belt which can be shifted so as to connect portions of their surfaces of different relative diameters. A *train of mechanism* consists of a series of moving pieces, each of which is follower to that which drives it, and driver to that which follows it. A *mechanical notation* has been contrived by Mr. Babbage, by which the nature, mode of connection, and motion of any train of mechanism, how complicated soever, can be clearly expressed by a system of symbols.

Outline of the Theory of the Work of Machines. —A force applied to a piece of mechanism is a *power* if it acts in the direction of the motion of its point of application; a *resistance* of it acts in the opposite direction; and a *lateral stress*, if it acts at right angles to that direction. All the forces applied to a piece of mechanism may be *resolved* or *analyzed* into *powers*, *resistances*, and *lateral stresses*. The only direct effect of a *lateral stress* is to strain the material of the mechanism and framework. It has an indirect effect in increasing the resistance by producing friction; but when the amount of the resistance so produced has been ascertained and included amongst the resistances in general, the lateral stress is to be left out of consideration so far as the work of the machine is concerned. A *reciprocating force* or *active resistance* is a force which acts alternately as a resistance and a power of equal intensity, according as its point of application moves in one direction, or in the opposite direction. Of this kind is the *weight* of any piece of the mechanism whose centre of gravity alternately rises and falls, being a *resistance* during the rise, and a *power*, of equal intensity, during the fall. Of this kind also is the elastic force of a spring, and of a mass of compressed air. A *passive resistance* is a force which either acts as a resistance in what direction soever its point of application moves (such as friction), or which acts as a resistance when its point of application moves in one direction, and ceases to act when that motion is reversed (such as the resistance of soft bodies to pressure). The passive resistance at the point where the useful work of the machine is performed is *useful resistance*; all other passive resistance is *useless* or *prejudicial resistance*, as being foreign to the purpose of the machine. This distinction of resistance into useful and useless has reference to human purposes only, and not to the order of the universe. A power may be employed either to balance a resistance or to produce

-rease of the velocity of a piece of the mechanism; e power employed in the latter way is called ving force, and its ratio to the weight of the ece accelerated is that of the increase of velocity oduced in a second to the increase of velocity oduced by gravity in a second, the value of ich last quantity is (to an accuracy sufficient the purposes of mechanism) 32·2 feet per cond, and is denoted, for brevity's sake, by g. et v, then, be the velocity of a piece of the echanism whose weight is m, and inertia $\frac{m}{g}$, d dv the increase which such velocity under-es in an instant of time, dt, then $\frac{m}{g} \frac{dv}{dt}$ is the oving force, which, inasmuch as it employs rt of the power which would otherwise act in lancing resistance, may be treated in compu-tion *as a resistance.* On the other hand, a sistance may be overcome, either by being lanced by a power, or by being employed in oducing diminution of the velocity of a piece of e mechanism. In this case the moving force oducing the retardation $-dv$ during the in-nt dt in the weight m, is represented by $-\frac{m}{g} \cdot \frac{dv}{dt}$, and, inasmuch as it employs part the resistance which would otherwise act in posing power, it may be treated in computation *a power.* In all actual machines, the velo-ies of the pieces of the mechanism when not nstant are *periodical*, being alternately accele-ed and retarded; so that the moving forces to be treated as *reciprocating forces*, acting resistance during the acceleration, and as a wer during the retardation.—*Work* is the oduct of a resistance overcome into the tance through which its point of applica-n is moved against it. Work is expressed merically by multiplying the resistance in its of weight (such as pounds) by the distance rough which it is overcome in units of length ch as feet). The unit of work employed in itain is called a *foot-pound;* that is, the work formed in lifting a mass weighing one pound rough the elevation of one foot. The *rate of rk*, or *duty*, at a given point, is the work per-med in an unit of time, and is the product of e resistance into the velocity. When a body ates round a fixed axis, the product of an plied force by the perpendicular distance of its int of application from that axis is called the *ment* of the force; and accordingly the rate work in overcoming a resistance applied to a ly rotating on a fixed axis, is the product of *moment of the resistance* into the *angular ocity.* When resistances are applied to various nts of a piece of mechanism having a move-nt of simple translation, the work is found by ltiplying the sum of the resistances by the mmon distance through which all their points application are moved alike, and the rate of work is the product of the sum of the resistances into the common velocity. When the resistance is expressed as a *pressure per unit of area*, the work is the product of that pressure into the cubic space moved through by the surface to which it is applied. When the points of appli-cation of the resistances have different velocities, the work is the sum or integral of the products of each resistance into the space described by its point of application. For example, if resistances be applied at various points of a body rotating round a fixed axis, the rate of work is the pro-duct of the sum of the moments of the resistances into the common angular velocity. The work performed in accelerating the velocity of a piece of mechanism of the weight m, having a movement of translation, from the amount v_1, to the amount v_2, is equal to that which would be performed in lifting the same piece to the height from which it would have to fall in order to be similarly ac-celerated; that is to say, the amount of the said work is represented by $\frac{m\left(v_2^2 - v_1^2\right)}{2g}$; or the product of the inertia of the piece into half the difference of the squares of its velocities before and after acceleration.—When the different por-tions of the piece have different velocities, or are differently accelerated, the work of acceleration is ascertained by conceiving the piece to be divided into small molecules, multiplying the weight of each by the height from which it would have to fall to produce its own proper acceleration, and taking the sum or integral of the products. For example, let the piece of mechanism rotate about a fixed axis with an angular velocity a; let it be con-ceived to be divided into a number of molecules, and let the weight of any one of those molecules be denoted by $d\,m$, and its perpendicular distance from the axis by r, so that its linear velocity is $a\,r$; then the work performed in accelerating the angular velocity from the amount a_1 to the amount a_2 is

$$\frac{a_2^2 - a_1^2}{2} \cdot \frac{1}{g} \int r^2 \, d\,m.$$

The quantity $\frac{1}{g} \int r^2 \, d\,m$ is the *moment of inertia* of the rotating mass.—Energy means *capacity for performing work.* The *energy of a power*, or *Potential Energy*, is the product of a power, in units of weight, into the distance in units of length through which it is capable of moving its point of application. The *energy re-ceived* by a machine is the work performed by the powers at their points of application. The *energy of a moving mass*, or *Actual Energy*, is the work which that mass is capable of performing by overcoming resistance during retardation from its actual velocity to a state of rest, and is the product of its inertia into half the square of its linear velocity, if the motion be one of simple

translation, or of its moment of inertia into half the square of its angular velocity, if the motion be one of rotation about a fixed axis. The work performed in overcoming the resistance of a *reciprocating force* becomes *stored potential energy*, available for the purpose of overcoming passive resistance when the motion of the point of application of the reciprocating force is reversed; and in like manner the work performed in accelerating the motion of a piece of the mechanism becomes *stored actual energy*, available for the purpose of overcoming passive resistance when the motion of the piece is retarded. The work performed by means of stored energy may be called *restored energy*. The work performed in overcoming *passive resistance* is irrecoverably expended, partly in *lost work*, being that performed in overcoming *useless resistances*,—partly in *useful work*. Its energy continues to exist in the universe, but not in a form available for the purposes of the machine. —These definitions and explanations having been premised, the following is the General Law of the Work of Machines:—*In any given time, the Energy received, added to the restored energy, is equal to the stored energy added to the work performed.* To express this symbolically, let P be any power applied to the machine; dp the space described by its point of application in a given time; P′ a reciprocating force acting as a power;—dp' the space described by its point of application; R′ a reciprocating force acting as a resistance; dr' the space described by its point of application; Q a useless resistance; dq the space described by its point of application; R a useful resistance; dr the space described by its point of application; E the increase of actual energy of a moving mass which is being accelerated; E′ the diminution of the actual energy of a moving mass which is being retarded; and let Σ denote the summation of terms referring to different points of the machine: then

The energy received $= \Sigma \cdot \mathrm{P}\, dp$;
The energy restored $= \Sigma \cdot \mathrm{P}'\, dp' + \Sigma \cdot \mathrm{E}'$;
The energy stored $= \Sigma \cdot \mathrm{R}'\, dr' + \Sigma \cdot \mathrm{E}$;
The work performed $= \Sigma \cdot \mathrm{Q}\, dq + \Sigma \cdot \mathrm{R}\, dr$;

and the expression of the general law is

$$\Sigma \cdot \mathrm{P}\, dp + \Sigma \cdot \mathrm{P}'\, dp' + \Sigma \cdot \mathrm{E}' = \Sigma \cdot \mathrm{R}'\, dr' + \Sigma \cdot \mathrm{E} + \Sigma \cdot \mathrm{Q}\, dq + \Sigma \cdot \mathrm{R}\, dr.$$

Perpetual Motion is a term applied to delusive machines, whose inventors, ignorant of the law above stated, expect them to perform work without receiving energy. Several such machines are patented in each year.—If, as is almost always the case in practice, the motion of a machine is *periodic*, so that, at the end of each period, stroke, or revolution, the points of application of the reciprocating forces return to their original positions, and each part of the mechanism resumes its original velocity; then the General Law takes the following form:—*In each period, the Energy received is equal to the work performed.*—Let $\int$ denote the summation or integration of the quantities of energy exerted and work performed in the several instants of a period; then

$$\Sigma \int \mathrm{P}\, dp = \Sigma \int \mathrm{Q}\, dq + \Sigma \int \mathrm{R}\, dr.$$

The Efficiency of a Machine is the ratio of the *useful work* to the *energy expended*, or

$$\frac{\Sigma \int \mathrm{R}\, dr}{\Sigma \int \mathrm{P}\, dp}$$

The *Modulus* of a machine is an equation showing the relation which its efficiency bears to its velocity, to the form and dimensions of its parts, to the forces applied to it, and to the other circumstances concerned in its working.—In designing a machine, the velocities and paths of the points of application of the power, are to be determined by the nature of the source of energy, so as to enable it to act to the best advantage. The velocities and paths of the points at which the useful work is performed are to be determined by the nature of the work, so that it shall be done in the best way. The intermediate train or trains of mechanism are to be adapted to transmit and modify the motion so as to suit the already assigned velocities and paths, with the least possible expenditure of work in overcoming useless resistance; that is to say, generally speaking, with the simplest possible mechanism. Each part of the mechanism and framework is to be made strong enough to resist the forces applied to it, not only without injury to the material, but without such alteration of figure as would impair the accuracy of the working of the machine.—The above stated law of the work of machines is a particular case of the general law of the Conservation of Energy, which regulates all the phenomena of the universe.—*Authorities.* Willis *On Mechanism;* Sang *On the Teeth of Wheels;* Poncelet, *Mécanique Industrielle;* Morin, *Notions Fondamentales de Mécanique;* Moseley's *Mechanics of Engineering and Architecture;* Rankine *On Applied Mechanics;* Rankine *On Prime Movers.*

Maculae (Solar). Spots upon the surface of the Sun. For a description of their probable nature and physical cause, see Sun.

Magellanic Clouds. Whitish appearances like clouds seen in the Southern Heavens, but with the apparent motion of the stars. There are three of them. They have the same indistinct milky appearance as we observe in the galaxy or milky way, and from the same cause—the density of the stratum of stars and their enormous distance—making them appear like star-dust.

Magic Lantern. See LANTERN, MAGIC.

Magnet: Magnets: Magnetization. The bject of the following article cannot be treated thout reference to principles, the development of ich is placed under MAGNETISM and ELECTRO-YNAMICS. Nevertheless, for the purpose of eing our theoretical discussion of the general estion of Magnetism from unnecessary con-ction with separate although collateral details, shall discourse, in this place, of Magnets— eir formation and properties.—The most gen-l, as well as, perhaps, the most correct de-iption of the primary or fundamental fact of gnetism, is this:—if a piece of iron or steel exposed sufficiently long to certain terrestrial nences—if it be held for a time in a certain inite position—if it be filed, hammered, twisted, , it becomes capable of attracting metallic gments and even of lifting pieces of iron of ne magnitude. This attractive power belongs an ore of iron, hence called the *natural magnet oadstone;* and, indeed, there is reason to believe t it resides, in greater or less intensity, in every siderable mass of the matter of our globe. atever approximation has been made to the ory of this singular activity, will be explained he course of the two succeeding articles: the now to be dealt with, is the following,—the liar power just specified, can be *communicated,* the presence of a body possessing it, or by ain processes, to bodies not possessing it in reciable degree,—hence named *artificial mag-* . We intend to describe the various modes of *netizing,* and the laws, in so far as they are wn, that regulate the virtue of those artificial nets. We shall, further, advert to certain es of external circumstances that affect Mag-zation, and certain Molecular Actions that riably attend it.

MAGNETIZATION.—There are two, appa-ly distinct, modes by which Magnetism can ommunicated to an indifferent bar or mass of or iron: the first depending on the presence her natural or artificial magnets;—the second perations already in part explained under CTRO-DYNAMICS.

.) *Magnetism induced by other Magnets.*— simplest illustration of the fact of the com-ication of Magnetic attributes in this way, e following:—Let a bar or needle of steel or swinging on its centre of gravity, be brought the pole of a body already magnetized. One of the needle will approach that pole as as possible, and the needle itself will point from the pole. Should the two bodies pre- this relative position for a brief time, the e itself will have become a magnet,—that will attract iron filings or small pieces of and retain them attached to it with some On the removal of the original magnet, ar does not lose the faculty imparted to it has become an artificial magnet:—only is quite a contrast as to the power of different substances to *retain* the faculty so newly and singularly acquired;—a steel needle retains it so long, that the magnetic property would appear to have become a permanent part of its constitution; a needle of soft iron, on the other hand, loses the greater part of it a few instants after the withdrawal of the inducing substance, and returns to a comparatively indifferent state. —The communicability of the magnetic faculty thus established, Inquirers sought the easiest means of communicating it. And here, Duhamel in France, Æpinus, and Mitchell and Canton in England, were early distinguished. The modes first proposed were the methods of *single* and *double touch,* and consisted in the process of slid-ing the poles of artificial magnets in various ways along the bar or bars meant to be magnetized. These processes being well known and also well nigh superseded, we shall not describe them in detail. It is proper, however, to specify the method—in so far as it is understood—and the success of Dr. Knight, who, at the close of last century, devoted much talent and time to this practical subject, and left a memorial of his energy in the famous Steel Magnet—more powerful than any previously known—which is still in posses-sion of the Royal Society. In so far as he ever explained it, Knight's method was this:—bring-ing together the opposite poles of two as potent magnets as he could obtain, he laid the steel bar about to be magnetized upon these magnets, so that its centre should rest upon the junction of their poles. Then he gradually withdrew the magnets, or made their opposite poles slide slowly along the bar, towards its opposite ends. And this process, repeated a few times, endowed the bar with a very high magnetic influence. It is quite probable, however, that the success of Dr. Knight depended as much on his persevering care and patience in repeated trials, as on any special virtue in his process. The great magnet which Knight termed his "reservoir of Magnetic Force," was once far more powerful than it is at present:—a fire occurring in the house where it was deposited after its maker's death, having bereft it of much of its energy. Still, a power of nearly an hundred-weight must be applied to detach its armature from the poles. Knight's magnet is composed of 450 magnetized bars, each fifteen inches long, one inch wide, and half-an-inch thick. These present at their extremities two poles coming out hori-zontally to a length of *six* inches, a height of *twelve,* and a width of *three.*—We wish we had leisure to describe those remarkable experiments by Dr. Scoresby, in which permanent magnets of high intensity were ultimately deduced from the feeble intensity obtained by hammering bars placed in the magnetic inclination. But the subject belongs rather to obscure questions re-ferred to in subsequent sections.

(2.) *Magnetization by Electric Currents.*—Soon after the discovery by Oersted of the magnetic effect of an electric current, Arago established

that such a current attracts iron filings and produces magnetization just as a magnet would do. And to the same acute physicist, along with Davy, we owe the capital fact, that if the conductor of a current be bent into a helix, a needle placed in the axis of that helix will become a magnet —permanent, if it be of tempered steel, temporary, if of soft iron. It subsequently appeared—in rigorous agreement with deductions from Ampere's theory—that in a *right-handed* helix, viz.: one in which the wire is wound to the right, the south pole of the needle is always at the extremity through which the discharge or the current *enters;* whereas, in a *left-handed* helix, the north pole is at the extremity through which positive electricity enters.—In the present section of this article, we shall take notice only of the practical manipulations depending on the foregoing principle. And, *first*, as to the mode in which the electric current may be used in the production of permanent magnets. The best form of manipulation, by aid of electricity, seems to have been proposed by M. Elias of Haerlem, about the year 1843; and certainly it is a method by which the smallest needle as well as the heaviest steel bar can be instantly magnetized to saturation, with the greatest facility. All the apparatus necessary is some twenty-five or thirty feet of copper wire of about one-eighth of an inch in thickness, and a powerful voltaic pair. The wire must be wound so as to form a hollow, very short but very thick cylinder; and a strong electric current then passed through it. The steel bar with soft iron armatures at both ends, or a horse-shoe bar with an armature, must be placed within the hollow of the cylinder, and moved up and down to its very ends. When the central position of the steel bar again occupies the cylinder, the circuit should be opened, and the bar—now perfectly magnetized—be withdrawn. So efficient is this simple method, that the poles of some very powerful bars, magnetized by Dr. Knight himself, were reversed by the effect of a single passing. A few magnets, endowed in this way, by M. Logeman, optician at Haerlem, were shown at the meeting of the British Association in Edinburgh, by Sir David Brewster, in 1850; and their power astonished every one. The following formula has been given by M. Häcker for the greatest lifting power of a permanent steel magnet of the weight N.

$$P = 10{\cdot}33\ N^{\frac{2}{3}}$$

The magnets executed by the best makers in Europe, according to the usual process, rarely come up to the efficiency indicated by this formula; but those of M. Logeman have twice that efficiency. One of these magnets, weighing only about one English pound, could support *twenty-seven* times its own weight: a second, weighing twelve and a-half pounds, supported a weight of 150 pounds; and the great magnet, now in possession of the Royal Institution, London, weighing only fifty-two pounds, sustains no less than 430 pounds. The permanence of the magnetism, seems as remarkable as the great power of these magnets. Sir David Brewster states that though its armature were torn away twenty or even a thousand times, the magnet would still carry as great a weight as before.—But, *secondly*, the greatest acquisition to the effective magnetic power within reach of the Physical Experimentalist, has come in the form of *Electro-Magnets*, properly so called. If a bar or needle of *soft iron* be placed within a helix through which a current is flowing, it becomes instantaneously a temporary magnet; and that magnetic faculty or virtue continues as long as the current. The merit of first constructing an effective horse-shoe magnet of this kind, is unquestionably due to the late Mr. Sturgeon,—a gentleman whose positive deservings have not been adequately acknowledged. Professor Moll of Utrecht, obtained magnets so made, from Mr. Watkins of London; but he did nothing further than apply the original idea inaugurated by Sturgeon. The greater part of our subsequent advance, in the construction of Electro-Magnets, as well as a considerable portion of our accurate results as to the laws of their power, is owing to Mr. Joule of Manchester—an Inquirer who has already secured a place in Scientific History that will rank him with the best discoverers in our time. Mr. Joule's theoretical investigations shall be noticed immediately: we refer at present only to his processes of Electric Magnetization and their results. He has given two forms to his chief electro-magnets. The *first*, made so early as 1840, is thus described by himself:—"A piece of cylindrical wrought iron eight inches long had a hole, one inch in diameter, bored through the entire length of its axis; one side of it was then planed away until the hole was laid open through its entire length. Another piece of iron, also eight inches long was then planed, and having been secured with its face in contact with the other plane surface, the whole was turned into a cylinder eight inches long, three and three-quarter inches in diameter, and one and one-fourth inch in the diameter of the bore. The larger piece, intend

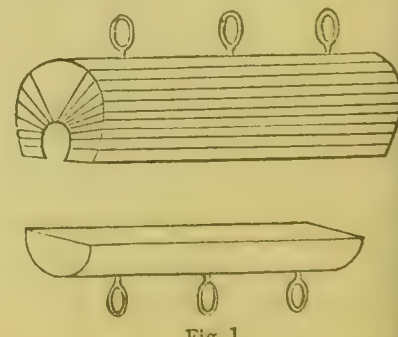

Fig. 1.

for the electro-magnet, was then wound rou with four copper wires, each of which was twen

hree feet long, one-eleventh of an inch in diameter, and covered with silk." The foregoing gure shows this electro-magnet and its armaure. It was shown at the Exhibition in 1852. he weight of the entire magnet was only fifteen ounds; and yet Mr. Joule found that the cirulation of a current from a powerful battery nabled the keeper or armature to resist force up a weight of 2,090 pounds! This great electroagnet, therefore, could exercise a retaining power ver a mass *one hundred and forty times* its own eight! But the marvel is increased by the atement, that *small* magnets have been made Mr. Joule, by similar arrangements, capable sustaining 3,500 times their own weight. ne of these, which weighed less than half a rain, was given by Mr. Joule to Dr. Roget.—r. Joule put more effectively into practice, in second effort, a principle of whose accuracy had become convinced, viz.; that if a particle wire conducting a voltaic current be made to t upon a very large surface of iron, the intensity the induced magnetism will not be much minished by an increase in the distance of the rticle from the surface of the iron. The follow- are the details of the electro-magnet now to described, and in which full advantage is taken the foregoing principle:—A plate of the best ought iron was obtained, one inch thick, enty-two inches long, twelve inches broad t the centre, and tapering from the centre, ough a curve, until at the edges the bar was y three inches broad. This plate was then t into a semicircular shape, so as to bring its s within twelve inches of one another. Next, was engirt by a coil consisting of a bundle of per wires, sixty-eight yards long, and weigh- one hundred pounds. The arrangement of great electro-magnet, in a wooden box proed for it, will best be understood by aid of the joined sketch.

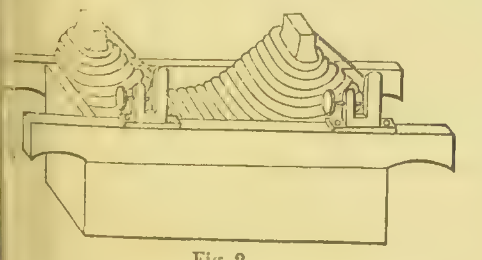

Fig. 2.

important results of the experiments made Mr. Joule with this magnet, are adverted to v: his general conclusion is, that the greatest ht that can be lifted by an electro-magnet ed of a bar of iron one inch square, bent into nicircular shape, is *four hundred pounds.*— requisite to add, that the form of the conon of mechanical energy obtained by the o-magnet is the only one yet known, in an almost complete effective conversion may be attained. It is not so much the loss of power in the conversion of energy that constitutes the inutility of electro-dynamic machines, as the expense of zinc, the oxidation of which is employed as the source of energy.

(3.) *Measure of the Magnetic Efficiency of Electric Currents. The Point of Saturation. The Coercitive Force.*—The nature of the general relationship between Magnetic Currents and the Magnetic Faculty having been ascertained, the question that next attracts attention is this,—what is their *exact* or *numerical* relationship? Through what formulæ can we deduce, under given conditions, the equivalent Magnetic Force of a certain amount and manner of Electric Force? Although no answer fully satisfactory can yet be given, the subject has obtained the notice and engaged the researches of our most distinguished physicists—such as Lenz and Jacobi, Joule, Thomson, Feilitzsch, Wertheim, Poggendorf, Plücker, Dub, &c., &c. There are two points of especial importance necessarily leading to exceptions or apparent breaches of continuity in any Laws that may be established on this subject; and it is not improbable that partial oversight of these may account for the apparently discrepant results of several of the foregoing Inquirers. In the *first* place, it is not to be expected that any law or rule regarding the magnetizing efficiency of electric currents, should hold when the bar, subjected to them, has nearly reached its maximum magnetic efficiency, or its point of *saturation.* In whatever the magnetic faculty consists, it is abundantly plain that it has intimate connection with molecular arrangement; and it is easy to see that any force—be it what it may—tending to evolve this molecular arrangement, will probably not act *proportionally*, after the molecules of the body affected by it have been *nearly all* brought into the position or condition which enables or disposes them to exert the specified virtue. It is only, therefore, at a certain, although at present undefined, distance from the point of saturation, that we ought to expect traces of proportion between the intensity of inducing electric currents, and induced magnetic effect. But Mr. Joule has recently pointed at a *second* cause of discordance in the experiments so assiduously made. The total magnetic action of an electric current consists of two parts that must be carefully distinguished. There is, in the *first* place, a magnetism existing under the pure inductive influence of the current, which is destroyed the moment the current ceases; and, *secondly*, the influence of a magnetism more or less permanently communicated to the bar itself, and henceforth residing in it,—an influence which Mr. Joule designates as the *magnetic set* of the bar. No doubt can exist that these two phenomena belong to different causes;—the latter depending on the constitution of the substance of the bar; the former purely representing, and solely depending on the Magnetic Influence of the Elec-

tric Currents. Mr. Joule's first object, therefore, was to eliminate this *magnetic set* out of the total effect; and he has detected, *generally*, the following laws:—In bars of diameters up to one-fourth of an inch, the *magnetic set* obtained by feeble currents is proportional to the square of the current producing it. This law subsists through a long series of Magnetic Intensities; but when the current is increased, and the diameter of the bars is so low as $\frac{1}{17}$th or $\frac{1}{25}$th of an inch, the set increases in a much higher ratio, varying as the *fourth*, or even the *sixth* powers of the current. This singular increase of the *magnetic set*, when the bar is highly impressed, is remarkably analogous to the law established by Professor Eaton Hodgkinson, respecting the permanent change of figure impressed on a beam of any material,—a change which is proportional to the square of the force applied, until we reach the neighbourhood of the *breaking point.* But the point of greatest importance, and of by far the widest interest, is this,—remove the *magnetic set*, and the doubt as to the proportionality of induced magnetic virtue, to the strength of the Inducing Currents, altogether disappears. It has been propounded by Professor Thomson, that, if the effect of *set* be abstracted from the results of pressure on bodies, the *elasticity* of all bodies will be found *perfect.* The analogy cannot be mistaken here, nor is it unreasonable to hope that so remarkable an analogy between magnetic and ordinary molecular actions, may lead us to a clearer insight into the intimate nature of Magnetism. At all events, the subject is in most competent hands.—Steering clear of the neighbourhood of the point of saturation, and of the disturbing effects of the *magnetic set*, it seems to have been ascertained by Jacobi and Lenz, that, *when two bars of iron of different diameters, but equal to one another in length, and surrounded by coils of wire of the same length, carry equal streams of electricity, the magnetism developed in the bars is proportional to their respective diameters.* Mr. Joule, in his earlier researches, had deduced the following theorem:—*The attractive force of the Electro-Magnet for a bar of iron induced by it is directly as the square of the Electric force to which its iron is exposed; or if* E *denote the quantity of Electricity,* M *the Magnetic attraction of the Iron made magnetic,* and W *the length of the* wire,— $M = E^2 W^2$:—*therefore the pure magnetic effect of the current must be proportional to* E W. The resolution of the apparent conflict of these two views, is undoubtedly in the conception of Professor Thomson, that "*similar bars of different dimensions, similarly rolled, with lengths of wire proportional to the squares of their linear dimensions, and carrying equal currents, cause equal forces at points similarly situated with reference to them.*" ——2. Reference has been made above to the point of *saturation*, or to the *maximum* of the power that any bar may assume under the influence of Electric Currents. Only one further very general proposition may now be adventured here. The magnetic effect of any Electric Current, will ultimately be found equivalent to its thermic effect, or its power to decompose, *minus* the thermic value of that one process which is still indefinite. It is impossible to state the amount of dynamic energy consumed by the molecules of the iron bar, as they take on their new and constrained positions; but we shall obtain light on this subject through these recent researches of Mr. Joule.——3. It is necessary to define in this place, an expression that has become *technical* in practical magnetism,—viz., the *coercitive force.* Reference has been already frequently made to the fact, that steel bars magnetized in any way retain their magnetic faculty; while bars of soft iron, magnetized by Electric currents, rapidly lose this induced faculty. It is a further fact, that inversely as the facility of the loss or departure of the Magnetic Faculty, is the slowness or difficulty with which any bar becomes endowed with it. Now, this difficulty is termed the *coercitive force.* It is the power or operation—whatever that may be—which, in tempered steel, opposes the development of the Magnetic Faculty, and interposes an obstacle to the return of a bar to its natural state, when active magnetization has ceased. Its ultimate cause is plainly the molecular constitution of the body.

II. The Influence of Heat, &c., on the Magnetism of Needles or Bars.—Something has been indicated in the previous sub-section concerning the dependence of the coercitive force on the *temper* of bars endowed with the Magnetic Faculty. We subjoin here a few remarks on the influence of other Physical conditions. In the first place, the *influence of torsion or hammering.* When a bar of iron is either much hammered or twisted, it seems to gain a coercitive power sufficient to enable it to become a permanent magnet. This curious subject has been much studied by Ed. Becquerel and M. Wertheim. Torsion has temporary as well as permanent effects. The temporary effects are mainly these: when a magnet, that is endowed to saturation, is twisted, it partially loses its power, but it recovers that power wholly on being twisted back again. The permanent effects are shown in the case of wires which when twisted have become permanent magnets. If such a wire of soft iron, surrounded by an electric helix, is twisted at the same time, it is found capable of receiving permanent magnetism: if, again, it is twisted anew while in the centre of the helix, its magnetic effects or faculties are *reversed.* According to M. Wertheim, these torsion experiments act in a very special way, by forcing the molecules of the wire to dispose or arrange themselves in the form of spirals, which is precisely the form assigned by Ampere to the Electric current. It is right to mention, however, that Wertheim's conclusions, alike experimental and theoretical, have

been contested. In the same uncertain category must at present be ranked certain supposed results of *Matteucci*, on the magnetic influence of *traction*, or a forcible lengthening of a magnetized bar.—But the important influence on the magnetism of bars, springs from the *Heat* to which they are or may be subjected. There is no doubt as to the reality of the Law that, by heating a magnetic bar, you diminish its magnetic faculty. Coulomb very early discerned this; and he saw besides, that no increase of the mere temper of a steel bar, could withdraw it from the aforesaid influence of heat. Kupffer recently took up the subject, and conceived that he had determined the law according to which the free or effective magnetism of a bar and its temperature vary as two co-ordinates. But the difficulty was met in the best form by Gauss. Possessed of the most delicate instruments, and having got rid by very ingenious methods, of all influence arising from the variations of terrestrial magnetism, Gauss reached the following three conclusions:—1. The variations of the magnetism of a bar, are subjected, when the temperature of the bar is being raised, to laws different from those which regulate these variations, as its temperature is being lowered.—2. The same bar acts differently under changes of temperature, according to the intensity of the magnetism it possesses. When its magnetic faculty is powerful, the bar retains it obstinately, and the change of temperature produces only feeble augmentations or diminutions. If, on the contrary, the magnetic faculty of the bar is feeble, changes of temperature have a powerful influence.—3. Changes of temperature and magnetic intensity are not simultaneous. For instance, an elevation of temperature, once accomplished, continues to act for a considerable time on the intensity of the bar; it diminishes that intensity at first rapidly, but its enfeebling action proceeds slower and slower.—The development of heat during the induction of Magnetism was long ago noticed by Mr. Joule, and has since been confirmed by Grove and Foucault.

III. Molecular Changes produced by the Magnetic Condition.—There cannot now be a doubt that no bar of steel or any substance becomes possessed of the magnetic faculty, without undergoing molecular changes. These are first manifested through *changes of dimension*. A magnetized bar of iron appears to preserve the precise volume or bulk that it had previous to its being magnetized; but though it does not alter in volume, its length increases while its breadth diminishes. We owe the indication and full experimental establishment of this remarkable fact to Mr. Joule. The greatest elongation observed by this latter physicist amounts to the $\frac{1}{180000}$th part of the length of the bar; he states further, that the elongation is proportional to the square of the developed magnetic intensity. He has also intimated that when iron wires, of a certain tension, be used, a diminution of length, instead of an augmentation, is observed, so that at a certain tension no alteration of length whatever occurs. These curious facts will one day conduce to a true theory of the Magnetic force. *Secondly*, Magnetic relations produce *sounds*;—thus clearly indicating their connection with molecular changes capable of impressing impulses on the atmosphere. For instance, if the pole of a powerful magnet be brought near the end of a spiral traversed by an electric current, a sound is heard. Also, if electric currents are transmitted across bars or plates of magnetized iron, there ensue similar phenomena at the moment when the current is opened or closed. In fact, regular musical sounds can be produced by this agency. This interesting subject has been discussed by Fabroni, De la Rive, Matteucci, Wertheim, Wortmann, Marrian, Beatson, and others.—As regards Circular Magnetic Polarization, see that article, as well as the general article following the present one.—*Thirdly*, No doubt remaining as to the close connection between the Magnetic Faculty of bodies and their molecular constitution, the question arises, whether the "coercitive force" may not afford some clue to explanation by leading towards ultimate causes? Assume as a postulate that magnetism is the result of electric currents circulating around material particles or molecules. Although such currents may exist around the molecules of all bodies, it is easy to conceive how intimate or rather ultimate physical differences, must determine the facility with which the molecules can be compelled to arrange themselves, so that these ambient currents flow *in one direction;* and, further, that in some bodies this superinduced arrangement will not be permanent, unless in presence of the primary and constraining force. Hence the comparative ease with which one mass may be magnetized, while to impress the same effect on another is difficult: hence, also, the retention by the one of the magnetic state, and its rapid abandonment by the other.—But the theory of the whole is in its infancy.

Magnetism. The singular force or virtue indicated by the term *Magnetism*, appears to have been recognized in the very earliest epochs of observation; but, until recently, it was regarded only as a specific or limited influence, exercised by a very limited class of bodies upon each other. At present, the entire aspect of the subject has changed. It is already unquestionable, that the force so named is a cosmical force, to the influence of which no known description of matter is a stranger—that it operates, not in one simple manner only, but in fashions apparently the most diverse—and that it is united in closest relationship with other energies, or rather, is probably only a modification of certain other energies which pervade space, and determine the most extensive of those changes to which the external universe is subject. Owing to the alphabetical arrangement controlling the structure of this volume, many important portions of the

general theme are discussed under special headings. The ordinary phenomena of the relations of MAGNETS and of MAGNETIZATION are unfolded in the article immediately preceding, while those belonging to TERRESTRIAL MAGNETISM occupy the article which follows this one. The relations between the *Magnetic Force* and the agency of *Electric Currents*, are discussed in ELECTRO-DYNAMICS. It has been clearly established in that article, and the proposition may be sustained alike by mathematical and experimental proofs, that *the action of the magnet is identical with the action exercised by direct electric currents on bodies exterior to the circuit traversed by these currents: the two species of action, indeed, may in all cases be substituted, the one for the other, as they produce the same effects under the same circumstances.* For an account of the relations of Magnetism with Heat, the student must turn to THERMO-MAGNETISM : and the practical applications of great theoretical doctrines connected with this subject, are explained under TELEGRAPH, VARIATION OF COMPASS, &c., &c.—Excluding, then, the topics just mentioned, and supposing some of them familiar to the reader, it is our desire to supply now, an account of the grand and essential phenomena and of the laws of the Force of Magnetism itself.

I. THE GENERAL PHENOMENA OF ORDINARY MAGNETISM.—These phenomena, as manifested by the evolution of a *polar force* in a body susceptible of magnetization have been already explained, and may fairly be held to be familiar. As we have seen, a magnetized bar or needle is a needle or bar so modified or excited that at its opposite ends or poles two opposite and equal forces are manifested. The general methods of excitation have been explained in the preceding article; it therefore remains that we study the leading habitudes of an excited mass.

(1.) *The Distribution of the Magnetic Force within Magnetic Bars.*—This distribution may be considered in reference to the *length* of the bars, or as to their *interior* or *mass.*—I. If a magnetic bar, eight, ten, or twelve inches in length, be subjected to experiment, it is found that the weights it will support increase as we pass from either extremity towards points at a short distance inward, and that as we pass from the points of maximum intensity farther inward, the sustaining power diminishes very rapidly, and soon becomes inappreciable or *null.* It is manifestly of great importance, in a theoretical point of view, that this law of increase and decrease be discovered, and that the actual position of these points of maximum intensity—the true poles of the magnet—be determined. The inquiry early attracted the attention of Coulomb, who brought to the pursuit of it, at once his own fine sagacity and the experimental resources offered him by that exquisite *torsion balance*, to which we have so often referred. His method of experimenting consisted in determining the amount of torsion required to counteract the attractive force of every position (in reference to a needle) of the magnet, within its actual length, and somewhat beyond either extremity. But the preferable method of *oscillations* has, since then, been extensively employed. A needle, unaffected by the presence of an artificial magnet, unfolds by its *oscillations* the intensity of the magnetism of the earth: in presence of an artificial magnet the rapidity and number of its oscillations are due to two causes—viz., the intensity of terrestrial magnetism, and the proper force of the magnet. If the former has been previously determined and withdrawn, the residue is evidently the measure of the artificial magnetic force; and this measure is the most delicate of all. Coulomb's graphic representation of the magnetic intensity of bars has been reduced within an empirical general formula by Biot. His formula is this—

$$y = \text{A}\,(\mu^{x} - \mu^{2l-x}),$$

in which A and μ are two constants, x the distance from the southern extremity of the needle or bar to the point whose magnetic intensity is y, and $2\,l$ the length of the needle or bar. When the bar employed is of considerable length, the value of μ is nearly $\frac{1}{2}$, and in that case a formula of a most simple form may be taken as virtually true,—viz.,

$$y = \text{A}\,\mu^{x}.$$

The distance (x^{1}) of the centre of gravity of the curve of intensities from the nearest extremity of the bar, is given by the following equation—

$$x^{1} = \frac{2\,l\,\mu^{l} + \dfrac{(1-\mu^{2l})}{\log^{1}\mu}}{(1-\mu^{l})^{2}}$$

log^{1} designating hyperbolic logarithms. When the length of the bar is sufficiently great that μl and μ^{2} may be considered insensible, the formula becomes,

$$x^{1} = -\frac{l}{\log^{1}\mu}$$

The thinner the needle is, the more do the centres of force approach to its extremities. When l is very small the calculation for x^{1} becomes exceedingly simple: it turns out, indeed, on developing the complex function given above, that in the case specified we virtually have—

$$x = \frac{l}{3}$$

The position of the centre of force depends—in such circumstances—only on the *length:* its distance from each extremity being one-sixth of the whole length of the bar, $2\,l$. The *curve* of intensities might then be considered as a straight line; its area on each side of the bar as a triangle; and we know that the centre of gravity of a triangle is placed at one-third of its height

from the base.—These are at present our best *empirical* results, from the experiments made on this very curious and important subject. But the whole subject cannot be theoretically co-ordinated unless on the ground of some such mathematical or rather dynamical theory as that of which Professor William Thomson has already sketched the bold outline. From this physicist, whose wonderful fertility does not surpass his sagacity and power, science expects, and will assuredly obtain, the co-ordination and regeneration of this as of many other departments of inquiry.—There are various specialties on this subject—for instance, the phenomena of *consequent points*, with regard to which we must refer the student to such treatises as those of De la Rive and Becquerel.—II. As to the distribution of the magnetic force through the *interior* of magnetic masses, no absolutely satisfactory light has been yet offered by experiment. The distribution of the magnetic force through such interiors was attempted to be expiscated by the artifice of tying a great number of separate thin bars together,—having insured their close contact; and by examining their total magnetic force, as well as the magnetic force of each, previous to and after their union. But it does not appear that Coulomb's investigations sufficed to inform him of the exact law of internal magnetic distribution,—several of the magnets, of which his pile was composed, turning out, to have had their poles reversed. The whole subject has been recently taken up by Nobili, and elaborately pursued by the same method of bundles of magnets, to which he superadded experiments on hollow cylinders. He soon concluded that the magnetic force of a bundle or sheaf of bars increases in a much less ratio than that of the number of bars. His general result is this:—"the interior of a bar may be conceived to consist of concentric layers, whose magnetism decreases from without towards the interior." We regret that we can only refer as above to Professor Thomson's *Mathematical Theory of Magnetism.*—De Haldat has shown, that in the case of broad and exact magnetized plates there are a vast number of poles distributed through the bar or sheet, and acting in all directions.—It may just be mentioned here in reference to the magnetic force of bars of different shapes and weights, that Coulomb proposed the following formula,

$$T = (m\, L^{\frac{1}{2}}\, E + n\, l)$$

to express the time of oscillation of a bar whose breadth is L, whose thickness is E, and l half its length; m and n being constants depending on the nature of the steel or other material magnetized. Coulomb further concluded that the best shape to give a magnetic bar is this—it should be broad, thin, and formed like an arrow.

(2.) *The mutual action between any given portions of Magnetic Matter; and the External Relations of Magnetized Bodies.*—The important inquiries indicated by the above heading, would require a volume for their adequate discussion: the most general of the conclusions that have been reached, can alone be indicated here.—I. The law of the longitudinal distribution of the magnetic forces through bars, and its approximate concentration in two poles, enabling us to conceive of a uniformly magnetized needle, as a bar in which equal quantities of northern and southern magnetic matter are placed at these poles,—it becomes comparatively easy to detect by experiment the mutual affections of these opposite kinds of magnetic matter. Coulomb accordingly, by aid of his torsion balance, soon reached fundamental propositions as to this subject. They are these:—(1.) Like portions of magnetic matter repel, and unlike portions attract mutually. (2.) Any two *small portions* of magnetic matter exert a mutual force which *varies inversely as the square of their distance.* (3.) Or generally, if quantities of magnetic matter be measured in units, and if the positive and negative sign be prefixed to denote the *species* of matter, whether *northern* (+), or *southern* (—), then, *if quantities* m *and* m′ *of magnetic matter be concentrated at points at a distance* d *from one another, they will repel with a force which may be expressed by the algebraic equation*—

$$F \propto \frac{m\, m'}{d^2}$$

This law has been universally accepted, and may be termed the rudiment or foundation of the dynamics of magnetism. But chiefly through the researches of Professors Faraday and William Thomson, a totally new set of questions has recently arisen. Considering one magnetic pole as inseparably and essentially connected with an opposite pole, and considering further that in no actual case is the magnetic matter diffused from *mere points*, we must conceive of its diffusion through any space within which it really acts, as a much more complex affair than can be represented by Coulomb's simple and fundamental law. The true or actual problem is this:—Given a space within which a given magnetized body of any kind is placed,—in what manner, according to what laws, or along what curves, will that body diffuse its influence through that space? The entire space through which it diffuses its influence has been happily named by Faraday its *Magnetic field*; or, according to the precise definition of Professor Thomson, "Any space at any point of which there is a finite magnetic force, is called a field of magnetic force, or simply (*magnetic* being understood) a *field of force.*" What, then, is the nature of the force at all points in any magnetic field? Not only what is its *intensity* (which, with certain reservations, we might deduce from the general law of Coulomb), but what is its *direction?* The system of lines or curves surrounding a magnetic pole,

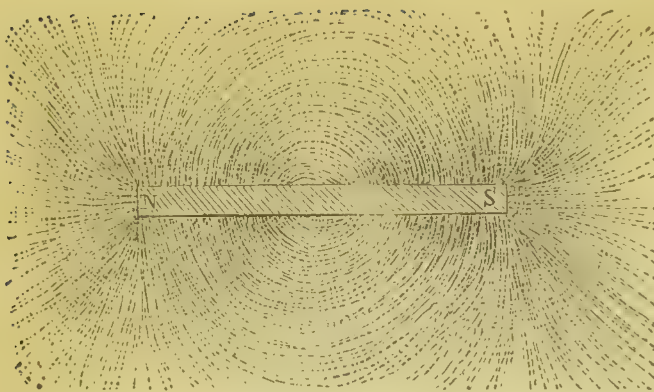

Fig. 1.

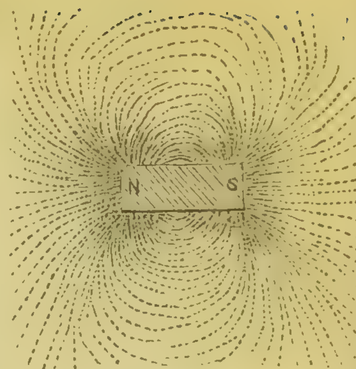

Fig. 2.

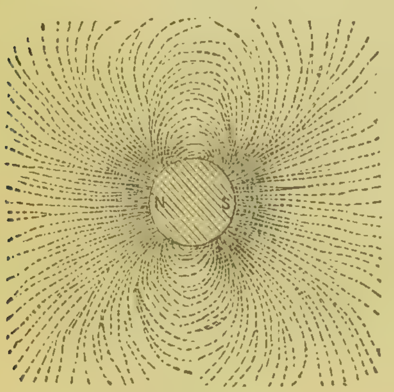

Fig. 3.

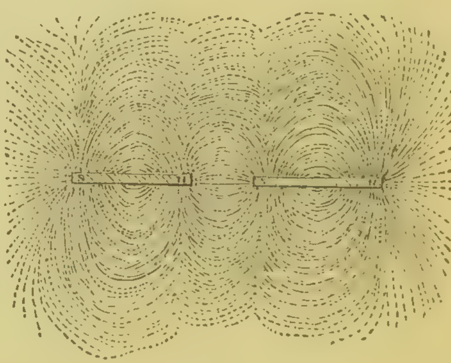

Fig. 4.

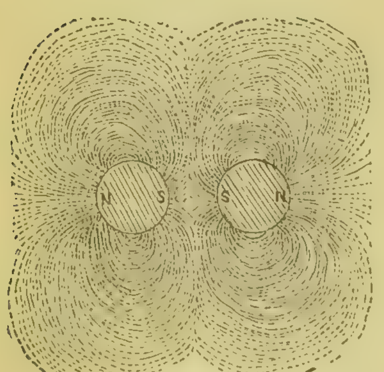

Fig. 5.

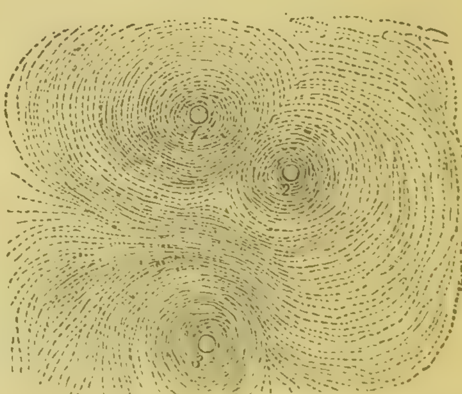

Fig. 6.

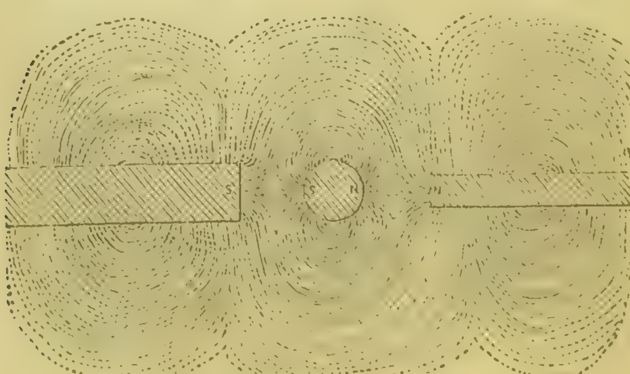

Fig. 7.

indicating that direction, is called the system of "Lines of Magnetic Force." A line of force is thus a line drawn through a magnetic field in the direction of the force at each point through which it passes, or a line touched at each point of itself by the direction of the magnetic force;—in the same way an "uniform field of magnetic force" is a space through which the lines of force are parallel straight lines, and the intensity of the force is uniform.—Can we, then, form a distinct conception of the magnetic field and the curves or lines of force that fill it in any individual instance? On the possibility, or rather the realization of such a conception, the promise of any effort to comprehend the behaviour of any body within the magnetic field evidently depends. These *lines of force* have long been referred to and recognized under the name Magnetic Curves. Their general distribution is roughly indicated by the arrangement of iron filings, or any light substance affected by the magnetic force, around any system of magnetic poles. For instance, the diagrams on the preceding page—which speak to the eye—explain fully what these lines are. These, of course, are but a few of the positions in which centres of magnetic force may exist in relation to each other; but they suffice to manifest the nature of what is termed a magnetic field, and the variety of the lines of force that, even in the simplest case, exist within it. Irrespective wholly of *physical* speculations as to the nature and correlations of the magnetic energy, it is clear that the exact determination and calculation alike as to the curvature or direction of such lines, and of the intensity prevailing in every portion of the field, constitutes, in connection with experimental truths, ample basis for a *mathematical* theory of magnetic phenomena. It is not unknown that the immortal Newton, scarcely satisfied with his grasp of that only law which we know to be universal—viz., that law of the propagation of the Force known as Gravitation—indulged in curious speculations as to its physical origin and dependencies, which have never led, and were incapable of leading, to any distinct conclusions. It is not often, indeed, that a pure dynamical subject is in hazard of falling back amidst physical hypotheses; the difficulty is, for Inquiry to escape from these. It may be permitted us to experience a degree of rare pleasure, that, chiefly through the discoveries, the sagacity and philosophic views of Faraday and Thomson, Magnetism has now risen above such rudiments, has become cleared of hypotheses concerning "Fluids" and "Ethers," and asserted its position as a science of *pure Force.*—We shall refer again to this subject.—II. But the conditions of magnetic problems are still more complex, inasmuch as a new and very obscure, although essential element speedily and imperatively asserts its claims. We have hitherto referred only to the mutual actions of *magnetic matter*, or what is the same thing, of permanently magnetized masses. But there are relations between a permanently magnetized mass and others which cannot assume a permanent magnetism. What are the laws, for instance, in obedience to which a permanent magnet affects a sphere of soft iron? This is not the mere question as to the rate of the diminution of the magnetic force according to distance from its pole; for the presence of the magnet seriously affects the mass of soft iron, and causes it to assume *reciprocal actions and relations* which are largely causative of the total result. Unfortunately, unless in comparatively a limited range of cases, it is not at all known what the specific change is which is thus impressed; and it seems not improbable that our comparative ignorance as to this essential element is a main source of discrepant opinions regarding certain spheres of magnetic action. "We know not," says Faraday, "whether such bodies as oxygen, copper, water, bismuth, &c., owe their respective paramagnetic and diamagnetic relation to a greater or less facility of conduction in regard to the lines of magnetic force, or to something like a polarity of their particles or masses, or to some yet unsuspected state." These words of our illustrious physicist perhaps underrate the amount of our actual knowledge; but no one can withhold assent from his subsequent assertion, that many "circumstances show that we have yet a great deal to learn about the physical nature of the magnetic force, and that we must not shut our eyes to the first feeble glimpses because they are inconsistent with our presumed laws of action, but rather seize them as hoping that they will give us the key to the truth of nature." "Bodies," Faraday adds, "when subject to the power of the magnet, appear to acquire a new physical state, which varies with the distance or the power of the magnet. Each body may have its own rate of increase and decrease, and that may be such as to connect extreme effects on the one hand with extreme effects on the other; and when we understand all this rightly, we may see apparent contradictions become harmony." We shall refer again, and more in detail, to this intricate subject, but in the meantime a few ascertained facts may be recorded. The following general results deduced by Professor Tyndall—one of our ablest inquirers—are worthy of all reliance. 1. The mutual attraction of a magnet and a sphere of soft iron, when both are in contact, is directly proportional to the strength of the magnet. 2. The mutual attraction of a magnet and a sphere of soft iron, when both are separated by a small fixed distance, is directly proportional to the square of the strength of the magnet. 3. The mutual attraction of a magnet of constant strength and a sphere of soft iron is inversely proportional to the distance between the magnet and the sphere. 4. When the distance between the magnet and the sphere varies, and a constant

force opposed to the *pull* of the magnet is applied to the latter,—to hold this force in equilibrium the strength of the magnet must vary as the square root of the distance.

II. PHENOMENA INDICATING THE ACTION OF MAGNETISM ON ALL BODIES.—It could never have been accounted as other than most singular that a force of the nature and energy of the Magnetic, should be limited in its manifestations within the narrow sphere of the relations of a very few bodies — iron, steel, nickel, and cobalt. Slight indications of the cosmical or universal nature of this truth had been remarked by Coulomb, Brugmann, Lebaillif, and Becquerel; but science unquestionably owes to our own Faraday the discovery that the isolated facts which had attracted the notice of earlier Inquirers are indications of general laws, and therefore all capable of being brought under one principle. The phenomena that arrested Faraday have been already noticed under DIAMAGNETISM, and are in their simplest form as follows:—Suppose that a prism of heavy glass is suspended horizontally by means of a waxed silk thread above and very near the two poles of a powerful electro-magnet, this prism on the moment of the completion of the galvanic circuit begins to oscillate, and finally places itself at rest in a line perpendicular to the line joining the two poles, or right athwart the position that would, under the same circumstances, be assumed by a bar or needle of any metal formerly accounted magnetizable. The latter takes on what Faraday terms an *axial* position—the prism of glass an *equatorial* one. But this property is not confined to glass: it is, on the contrary, possessed in different degrees by all substances—organic and inorganic, which were not held magnetic, or that *contain no portion of magnetic elements.* Likewise, if the prism of glass is suspended so that its centre is nearer one pole of the magnet than to the other pole, it still assumes the equatorial position, but at the same time it is repelled—*en masse*—parallelly to itself, by the pole nearest to it. In performing original experiments on this subject, or in repeating Faraday's, every care must be taken that the substances experimented on, be *pure*—at least that they do not contain, or have attached to them, any ordinary magnetic element:—for instance, it is sufficient to have cut a piece of wood with a *knife* to induce it to place itself *axially*, although wood, free of previous affection from iron, has a strong directive power *equatorially.* Exactly the same kind of affections are found to belong to liquids and gases. These remarkable results induced Faraday to question whether we had previously exhausted the list of bodies capable of manifesting the ordinary magnetic action, in opposition to which he named this new mode of action—*diamagnetic*, or cross magnetic. He extended the old list considerably; and according to his, and subsequent researches, a list may now be made out, appearing to show that the magnetic or paramagnetic sensibility passes gradually, from a considerable force, apparently to *zero:* after which zero, it shows itself slightly *negative* or diamagnetic, and then rises to a considerable amount of diamagnetic energy. The following is Faraday's first arrangement of substances:—

Iron.	Alcohol.
Nickel.	Gold.
Cobalt.	Copper.
Manganese.	Silver.
Chromium.	Lead.
Cerium.	Water.
Titanium.	Mercury.
Paladium.	Sodium.
Crown glass.	Flint glass.
Platinum.	Cadmium.
Osmium	Tin.
Air and Vacuum 0° or zero.	Zinc.
	Heavy glass
	Antimony.
Air and Vacuum.	Phosphorus.
Arsenic.	Bismuth.
Ether.	

Becquerel and Plücker have both engaged in this species of research, and have sought to determine actual *numbers*, representing the *specific* magnetisms—paramagnetic or diamagnetic—of a large number of bodies. In so far as the term *specific* involves a theory, we disclaim such use of it; but the positive researches of Becquerel are independent of all theory. The following table of Plücker, which, with the preliminary observations, we copy from the classical work of De la Rive, represents at present the condition of this inquiry:—"Laying down as a principle that the proper magnetism or diamagnetism of each substance is proportional to its mass,—a principle that M. Plücker endeavoured to verify directly by mixing, in greater or less quantity, fine iron filings with wax, so as that the total volume was always the same,—the magnetism or diamagnetism of the bodies was obtained by dividing by their *weights*, the force, also expressed in weight, with which an equal volume of each of them is attracted or repelled. We thus obtain the element sought, for *equal weights.* Solid substances in these experiments are reduced into as impalpable a powder as possible.—It was found by this method that, expressing the intensity of the magnetism of iron by 100,000, this intensity for loadstone is 40,227, for micaceous iron ore 533, and for the brown peroxide 71. Of all the solid or liquid compounds into which iron enters, this latter is the one that has given the most fertile result. The following, however, is the detailed table of the results, upon which we shall confine ourselves to remarking, that the combination of acids with oxides, in order to form salts, does not enfeeble the original magnetism of the latter,—that the water of hydration sometimes adds force to the magnetism, as is the case with the hydrate of protoxide of nickel, which is three times more magnetic than the protoxide itself,—that finally, all the compounds of manganese, which were submitted to experiment, were found to be magnetic

Iron	100,000
Loadstone	40,227
Oxide of iron, No. 1	500
do. do. No. 2	286
Red ochre	134
Micaceous iron ore	533
Hydrated peroxide of iron	156
Brown peroxide of iron	71
Artificial hæmatite	151
Dry sulphate of oxide of iron	111
Green vitriol	78
Saturated solution of nitrate of oxide of iron	34
do. do. hydrochlorate	98
do. do. sulphate of iron	58
do. hydrochlorate of potass	84
Green vitriol in solution	126
Sulphate of protoxide dissolved in vitriol	142
Nitrate of oxide in solution	95
Hydrochlorate of oxide of iron	224
Sulphate of oxide of iron	133
Hydrochlorate of protoxide of iron	190
Sulphate of protoxide of iron	219
Dentochloride of iron in solution	254
Protochloride	216
Iron pyrites	150
Protoxide of iron in hydrochloric solution	381
do. do. sulphuric solution	462
Peroxide of iron in the hydrate	168
Peroxide of iron in hæmatite	168
do. do. nitric solution	287
do. do. hydrochloric solution	516
do. do. sulphuric solution	332
Iron in the loadstone	55,552
do. do. oxide, No. 1	714
do. do. do. No. 2	409
do. do. red ochre	191
do. do. micaceous iron ore	761
do. do. hydrated oxide	296
do. do. hæmatite	240
do. do. pyrites	321
do. do. sulphate of oxide	549
do. do. green vitriol	385
do. do. solution of nitrate of oxide	410
do. do. do. hydrochlorate	737
do. do. do. sulphate	474
do. do. hydrochlorate of protoxide	290
do. do. sulphate of protoxide	594
Protoxide of nickel	35
Hydrate of protoxide of nickel	106
Nitrate of protoxide of nickel in solution	65
Sulphate	100
Chloride of nickel in the preceding solution	111
Protoxide of nickel in hydrate	142
do. do. nitric solution	164
do. do. hydrochloric	171
Nickel in oxidule	45
do. hydrate of protoxide	180
do. nitric solution	208
do. hydrochloric solution	217
Hydrate of manganic oxide	70
Manganous oxide	167
Manganic oxide in hydrate	78
Manganous in hydrate of oxide	112
do. do. oxidule	322

Theories regarding the relations between the ...amagnetic and diamagnetic forces we shall ...fly discuss below. But two facts much founded ...y the upholders of the various views taken of ...e relations, must be mentioned here. In the ... place, the phenomenon noticed by Poggendorf ... Reich. The former philosopher found that if ...extremity of an extremely feebly magnetized ...lle be brought near a bar of bismuth or other ...ng diamagnetic in its equatorial position, this ...emity attracts the bar on the same side that ...ould have repelled it had it been of iron:— ... the electro-magnet had determined in dia...netic substances *poles of the same name as ... which act upon them.* Reich, again, noticed that when the *opposite* poles of two magnets act simultaneously upon the same face of a bar of bismuth the repulsion engendered is equal to the *difference*, not to the *sum* of the forces of each pole acting alone. *Secondly*, the remarkable class of facts discovered by Weber. On placing a bar of diamagnetic metal, instead of one of soft iron, within a bobbin or coil, Weber thought he had found proof that this metal, under the influence of a strong electro-magnet, acquires poles of a contrary nature to those acquired by soft iron under the same circumstances. Weber hence concluded that the diamagnetic had really acquired a *polarity*, directly the *opposite* to that manifested by the soft iron. But after long and careful study, Faraday came to the conclusion, that the effects in question are not due to diamagnetism, but to the greater or less degree of conductibility of the metals when, within the bobbin, inductive currents are established.—It would be very wrong to conclude this notice of phenomena, without especially referring the student to the painstaking, manifold, and most satisfactory researches of Professor Tyndall. They are recorded in the *Transactions of the Royal Society*, and in the *Philosophical Magazine.*

III. Magnetic Affections of Crystals, or the Magneto-Crystalline Force.—The foregoing exposition of facts indicates two important truths:—*first*, that the magnetic energy influences all bodies and modifications of matter; and *secondly*, that its action or efficiency varies with the constitution of the body. It remained to be seen whether those bodies whose internal constitution varies *along certain axes*, manifest different relations to the magnetic force, according to the position of these axes in reference to the magnetic poles or *origins* of that force. We speak of course, of *crystals*. The problem as now stated appears to have first occurred to Plücker, and his investigations have been followed up by many able men—especially Faraday, Tyndall, and Knoblauch. The *optic axes* of crystals, depending on the internal arrangement of the particles, Plücker first inquired whether the position of these axes in the crystalline substance subjected to the magnetic force, had any influence on the position assumed by the crystal, whether paramagnetic or diamagnetic *in itself*. And he arrived at the following remarkable conclusion:—eliminating the mere paramagnetic or diamagnetic effects due to the *substance* of the crystal, the optical axis of uniaxial crystals has a tendency to assume an equatorial position, or a position athwart the lines joining the two poles. He considered it easy to eliminate the ordinary paramagnetic or diamagnetic agency from such experiments, because the force producing the repulsion of the optical axis seemed to diminish *in a proportion less rapid in reference to distance* than the force acting on the mass of the substance. It appeared simply necessary therefore—to test this special force—that the poles or

sources of magnetic energy be withdrawn to a *greater distance*. In the case of crystals with *two magnetic axes*, the two axes were equally affected by both magnetic poles, so that their *mean line* took on the equatorial position. In so far as Plücker's original researches went, their results or laws appeared simple enough; but fresh investigations by Faraday, confirmed subsequently by M. Plücker, manifest a characteristic difference between different crystalline substances. In the course of these researches, Faraday found it right to institute two definitions, viz., the *Magneto-Crystalline Force* is the peculiar modification of the magnetic force manifested during its action on crystals; the *magne-crystalline line* is on the contrary the line according to which the *directive force* is exercised. The earliest results of Faraday's inquiries appeared wholly to contradict those of Plücker, inasmuch as the optical axis was found in very many cases to take on an *axial* and not an *equatorial* direction, and to follow the magnetic power as if it were a *paramagnet*. But the resolution of the difficulty was speedily effected by the discovery of a higher law;—viz., in the case of a *positive* crystal there exists between the optic axis and the poles of the magnet, an attractive, or in so far, an *ordinary* magnetic affection; in crystals, where the optic axis is *negative*— those on which Plücker at first experimented—his law holds, viz.:—There is repulsion, and the optical axis places itself *equatorially*. In the same way, in regard to crystals of two axes, the mean line is attracted or repelled according as the crystals themselves are *positive* or *negative*. It is not difficult to conceive the interest created by these identifications of the magne-crystalline lines with the lines of elasticity in the crystal. But anomalies, or apparent anomalies presented themselves, for the experimental clearing up of which we are indebted mainly to the investigations of Tyndall and Knoblauch. These energetic and most reliable Inquirers have laid down as a general result of varied and judicious experiments, the Laws that when the molecular constitution of a body is such that the particles of which it is formed are in greater proximity in one direction than they are in other directions, that direction is the one in which the magnetic or any other force exerts itself with the greatest energy; so that the line representing this direction is the one which places itself *axially* or *equatorially*, according as the body is *paramagnetic* or *diamagnetic*. There are still multitudes of apparent exceptions to this law, but when analyzed they are found to originate in special circumstances, or in the counteraction of separate and distinguishable forces. "In this mode of explaini g these phenomena," says De la Rive, "the action of the magnet is always exercised *upon the particles;* and it is according to their magnetic and attractive, or their diamagnetic and repulsive nature. The only difference between crystals and other bodies is, that by the fact of their non-homogeneous structure, crystals present certain directions, according to which the action, whether magnetic or diamagnetic, is more energetic than it is along other directions, on account of the greater approximation of the particles that occurs in these same directions; a phenomenon altogether analogous to that of dilatation by heat, which, in a crystal of calcareous spar, for example, operates more powerfully, according to Mitscherlich, in the direction of the optical axis, because the particles being more closely packed along this direction than along the others, repel each other with more energy, for the same elevation of temperature." If the special properties evinced by magneto-crystalline action, be thus represented, it is clear that they form no exception to the general laws of magnetism, but originate in a special grouping of the particles of the crystals. And this view has since been confirmed in a remarkable manner by Professor Tyndall himself, and also by Matteucci. Professor Tyndall has succeeded in obtaining from solids, to which he had applied strong pressure, a magnetic direction depending on the pressure, precisely similar to that exhibited by the crystalline condition; and his results have largely been confirmed by Matteucci. "This inquirer," to use the words of De la Rive, "obtained the same effects from compression both upon sulphur and stearic acid, as upon bismuth. The pieces subjected to compression were placed between two cheeks in a vice, so that all the points of their two surfaces were equally compressed; they were then washed in hydrochloric acid, to free them of impurity; the form of cubes had been given to them, and these cubes being suspended so that the line of compression was horizontal, constantly took a direction between the poles of the electro-magnet, so that this line was perpendicular to the polar line. On taking a cube of 0 39 inches, and cutting from the side that had been strongly compressed prismatic needles in different directions, they directed themselves *equatorially* or *axially*, according as the line of compression was parallel or perpendicular to their axis, which depended on the manner in which they have been cut. M. Matteucci succeeded in imitating all the effects of crystallized bismuth, by producing cubes and needles by means of small very thin plates of bismuth, obtained by dropping liquid bismuth in small drops from a certain height upon a marble plane; one more proof that the cleavage planes act as separated plates." In the course of his experiments, Matteucci conceived that he had discovered facts inconsistent with the conclusions of Tyndall and Knoblauch. We believe he has renounced this opinion, and recognized that the irregularities are mainly attributable to the important part played by *induction* in every phenomenon in which magnetic action takes a part. It is not improbable, as De la Rive remarks, that in many cases a molecular induction is broug

about in the particles, giving rise to a molecular current when the particle is isolated, and to a finite induced current when several particles are agglomerated so as to form a continuous mass. —It is impossible to conclude this portion of our article without adverting to the correlations of the manifestations of the magneto-crystalline force now described, with many others that have been noticed, and become a recognized foundation of physical theories. It is not necessary to revert to the connection of the optic axes of crystals, so firmly established by Sir David Brewster, (one indeed of the greatest and most fertile discoveries of our time.) with the molecular or physical axes. This connection stretches much farther. For instance, Savart, during his extensive and most interesting studies regarding vibrating plates determined a distinct relation between the acoustic figures produced by the vibrations of such plates and the crystallization of the substances of which these plates were formed. The direction of the optical axis is invariably connected with the axis of the forms of such acoustic figures. So minute is the correspondence, that by these acoustic forms it may be at once determined whether a crystal is positive or negative, or which of the axes is the line of the greatest and which of the least elasticity. On this discovery by Savart followed those of Mitscherlich, regarding the uneven expansion of crystals by Heat—an inequality also depending on the position of the optical axes; and Senarmont still more recently observed (see Acoustics), that conductibility for heat, in all systems of crystals in which the axes are not equal, has a maximum and minimum value, according to directions parallel to the crystallographic axes. There is a most close analogy indeed, between calorific aud luminous propagation within such media; although there are such discrepancies as one would expect from the different rates of propagation of the different rays or portions of the spectrum. The question has been still more recently taken up by M. Wiedmann in reference to electro-conductibility. His general conclusion is, "that crystals which possess a better conductibility in the direction of the principal axis, all belong to the class of negative crystals; whilst those that have a better conductibility in the direction perpendicular to the axis are *positive;* which indicates that the best conductibility for elasticity is also that along which light is propagated with the greatest velocity. Is it not clear, then, that the molecular axes of crystals is truly the root of all phenomena belonging to their relations to Light, Heat, Electricity, and Magnetism?

IV. Action of Magnetism on Flames.—This action is very striking; but theoretically it is merely the manifestations of magnetic action on *heated gases.* The phenomena are exceedingly curious in their manifestations, and methods are suggested by them for determining whether a liquid is diamagnetic or otherwise. We must refer for details to the classical and almost exhaustive work by De la Rive.

V. Effect of Magnetism in Developing or Changing the Optical Properties of Transparent Bodies.—The class of facts now to be described, although clearly mixed up with the action of the magnetic force on the internal or molecular constitution of bodies, is yet so distinct from anything hitherto adverted to, that we must contemplate it apart. The general features of these facts have been already noticed under Circular Magnetic Polarization (*q. v.*), nevertheless we shall again rapidly describe them. The phenomena of *Polarization* need no further notice here; neither what is termed the *plane of polarization.* Suppose a ray of light polarized by one Nicol's prism or by a tourmaline, and viewed through another prism or tourmaline suitably placed, we know that the light of the ray (if that ray be homogeneous) is wholly extinguished. If between the polarizer, and the analyzer thus placed, a prism, say of heavy glass, is laid, so that the ray pass through the glass, no change is observable; but should the opposite poles of a strong magnet be also placed at the ends of that glass prism, the analyzing plate or prism immediately shows a degree of light, and it must be turned round or *rotated* by a certain angle until the ray is again extinguished. It follows from this fact, that the *plane* of polarization has been rotated or turned through a certain angle (measured by the rotation of the analyzer) by the effect of magnetism on the glass prism. The phenomenon is of precisely the same nature as that of ordinary Rotatory Polarization, described under Polarization; so that the magnetic influence has sufficed to bestow on the glass prism that peculiar power to rotate or change the plane of polarization, which is inherent in the atomic structure of certain bodies. This memorable influence was first noticed by Faraday: indeed its discovery was the prelude of his researches in Diamagnetism. It has since occupied much attention on the part of this most eminent physicist, and of MM. Bertin, Verdet, &c. It may just be added as a preliminary remark, that Wartmann has shown that polarized calorific rays are acted on in a way altogether similar. For instance, a piece of rock salt, in the way of polarized rays of heat, determines a rotation in the plane of its polarization, if a powerful electro-magnet is made to act upon it. This result of Wartmann has been confirmed by Provostsaye and Desains, who, by aid of very delicate processes and apparatus, have succeeded in the extremely difficult task of measuring the deviation produced in that plane. An apparatus needful to determine this element in case of the rays of light is much more easily obtained; its best and most convenient form is that prepared by M. Rhumkorff.—Without referring to the possible ultimate causes of this strange set of

results—for the scrutinizing of which the time has not yet come—let us state as briefly as we can, such general laws regarding them as, with any clearness, have been ascertained. (1.) And in the first place, Faraday soon established a noticeable difference between the action of magnetized transparent bodies in rotating the plane of polarization, and those bodies, such as quartz and liquid solutions, which produce, of themselves, a similar effect. In the latter set of cases the amount or angle of rotation increases with the thickness of the substance through which the ray passes: in the former, that thickness has not *of itself* any influence whatever—the deviation being the same whether the prism of glass be long or short, in so far as that element is concerned. This latter indeed has one influence, but one owing wholly to the fact that the longer the prism the farther apart must be the poles of the influencing magnet; whence a large modification of the purely magnetic effect.—(2.) But if the mass or length of the transparent substance has no separate effect, it is otherwise with the *nature of the substance.* Different transparent substances give very different deviations or rotations. At first, as apparently ascertained by certain experiments of M. Bertin, it was supposed that the rotation varies directly with the *refractive index.* But M. Verdet has recently shown that there is no relation whatever between these two quantities. For instance, notice the following table:—

Substances.	Refractive Index.	Angle of Rotation.
Distilled water, ...	1·334	4° 00′
Solution of muriate of ammonia,	1·359	4 45
Protochloride of tin, ...	1·364	5 27
Carbonate of potash, ...	1·371	4 21
Chloride of calcium,............	1·372	4 55
Chloride of zinc, ...	1·394	5 57
Nitrate of ammonia, ...	1·448	3 44

Other substances yield the same results, completely negativing any dependence of the one element or *constant* on the other. Bertin further showed that such substances as nitrate of ammonia and protosulphate of iron dissolved in water, diminish the rotatory power of the liquid; and upon the ground of this and other facts of like nature, Becquerel concluded that it may be said in a general way, that the rotation of the plane of polarization due to the influence of magnetism varies *in an opposite ratio to the magnetic power of the bodies.* M. Verdet has renewed the inquiry in a recent elaborate memoir (*Annales de Chimie et de Physique,* third series, vol. xxiii.), and appears to have traced the source of the peculiar mode of action of all ferruginous compounds. If proto-salts and per-salts of iron are dissolved in water, the rotatory power of the solution is always less than that of water: in fact things take place as if the dissolved iron salt *has a rotatory power in the opposite direction to that of water.* The same result holds in the case of ethereal and alcoholic solutions of alkaline or metallic salts; so that it may be concluded generally, that the salts of iron submitted to the action of magnetism exert an action *opposite* to that of the generality of transparent substances upon polarized light. Nevertheless, Becquerel's law does not hold. On the contrary, salts of nickel and manganese seem to possess a *direct* rotatory power, inasmuch as their solutions have a larger rotatory power than that of their transparent solvent. It must therefore be said that, on this very important feature of these phenomena, no law or general truth is at present established.—(3.) *Thirdly,* in what manner, or according to what laws, do these rotations depend on the *distance* and *on the force of the magnetic centres,* acting on the transparent bodies? This inquiry has also been well elaborated by M. Verdet, in a memoir reproduced in vol. ix., fourth series, of *Philosophical Magazine.* Verdet begins by detailing the experimental contrivances by which he insured that the transparent mass on which he operated should be placed as nearly as possible within what Faraday has called a *magnetic field of equal intensity;* in other words that it should not at different times, or at different parts of it, be exposed to the action of magnetic forces acting in different directions or along different curves. The vital importance of such an arrangement in the conduct of all such inquiries will appear more fully in the next general section of this article: for the devices by which M. Verdet assured the essential effect in his own researches, we must refer to the memoirs already quoted. Further, by ingenious treatment he freed himself from the ambiguity arising from the use of pure solar light, as well as from that comparative darkness and the doubtfulness belonging to the measurement of the differences of small angles which is interwoven with the employment of the least refrangible rays. The substances experimented on were only *three,* viz., Faraday's *heavy glass, common flint,* and *bisulphide of carbon;* but as Verdet remarks, these three substances differ so much from one another, that a law which suits them equally, may be regarded as general. The rotatory power of these different substances is of course different, but in all cases the law holds, that *the rotation of the plane of polarization is proportional to the magnetic action.* This proportionality holds with the same exactitude whether the distance of the magnetic centres from the transparent substances is changed, or whether the quantity of free magnetism accumulated in the various centres undergoes a variation. The formula indeed may be expressed universally, and for all circumstances, in the following terms:—" *The rotatory power developed by the action of a magnetic centre in an infinitely thin plate of a non-refracting substance varies proportionately to the magnetic action,* i. e., *directly as the quantity of magnetism accumulated in this centre, and inversely as the square of the distance.*"—(4.) A

other point of corresponding importance has also been examined by Verdet. In publishing his discovery of the rotation of the plane of polarization, Faraday stated that the phenomenon manifested itself with the greatest intensity when the direction of the ray of light was parallel to the direction of the magnetic forces, and that it disappeared when these two directions were perpendicular to each other. But it remained to explain the law of the dependence of that rotation on *intermediate inclinations* of these two directions,—a law, the knowledge of which is clearly essential towards the formation or the verification of any just general theory. It is indeed needless to insist on the inherent interest of such a research. Verdet has sought to determine in the most careful manner what takes place where the angle formed by the direction of the rays of light with the magnetic direction varies from 0° to 90°. Neither in this case can we do justice to his methods:—the reader is again referred to his own memoir. But the important and apparently established result is embodied in the following theorem:—*The rotation of the plane of polarization is proportional to the cosine of the angle enclosed between the direction of the luminous rays and that of magnetic action.* Verdet adds the following pregnant remark:—"If we adopt the theoretical views of Fresnel, with respect to the rotation of the plane of polarization, we must imagine the polarized ray falling perpendicularly upon the transparent substance which is submitted to the influence of magnetism, transferred into two rays circularly polarized, and to opposite directions, and propagated with unequal velocities. If the velocities of propagation be represented by v and v', it follows from the law above enumerated that the expression

$$\frac{1}{v} - \frac{1}{v'}$$

varies proportionally to the cosine of the angle enclosed between the direction of the luminous rays and that of magnetic action."—It is clear at least that the subject now treated, is one more and very momentous contribution towards the practicability of the inquiry as to the correlation of Physical Phenomena and Molecular Forces.

VI. THEORIES AS TO THE RELATIONS BETWEEN THESE VARIOUS MODES OF MAGNETIC ACTION.—The subject on which we now offer a few remarks is one of the most delicate in modern physics. The forces in play, however real, are unfortunately, for the most part, so slight—in many cases nearly evanescent—that satisfactory experiments regarding disputed points become arduous, uncertain, and frequently impracticable. Hence the extreme difficulty of determining between conflicting views by anything like an *experimentum crucis*.—The general facts as explained in the previous part of this article, and others preceding it, may be summed up as follows:—(1.) The action of the magnet on magnetic bodies usually so called, accompanied by a directive *axial* force, and a decided development of magnetic polarity.—(2.) Action upon diamagnetic bodies, indicated by a directive force producing the athwart or *equatorial* position.—(3.) Action upon transparent bodies, solid and liquid, endowing them with the power of rotating the plane of polarization.—(4.) Action upon all good conducting bodies,—developing in them instantaneous currents, called currents of *induction*.—(5.) Magneto-crystalline action.—Now, between these various modes of action, there are certain apparent (*external*) relations, which suffice to indicate that some central or all-comprehending law most probably exists. For instance, diamagnetic bodies and magnetic bodies may almost be classed, on *à priori* grounds, and separated from each other, so soon as we know their atomic weights, and their power to conduct electricity. According to De la Rive, diamagnetic bodies are those which, volume for volume, contain the smallest number of atoms, or which, if they contain more, are very good conductors of electricity. Atomic weights along different axes, appears likewise to be the main influence in determining the *magne-crystalline* lines. And between the two classes of phenomena in the foregoing list which seem the most diverse—viz., the diamagnetism of bodies, and their rotatory power when under magnetism, there are likewise striking relations, thus enumerated also by De la Rive. *First*, the position assumed by the diamagnetic body so that it escape the action of the magnet, is *that* in which the body ceases to possess rotatory magnetic power,—supposing that the polarized ray continues to traverse it in the direction of its length. *Secondly*, all the circumstances that increase the rotating magnetic power of a body also increase its diamagnetism, for instance;—(1.) The rotation of the plane of polarization is proportional to the intensity of the current, or of the magnet acting on the substance submitted to it; now the diamagnetic force of the substance is also proportional to that intensity.—(2.) Crystals have a very feeble rotating power; and their diamagnetism is also feeble; sometimes they are directed *axially*, sometimes *equatorially*.—(3.) Substances, which by their nature show the largest amount of diamagnetism, are also those which exercise the most powerful rotatory action upon the plane of polarization; and, reciprocally, the more a substance is *magnetic*, the less decided is this action. On classifying a large number of bodies according to their power as manifested in these two ways, it will be seen that the *order* of their diamagnetic power is the same as the order of their rotating power. The numbers which express the energy of these influences are not proportional, but, as De la Rive remarks, "This is not to be wondered at, inasmuch as considering the very different *form* of the phenomena manifested by the same

substance under influence of the magnet, we could not expect the two results to be the *same function* of the forces in play."—These relations, although emphatic in so far, we have yet seen it proper to term *external;* they connect the *forms* of the different phenomena, but do not indicate whence their relationship springs. The prominent question in this ulterior inquiry is, evidently the question, what is the relation between *Magnetism* commonly so called, and *Diamagnetism?* Are they the *same* influence manifested under different circumstances, or *different* influences? Two theories have been advanced favouring these opposite views; and there is besides a purely dynamical theory, all of which we shall endeavour briefly to expose. But as we desire to explain as fully as we can the dynamical theory, our reference to the two former must be summary.

(1.) *Is Diamagnetism an antagonistic Magnetism?*—The answer in the affirmative amounts to this:—"There are two magnetic influences or forces, or magnetic polarities, not only distinct but antagonistic; and, our greater knowledge of the one, and consequent leaning to consider its manifestation as the chief source of information regarding the magnetic force, depend solely upon its superior energy, and the fact that it therefore pressed itself more forcibly on attention. Had we known only the delicate phenomena manifested by *bismuth* and its cognates, we should have framed a theory of Magnetism having as its basis different and antagonistic polarities." This view was recommended to some experimentalists by the circumstance that they supposed they had detected in *diamagnets, poles of the same name as the magnetic poles nearest them.* The earliest experiments of Poggendorf, Reich, and Weber, which seemed to point in this direction, received another interpretation—their effects, as already remarked, being essentially due to the production of inductive currents over the surface of the metals subjected to the influence of electro-magnets, or merely to that of closed currents. The subject has been resumed by Weber, and a distinguished Inquirer in our own country. The experiments of Professor Tyndall are above question as to their ingenuity, or the mode in which he has conducted them; neither are their positive results to be doubted for a moment. The question remains, however, are these results not explicable without the interposition of a *new physical force*—of a really *new polarity*—of an *antagonistic Magnetism?* Critical discussion in this place is wholly beyond our power. But we may be allowed to suggest two considerations, which in the present undetermined condition of this most delicate and difficult inquiry, appear worthy of attention.—Setting aside the objections of Matteucci, it does appear remarkable, and certainly is deserving of weight, that—except in one moment perhaps of apparent wavering—our admirable Faraday at the close of the finest series of inductive researches which this quarter of a century has produced, does not feel it necessary to introduce the hypothesis of any new force: and secondly, that philosophy has seldom benefited by the sometimes rapid acceptance of new causes. Its successful career is rather marked by the simplification and assimilation of theories as to causes. And assuredly a critical review of the recent history of a cognate physical subject does not dispose one to receive, unless on sheerest compulsion, the conception of novel *polarities.*—The question really is, can the effects be explained without the introduction of any new polarity?

(2.) *Is there a Specific Magnetism in the sam[e] sense as there is a Specific Heat in relation t[o] Bodies?*—The affirmation is advanced by M. M. Becquerel—than whom there are few more deserving writers or Inquirers. It is maintained that all bodies are magnetized by magnetic centres, precisely as soft iron is, only in a greater o[r] less degree—the direction they assume dependin[g] on the difference existing between the action tha[t] is exercised upon them and the action exercised upon the medium by which they are surrounde[d]. Just as in the case of heavy bodies of differen[t] specific gravities, a substance under this vie[w] would be attracted by a magnetic centre wit[h] the *difference* of the action exercised upon *it* an[d] the *medium* by which it is surrounded: it woul[d] be attracted on the same principle as a piece o[f] metal falling through the air, and repelled for [a] reason analogous to that which causes a soa[p] bubble filled with hydrogen gas to ascend. The[re] is value in M. M. Becquerel's view. It does no[t] assume two different classes of forces, and it indi[-] cates that "magnetism" and "diamagnetism" must follow the same laws, and vary proportion[-] ately to the square of the magnetic intensit[y]. But there are serious physical objections. F[or] instance, it would follow from Becquerel's ex[-] pressions, that all bodies ought to be attracted b[y] magnets *in vacuo,* since the *medium* is, in th[is] case, removed, on whose presence depends t[he] result of attraction or repulsion. Now, ma[ny] diamagnetics are more repelled *in vacuo* than [in] *air;* and M. Becquerel has thus been driven [to] the physical hypothesis, that apparent vacu[um] has a specific magnetism, or that there is a po[si-] tive magnetic fluid permeating all space. W[e] confess as great a repugnance to an augment[a-] tion of the list of impalpable physical fluids, [as] we have to additions to the list of our polariti[es].

(3.) *The Dynamical Theory.*—The view tak[en] by Faraday from the first, and so finely a[nd] largely extended by Professor William Thoms[on] is totally different in its character and found[a-] tions. That view, indeed, rests on no physi[cal] hypothesis whatever, but on the fact that *for[ces]* emanate from the poles of magnets in cert[ain] directions which are called *lines of force,* [and] occupy a *magnetic field.* If any body is plun[ged] within this magnetic field, it disturbs or modi[fies]

these lines of force, according to its nature. If magnetic, it concentrates the lines, or draws them towards itself; if diamagnetic, it makes them diverge—thus originating attractive movements for magnetic bodies, and repulsive for diamagnetic. The several expressive diagrams of lines of force under various circumstances, given in page 458, will enable the reader, to a large extent, to realize this by a mere glance of the eye. For instance, if a magnetic field is composed of equal forces equally distributed—such a field as may be obtained with a horse-shoe magnet—we have merely to place a sphere of iron or nickel in this field to cause an immediate disturbance in the direction of these lines of force, which in this case experience a convergence upon the opposite faces of a magnetic sphere; and there would be a similar divergence at the opposite sides of a diamagnetic sphere. Professor Faraday has likewise shown in what manner temperature diminishes the power possessed by bodies to influencing the direction of the lines of force—even causing them to disappear at certain points.—These remarkable views clearly suggested the foundation of a purely dynamical theory of the entire subject; and they were taken up and carried to their minutest consequences by Professor William Thomson, in whom are happily united great skill in analysis, rarest power of abstraction, and the ability to recognize and grasp firmly the *essentialia* of individual physical phenomena. Mr. Thomson's earliest published researches, dating at 1847, appeared in the *Cambridge and Dublin Mathematical Journal*, in a memoir "*On the forces experienced by small spheres under magnetic influence, and on some of the phenomena presented by diamagnetic substances.*" Starting from the recognized and fundamental truth, that no mutual attraction or repulsion between two bodies can result from magnetism in one, unless the other be also magnetized, or—what is the same thing—that the forces observed are the consequences of a temporary magnetic state induced in the neutral body by the neighbourhood of a magnet, he proceeds to deduce analytically the resultant directive energy of a magnetic field on a sphere of finite dimensions placed anywhere in it; and it follows readily from his formulæ that while a sphere of soft iron is always urged in the direction in which the magnetizing force increases most rapidly, there are cases (cases in which his quantity i is negative) in which the sphere may be urged *across the direction of the magnetizing force*,—in fact, that this must take place with all substances (diamagnetics) in which i (a proper fraction depending on the capacity of the substance for magnetic induction, and determinable by experiment) is negative. The student will obtain a briefer and perhaps clearer idea of the nature of these most interesting researches from a paper in *Philosophical Magazine* for October, 1850, in which, indeed, they are not only summed up but illustrated by experiment. The experiments described very fully in that paper are extremely simple, and show how, through modifying the direction and the intensity of the forces in the magnetic field by various positions given to the magnet's motions may be impressed on *magnetic* bodies—such as a ball of soft iron—exactly analogous to those presented by *diamagnetic* bodies under the same circumstances. For instance, a ball of soft iron of very small volume and very delicately suspended from a long horizontal lever, under the influence of two contrary poles of unequal force placed on the same side in respect to it, but at different distances, may be held in equilibrium at a certain distance from both. So also, the same ball when submitted to the action of two poles, equal in force, but of the same name, will have not only a position of unstable equilibrium in the middle of the lines joining these poles, but also two positions of stable equilibrium, at the two extremities of a right line drawn perpendicularly to the middle of the former, and of a length depending upon the force of the magnetic poles. All these effects, and others of the same kind, may be obtained in a similar manner, by supplying the place of the soft iron by bodies that are very slightly magnetic, and even by diamagnetic bodies. Professor Thomson has frequently resumed this line of investigation. Another memoir (read to the British Association in 1850) is reprinted in *Phil. Magazine* for March, 1851, entitled, "*On the Theory of Magnetic Induction in Crystalline and non-Crystalline Substances.*" In this paper he clears the foundation of the mathematical theory of Magnetism of all physical hypotheses as to "magnetic fluids," and unfolds the extension of Poisson's formulæ (thus cleared) to problems in which it is a condition that the magnetic elements are not spherical, or not symmetrically arranged within a substance in the neighbourhood of a magnet,—thus enabling himself to grasp all the phenomena of what has been termed the magneto-crystalline force. It were vain to attempt within our space more than the foregoing brief narrative and bald outline of the researches of this Physicist; suffice it that he considers that not one of the most striking of recent experimental results—not even Weber's remarkable results—may not be deduced as corollaries from a purely dynamic theory of Magnetism. The scientific world assuredly expects from him a full and methodical work on the entire subject—worthy of the theme itself, and of his own well-won distinctions.—We refer the student for many details respecting all portions of Magnetism to De la Rive's excellent and copious *Treatise*, and to the recent workin three volumes by M. M. Becquerel.—Professor Thomson's chief theoretical paper is in the *Philosophical Transactions* for 1851, Part I. On the most recent condition of this and all cognate physical subjects, every information may be obtained in the new and most choice *Dissertation*, by Professor J. D. Forbes.

Magnetism, Correlation of.—See next article.

Magnetism, Dynamical Relations of.—I. MECHANICAL VALUES OF DISTRIBUTIONS OF ELECTRICITY AND MAGNETISM.—1. *Electricity at rest.*—To electrify an insulated conductor (a Leyden phial, for instance, or any mass of metal resting on supports of glass) in the ordinary way, by means of an electrical machine, requires the expenditure of work in turning the machine. But inasmuch as part, obviously by far the greater part, of the work done in this operation goes to generate heat by means of friction, and of the small residue some, it may be a considerable proportion, is wasted in generating heat (electrical light being included in the term) by the flashes, illuminated points, and sparks, which accompany the transmission of the electricity from the glass of the machine where it is first excited, to the conductor which receives it, the mechanical value of the electrification effected would be enormously overestimated if it were regarded as equivalent to the work that has been spent. The portion of this work which produces the actual electrification of a conductor in stated circumstances is, however, perfectly definite and determinate. The mechanical value of any electrification of a conductor has a perfectly definite character, and it may be calculated with ease in any particular case, by means of formulæ demonstrated by Professor Thomson (Glasgow Philosophical Society, January 26, 1853, and *Philosophical Magazine*, March, 1854). The simplest case is that of a single conductor insulated at a distance from other conductors, or with only uninsulated conducting matter in its neighbourhood. In this case the mechanical value of the electrification is equal to *half the square of the quantity of electricity, divided by the capacity of the conductor.**

In any case whatever, the total mechanical value of all the distributions of electricity on any number of separate insulated conductors electrified with any quantities of electricity, is equal to half the sum of the products obtained by multiplying the "potential"† in each conductor by the quantity of electricity with which it is charged. Each term of this expression does not represent the independent value of the actual distribution on the conductor to which it corresponds, inasmuch as the "potential" in each depends on the presence of the others, when they are near enough to exert any sensible mutual influence; but independent expressions of these independent values are readily obtained, although not in a form convenient for statement here; and their sum is equal to the total value, as calculated by the preceding expression. When a conductor is discharged without other mechanically valuable effects being developed,—as for instance, when the knob of a Leyden phial is put in communication with the outside coating, or when a flash of lightning takes place—the heat is equal in mechanical value to the distribution of electricity lost. Hence, by what precedes, the amount of heat is proportional to the *square* of the quantities discharged, as was partially discovered by various experimenters of last and of the present centuries, but first demonstrated on dynamical principles by Joule, in a communication to the Royal Society in 1840. Mr. Joule's result has been verified by independent experimenters in France, Italy, and Germany. Professor Thomson has pointed out other applications of his investigation, some of a practical kind, and others in the Mathematical Theory of Electricity,‡ and has stated that although he had first arrived at the results in 1845, and used them in papers published in that year, the first explicit publication of the theorem regarding the mechanical value of the electrification of a conductor appears to be in 1847, in a paper entitled "Ueber die Erhaltung der Kraft," by Helmholz, who had independently arrived at the same theorem.

The excellent terms—potential energy and actual energy—which have been introduced by Professor Rankine to designate the statical and the dynamical forms of mechanical energy, are well illustrated by their application to this subject. Thus what has been defined above as the mechanical value of an electric charge is its "potential energy." When two electrified bodies repel one another, and experience no sensible resistance to their motions, a portion of the potential energy of the electrical system is converted into actual energy of motion. If a body is repelled or attracted upwards by electric force, there is a conversion of potential energy of electricity into potential energy of gravitation. If an electrified conductor is discharged without being allowed to produce electrolytic, or ordinary mechanical effects, its potential energy is, as has been remarked above, wholly converted into actual energy of heat and light in the flash. If in its discharge it breaks a body into fragments thrown violently asunder, and some of them raised against gravity by the electric force, and at the same time decomposes water; its potential energy is converted partly into actual energy of motion, partly potential energy of chemic[al]

* The term, "electro-static capacity," has been introduced by Professor Thomson to signify the proportion of the quantity of electricity that the conductor would retain to that which it would communicate to a conducting ball of unit radius, insulated at a great distance from other conducting matter, if connected with it by means of a fine wire. In other words, the electro-static capacity of a conductor is the quantity of electricity which it holds when charged to unit potential.

† A term first introduced by Green, which may be defined as the quantity of mechanical work that would have to be spent to bring a unit of electricity from a great distance up to the surface of the conductor, supposed to retain its distribution unaltered.

‡ Among the latter, an analytical investigation of th[e] mutual attraction or repulsion between two electrifi[ed] spherical conductors may be referred to. Results of th[is] investigation were published as early as 1845, althou[gh] it was not till some years later that the author su[c]ceeded in finding a synthetical verification, whi[ch] along with the original analytical solution, is publish[ed] in the *Philosophical Magazine*, for April, 1853.

affinity, and partly into potential energy of gravitation: and the remainder into actual energy of heat.

2. *Magnetism.*—If a piece of soft iron be allowed to approach a magnet very slowly from a distant position, and be afterwards drawn away so rapidly that at the instant when it reaches its primitive position, where it is left at rest, it retains as yet sensibly unimpaired the magnetization it had acquired at the nearest position, a certain amount of work must have been finally expended on the motion of the iron. For during the approach, the iron has only the magnetization due to the action of the magnet on it in its actual position at each instant, but at each instant of the time in which the iron is being drawn away, it has the whole magnetization due to the action of the magnet on it when it was the nearest. Hence it is drawn away against more powerful forces of attraction than those with which the magnet attracts it during its approach; from which it follows that more work is spent in drawing the iron away than had been gained in letting it approach the magnet. The sole effect due to this excess of work is the magnetization which the iron carries away with it; and consequently, the mechanical value of this magnetization must be precisely equal to the mechanical value of the balance of work spent in producing it.

After a very short time has elapsed with the piece of soft iron at a great distance from the magnet, it will have lost, as is well known, all, or nearly all, the magnetization which it had acquired temporarily in the neighbourhood of the magnet; and in this short time some energy, equivalent to that of the magnetization lost, must have been produced. Mr. Joule's experiments show that this energy consists of heat, which is generated in the iron during demagnetization; and we infer the remarkable conclusion, that at the end of the process which has been described, or of any motion of a piece of soft iron in the neighbourhood of a magnet, from a certain position and back to the same, the iron will be as much warmer than it was at the beginning, as it would have been without any magnetic action, if it had received the heat that would be generated by the expenditure of the same amount of work on mere friction.

A curious experiment illustrating these principles is easily made by subjecting a piece of soft iron at rest to alternate magnetizations and demagnetizations, or reverse magnetizations, through the action of an electro-magnet, and observing the changes of temperature which it experiences, care being taken to prevent any sensible effect of conduction of heat from the coils of the magnet. In a variety of ways it is easy to show by this action an elevation of temperature amounting to several degrees centigrade, in a piece of unmoved soft iron. Surely when this subtle magnetic influence, producing heat at a distance through bodies seemingly insensible to its effects, is known, even as we may now imagine "knowing" it, we shall know that it is a dynamical quality—a modification of the constitutional motions,—of all the matter occupying space in which it rests.

For theoretical indications regarding reverse thermal effects which it is anticipated must be experienced by a body according as it is moved towards or from a magnet, see THERMO-MAGNETISM.

The same considerations are applicable to the magnetization of a piece of steel, with this difference, that according to the hardness of the steel, the magnetization which it receives in the nearest position will be more or less permanent, and if there be any demagnetization after removal from the magnet, it will be much less complete than in the case of soft iron, and that heat will be necessarily generated both in the magnetization which takes place during the gradual approach, and in the subsequent demagnetization. Further, by putting together a number of pieces of steel, each separately magnetized, a complete magnet will be formed, of which the mechanical value will be equal to the sum of the mechanical values of its parts, increased or diminished by the amount of work spent or gained in bringing them together.

3. *Electricity in Motion.*—If an electric current be excited in a conductor, and then left without electro-motive force, it retains energy to produce heat, light, and other kinds of mechanical effect, and it gradually falls in strength until it becomes insensible, as is amply demonstrated by the initial experiments of Faraday and Henry, on the spark which takes place when a galvanic circuit is opened at any point, and by those of Weber, Helmholz, and others on the electro-magnetic effects of varying currents. Professor Thomson has shown how the mechanical value of all the effects that a current in a closed circuit can produce after the electro-motive force ceases, may be ascertained by a determination, founded on the known laws of electro-dynamic induction, of the mechanical value of the energy of a current of given strength, circulating in a linear conductor (a bent wire, for instance) of any form. To do this, it may be remarked, in the first place, that a current, once instituted in a conductor, and circulating in it after the electro-motive force ceases, does so just as if the electricity had inertia, and will diminish in strength according to the same, or nearly the same, laws as a current of water or other fluid, once set in motion and left without moving force, in a pipe forming a closed circuit. But according to Faraday, who found that an electric circuit consisting of a wire doubled on itself, with the two parts close together, gives no sensible spark when suddenly opened, in comparison with that given by an equal length of wire bent into a coil, it appears that the effects of ordinary *inertia* either do not exist for electri-

city in motion, or are but small compared with those which, in a suitable arrangement, are produced by the "induction of the current upon itself." In the present state of science it is only these effects that can be determined by a mathematical investigation; but the effects of electrical inertia, should it be found to exist, will be taken into account by adding a term of determinate form to the fully determined result of the present investigation which expresses the mechanical value of a current in a linear conductor as far as it depends on the induction of the current on itself.

The general principle of the investigation is this—that if two conductors, with a current sustained in each by a constant electro-motive force, be slowly moved towards one another, and there be a certain *gain of work* on the whole, by electro-dynamic force, operating during the motion, there will be twice as much as this of work spent by the electro-motive forces (for instance, twice the equivalent of chemical action in the batteries, should the electro-motive forces be chemical) over and above that which they would have had to spend in the same time, merely to keep up the currents, if the conductors had been at rest, because the electro-dynamic induction produced by the motion will augment the currents; while, on the other hand, if the motion be such as to require the *expenditure* of work against electro-dynamic forces to produce it, there will be twice as much work saved off the action of the electro-motive forces by currents being diminished during the motion. Hence the aggregate mechanical value of the currents in the two conductors, when brought to rest, will be increased in the one case by an amount equal to the work done by mutual electro-dynamic forces in the motion, and will be diminished by the corresponding amount in the other case. The same considerations are applicable to relative motions of two portions of the same linear conductor (supposed perfectly flexible). Hence it is concluded that the mechanical value of a current of given strength in a linear conductor of any form, is determined by calculating the amount of work against electro-dynamic forces, required to double it upon itself, while a current of constant strength is sustained in it. The mathematical problem thus presented leads to an expression for the required mechanical value consisting of two factors, of which one is determined according to the form and dimensions of the line of the conductor in any case, irrespectively of its section, and the other is the square of the strength of the current. The mechanical value of a current in a closed circuit, determined on these principles, may be calculated by means of the following simple formula, not hitherto published:—

$$\frac{1}{8\pi}\iiint R^2\,dx\,dy\,dz,$$

where R denotes the resultant electro-magnetic force at any point (x, y, z). This expression is very useful in the dynamical theory of magneto-electric machines and electro-magnetic engines. As an example, let the circuit consist of a length l of wire, wrapped in a helix approximating to a succession of circles on a cylinder of length a. The mechanical value of a current of strength γ flowing through it is approximately

$$\frac{1}{2}\frac{l^2}{a}\gamma^2,$$

whatever be the diameter of the cylinder, provided it be very small in proportion to the length. For instance, let 100 feet of wire be wrapped on a cylinder an inch or two in diameter, and one foot long. The mechanical value of a current of unit strength (or a current which would decompose about $\frac{1}{10}$ of a grain of water per second) flowing through it is 50,000 dynamical units.

We must divide this by 32·2 to reduce to "foot grains," and we find therefore that the "vis viva," or "actual energy," or "mechanical value" of the current in that case (which is just such as a very ordinary experimental illustration might be) is 1550 times as much as is produced by gravity upon a grain of water descending through one foot. If we divide again this number by 1390, to reduce to thermal units, we find 1·12; and conclude that about a grain and a tenth of water would be raised in temperature one degree cent., by the spark on breaking circuit, or that 1,600 such sparks would give the same elevation of temperature to a quarter of a pound of water. It is well known that these effects are immensely increased by inserting soft iron into the cylindrical space surrounded by the coil. The same theory gives complete indications for finding how much, when experimental determinations of the law and amount of magnetization are complete enough to supply the requisite numerical data.

The following definition is of importance in applications of the theory of the energy of electricity in motion:—

The electro-dynamic capacity of a linear conductor of any form is the mechanical value of a current of unit strength circulating in it.

II. Transient Electric Currents.*—Professor Thomson has determined the motion of electricity at any instant after an electrified conductor of given capacity is put in connection with the earth by means of a wire or other linear conductor of given form and given resisting power. The solution is founded on the *equation of energy* (corresponding precisely to "the equation of vis-viva" in ordinary dynamics), which is sufficient for the solution of every mechanical problem, involving only one variable element to be determined in terms of the time. That there is only one such variable in the present case

* The mathematical investigations from which the results are deduced is published in the *Philosophical Magazine*, June, 1853.

follows from two assumptions which are made regarding the data, namely—

(1.) That the electrical capacity of the first mentioned, or principal conductor, as it will be called, is so great in comparison with that of the second or discharger, as to allow no appreciable proportion of its original charge to be contained in the discharger at any instant of the discharge, which will imply that the strength of the current at each instant must be sensibly uniform through the whole length of the discharger.

(2.) That there is no sensible resistance to conduction over the principal conductor, so that the amount of charge left in it at any instant of the discharge will be distributed on it in sensibly the same way as if there was complete electrical equilibrium.

The results of the investigation include expressions for the mechanical values of the charge left in the principal conductor, and for the electrical motion in the discharger, at any instant, in terms of the amount of that charge, and the rate at which it is diminishing. The sum of these two quantities, constitutes the whole electro-statical and electro-dynamical energy in the apparatus, and the diminution which it experiences in any time, must be mechanically compensated by heat generated in the same time. We have thus an equation between the diminution of the electrical energy in any infinitely small time, and the expression according to Joule's law for the heat generated in the same time in the discharger multiplied by the mechanical equivalent of the thermal unit. The equation so obtained is in the form of a well known differential equation, of which the integral gives the quantity of electricity left at any instant in the principal conductor, and consequently expresses the complete solution of the problem. Precisely the same equation and solution are applicable to the circumstances of a pendulum, drawn through a small angle from the vertical, and let go in a viscous fluid, which exercises a resistance simply proportional to the velocity of the body moving through it.

The interpretation of the solution indicates two kinds of discharge, presenting very remarkable distinguishing characteristics; a continued discharge, and an oscillatory discharge, one or other of which will take place in any particular case. In the continued discharge the quantity of electricity on the principal conductor diminishes continuously, and the discharging current first increases to a maximum, and then diminishes continuously until after an infinite time equilibrium is established. In the oscillatory discharge, the principal conductor first loses its charge, becomes charged with a less amount of the contrary kind of electricity, becomes again discharged, and again charged with a still smaller amount of electricity, but of the same kind as the initial charge, and so on for an infinite number of times, until equilibrium is established; the strength of the current and its direction, in the discharger, has corresponding variations; and the instants when the charge of either kind of electricity on the principal conductor is at the greatest, being also those where the current in the discharger is on the turn, follow one another at equal intervals of time. The continued or the oscillatory discharge takes place in any particular case, according to the "electro-static capacity" of the principal conductor, the "electro-dynamical capacity" of the discharger, and the resistance of the discharger to the conduction of electricity. Thus, if the discharger be given, it will effect a continued or an oscillatory discharge, according as the capacity of the principal conductor exceeds or falls short of a certain limit. If the principal conductor, and the length and substance of the discharger, be given, the discharge will be continued or oscillatory according as the electro-dynamic capacity of the latter, depending as it does on the form into which it is bent, falls short of, or exceeds a certain limit. Lastly, if the principal conductor, and the length and form of the discharger be given, the discharge will be continued or oscillatory, according as the resistance of the discharger to conduction exceeds or falls short of a certain limit.

It ought to be remarked, that although the electrical equilibrium is not rigorously attained, whatever kind of discharge it may be, in any finite time; yet practically, in all ordinary experimental cases the discharge is finished almost instantaneously as regards all appreciable effects; and the great obstacle in the way of experimenting at all on the subject arises from the difficulty of arranging the circumstances, so that the periods of time indicated by the theory for the succession of various phenomena (as, for instance, the alternations of the charges of the contrary electricity on the principal conductor) may not be inappreciably small.

It is not improbable that double, triple, and quadruple flashes of lightning which are frequently seen on the continent of Europe, and sometimes, though not so frequently, in this country, lasting generally long enough to allow an observer, after his attention is drawn by the first light of the flash, to turn his head round and see distinctly the course of the lightning in the sky, result from the discharge possessing the oscillatory character. A corresponding phenomenon might probably be produced artificially on a small scale, by discharging a Leyden phial or other conductor across a very small space of air, and through a linear conductor of large electro-dynamic capacity and small resistance. Should it be impossible on account of the too great rapidity of the successive flashes, for the unaided eye to distinguish them, Wheatstone's method of a revolving mirror might be employed, and might show the spark as several points or short lines of light separated by dark intervals, instead of a single point of light, or of an unbroken line

of light, as it would be if the discharge were instantaneous, or were continuous and of appreciable duration.

The experiments by Riess and others on the magnetization of fine steel needles by the discharge of electrified conductors, illustrate in a very remarkable manner the oscillatory character of the discharge in certain circumstances; not only when, as in the case with which we are at present occupied, the whole mechanical effect of the discharge is produced within a single linear conductor, but when induced currents in secondary conductors generate a portion of the final thermal equivalent.

The decomposition of water by electricity from an ordinary electrical machine, in which, as has been shown by Faraday, more than the electro-chemical equivalent of the whole electricity that passes appears in oxygen and hydrogen rising mixed from each pole, is probably due to electrical oscillations in the discharger consequent on the successive sparks.* Thus, if the general law of electro-chemical decomposition be applicable to currents of such very short duration as that of each alternation in such an oscillatory discharge as may take place in these circumstances, there will be decomposed altogether as much water as is electro-chemically equivalent to the sum of the quantities of electricity that pass in all the successive currents in the two directions, while the quantities of oxygen and hydrogen which appear at the two electrodes will differ by the quantities arising from the decomposition of a quantity of water electro-chemically equivalent to only the quantity of electricity initially contained by the principal conductor. The mathematical results lead to an expression for the quantity of water decomposed by an oscillatory discharge in any case to which they are applicable, and show that the greater the electro-dynamic capacity of the charger, the less its resistance, and the less the electro-statical capacity of the principal conductor, the greater will be the quantity of water decomposed. Probably the best arrangement in practice would be one in which, in place of a principal conductor fulfilling the conditions prescribed above, merely a small ball or knob is substituted, but those conditions not being fulfilled, the circumstances would not be exactly expressed by the formulæ of the present investigation. The resistance would be much diminished, and consequently the whole quantity of water decomposed much increased, by substituting large platinum electrodes for the mere points used by Wollaston; but then the oxygen and hydrogen separated during the first direct current, would adhere to the platinum plates and would be in part neutralized by combination with the hydrogen and oxygen brought to the same plates respectively by the succeeding reverse current; and so on through all the alternations of the discharge. In fact, if the electrodes be too large, all the equivalent quantities of the two gases brought successively to the same electrode will recombine, and at the end of the discharge there will be only oxygen at the one electrode and only hydrogen at the other, in quantities electro-chemically equivalent to the initial charge of the principal conductor. Hence we see the necessity of using very minute electrodes, and of making a considerable quantity of electricity pass in each discharge, so that each successive alternation of the current may actually liberate from the electrodes some of the gases which it draws from the water.

* This conjecture was first, it is believed, given by Helmholz, the existence of electrical oscillations in many cases of discharge having been indicated by him as a probable conclusion from the experiments of Riess, alluded to in the text.

The above results may be applied to determine the laws, according to which a current varies at the commencement and end of any period, during which a constant electro-motive force, such as that of a galvanic battery, acts in a conductor of given electro-dynamic capacity and resistance, and to show how the relation between the electro-statical and electro-dynamic units of electrical quantity and electro-motive force may be experimentally determined.

Induction Coils.—In the single coil apparatus, the recipient arc completes a circuit with the induction coil. A battery sends its current divided through the "coil" and the "recipient arc," in quantities inversely proportional to the resistances of these two channels. At the instant when the battery circuit is broken, the current in the coil, with its great *momentum*, overbalances the comparatively small *momentum* of the current previously excited in the recipient arc by the direct action of the battery, and gives rise to the "induced current."

In the double apparatus—with primary and secondary coils—the impulse induced in the secondary coil is equal in absolute measure tot he strength of the current stopped in the primary, multiplied by a co-efficient of induction. The whole quantity of the current which it produces is equal to its own measure divided by the resistance in the whole circuit of secondary coil and recipient arc. The more sudden the stoppage of the primary current, the more intense and the shorter in duration is the shock in the secondary, and hence alone the improved effect produced by Fizeau's addition of the condenser.

References: Joule, *Philosophical Magazine Proceedings Royal Society*, and *Proceedings Manchester Philosophical Society;* various articles from 1840 to recent dates—especially (1) "Generation of Heat by Electricity;" (2), "On the Heat of Electrolysis;" (3), "On the Calorific effects of Magneto-electricity and the Mechanical value of Heat." Helmholz, "Erhaltung der Kraft," Berlin, 1847. Grove, "Correlation of the Physical Forces" Thomson, Papers referred to above; also, "Mechanical Theory of Electrolysis," and "Applications of the Principles of Mechanical Effect, &c."; *Philosophical Magazine*, Dec., 1851.

Magnetism of Ships. See VARIATION OF COMPASS.

Magnetism, Terrestrial. It has long been known that the Globe as a whole exerts certain magnetic influences, enabling it, for instance, to act on a freely suspended magnetic needle, in a manner varying (within certain limits) with the locality where the needle is placed. All effects of Terrestrial Magnetism, however varied they appear, are indeed ultimately referable to some one of the changes indicated by the Needle; which changes may therefore be taken as the cardinal phenomenon of the subject. Now, this phenomenon, although in itself simple and indivisible, becomes more palpable, if at first we regard it under three aspects.—I. Let a steel bar or needle be magnetized by the ordinary processes, and its centre of gravity then detected. If it be freely suspended by that centre, or placed as below on the top of a pointed support, so that it may swing freely and assume any direction, it will be found that, in every locality, the needle chooses a certain

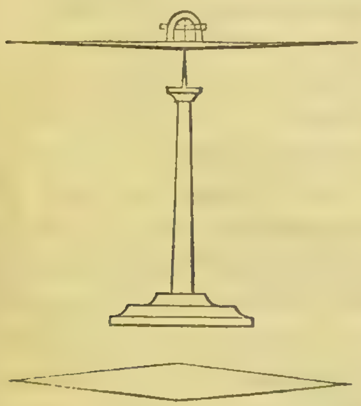

Fig. 1.

direction; it first oscillates round that direction —as the pendulum does from side to side of the direction, of gravity—and, finally, returns and rests there. Speaking roughly, this natural direction of the needle is from South to North: nevertheless, it does not in general coincide with the Geographical Meridian, but deviates from it by an angle termed the *Magnetic Declination*. The line along which the needle really does point, may be called the *Magnetic Meridian*.—II. Take next an *unmagnetized* steel needle, and pass through its centre of gravity a horizontal axis: if the ends of that axis be placed on horizontal supports, the needle will necessarily assume a horizontal position whatever be its direction relative to geographical or magnetic meridians. Let the needle be now magnetized, and replaced on its supports: —a phenomenon quite of a new description immediately appears. The simple act of magnetizing the bar, cannot, of course, have changed the absolute *gravity* or weight of either of its arms; nevertheless, it no longer retains the horizontal position; one end of it *seems* to have been made heavier than the other, for that end *dips*. This phenomenon is represented in fig. 2. The needle thus suspended is called the *dipping needle;* and the apparatus now exhibited is planned to measure this *inclination* or *dip*—in other words, the angle separating the lower end of the needle from the horizontal point. The instrument delineated below (fig. 2), has a motion in azimuth round a vertical axis L; and the position of the needle in azimuth, or relative to the geographical meridian, is noted on the graduated circle O, O, O, by a vernier or microscope at K. If the needle itself were free to move in azimuth (which the mode of its suspension absolutely prohibits), it would undoubtedly, besides dipping, assume the direction of the declination needle. Turn round the instrument on the axis L, until the needle be in the declination plane, and we shall then have it, in all respects, freely under the influence of the Terrestrial Magnetic Force. The deviation of the lower end of the needle from the horizontal point, under these circumstances, is the true or absolute *dip* or *inclination*, characteristic of the locality and time at which the observation is made. If the instrument is turned in azimuth, so that the plane of the needle lie at right angles to the magnetic meridian, the *Declination Force* will manifestly be nullified, and we shall find the needle pointing *vertically* in absolute obedience to the *Inclination Force;*—between which position and that of the declination plane, the departure of the needle from the horizontal necessarily varies according to the azimuth of the plane to which it is constrained. These are the two elements or manifestations by which the *direction* of the Terrestrial Magnetic Force is determinable; but, as has been already indicated, they are not *separate* elements; on the contrary, their apparent separation is the mere result of convenience as to the best means of determining them. The free position of the dipping needle, when its plane has the suitable azimuth, represents by itself the total directive effect of the Terrestrial Magnetic Force.—III. There must, however, be a *third* element connected with this Force, which it is most needful to know. The foregoing elements merely inform us of the line along which that Force acts in different localities; they throw no light whatever on its *magnitude* or *intensity*. Let its power be small or great, the needle would deflect and dip precisely in the observed directions,—just as a stone would fall along the vertical to the surface of the earth, whatever the size of our planet or the amount of gravity belonging to it. The third essential element of our general Telluric phenomenon, therefore, is this *Intensity*. And it is clear that when the Declination, Inclination, and Intensity, are determined for any given place, the effective action of Terrestrial Magnetism at that place has been experimentally valued and defined.——An inquiry whose conditions and aims are thus easily

narrated, might seem a simple and circumscribed one. But the subject of Terrestrial Magnetism is now one of the widest in Physics; it has sometimes occupied immense enterprises; and it is rapidly leading us into presence of profound cosmical relations of our Globe. In the sequel of this paper we shall examine the character of the researches which this inquiry has originated; describe the accessible modes of conducting these researches; narrate the efforts already expended in this direction alike by individuals, associations, and governments; and record the general Laws which have been established, as well as the nature of others to which our present yet imperfect

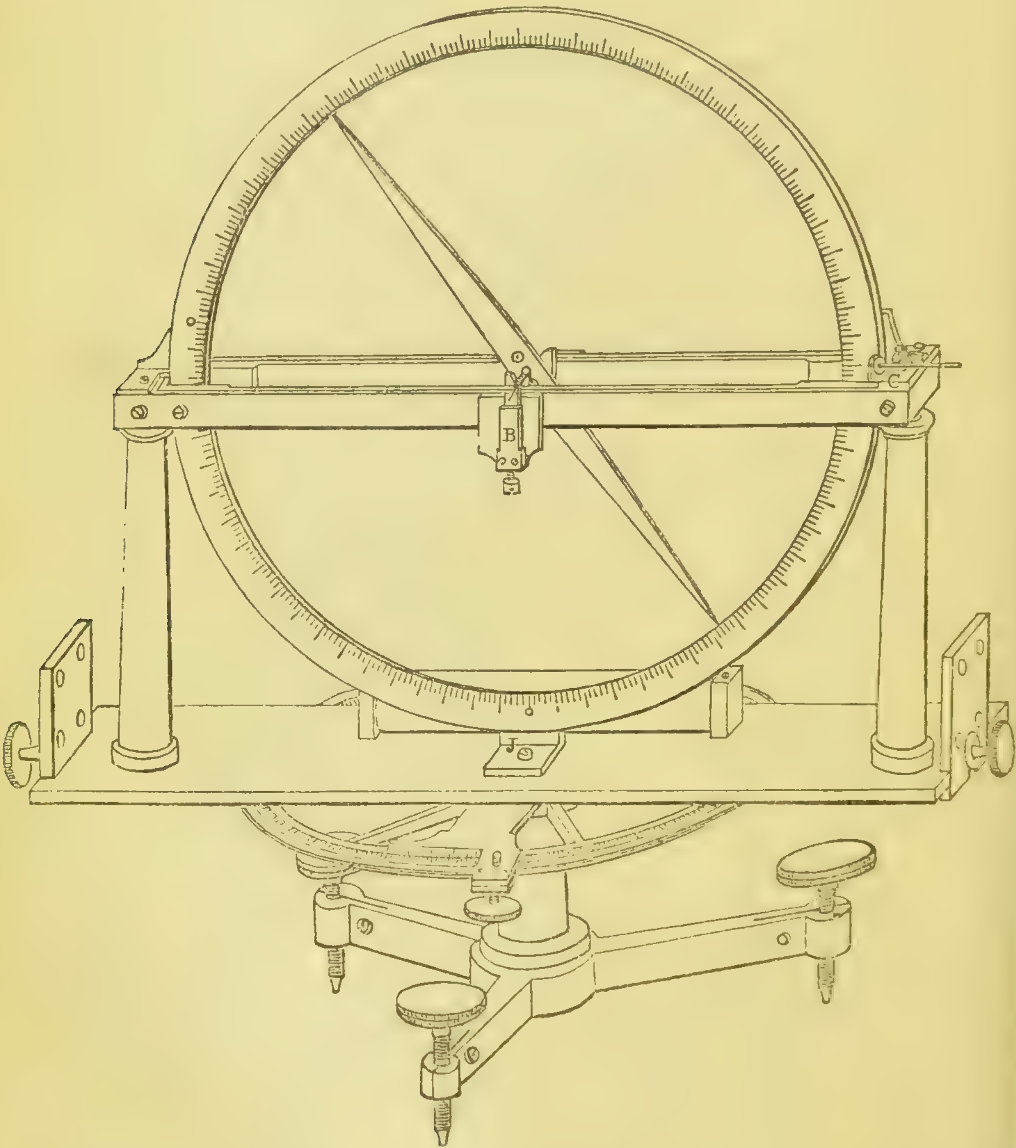

Fig. 2.

knowledge, appears to point. Our essay shall terminate with a brief notice of Theories of Terrestrial Magnetism.

I.

Modes of Determining the Elements of the Telluric Magnetic Force at any one place, as well as their Disturbances or Variations.—An endeavour to give satisfactory accounts of the various instruments employed at different times, in determining the essential elements of the earth's Magnetic Force, would lead us quite beyond present limits. Suffice it, that although many of these older instrumen[ts] were admirable as to construction—issuing fr[om] the workshops of Troughton of London, Ga[m]bey of Paris, &c., they were in nowise unifo[rm] either as to plan or delicacy, and were also [for] the most part unsuited to play an equal part [in] determining the absolute values of the Eleme[nts] and the Variations to which every one of thes[e is] subject. An entire change in our instrumen[tal] means, and, indeed, in our general method of [in]quiry, was proposed by the illustrious Ga[uss] between the years of 1832 and 1836,—a cha[nge]

apart from which or some equally effective proposal, the modern and greatest era in the history of Terrestrial Magnetism, could not have been inaugurated. Discerning that it was not necessary for the object of science, that the three foregoing essential elements should be determined separately and directly, provided they could be determined indirectly or *mediately* with greater accuracy and convenience, Gauss wholly abandoned part of the old method of inquiry. Although the element of the Inclination or Dip for instance, may be ascertained by direct observation with some facility, the usual or even any possible form of Inclination Needles does not render them suitable for giving nice indications of the passing variations of that element. But the element of the INTENSITY may be resolved into two rectangular components—viz., the *Horizontal Intensity*, and the *Vertical Intensity*,—components, fortunately, capable of being ascertained with comparative minuteness and ease: and it is evident that if these are fixed, their *resultant* or the total intensity is known, as well as its *direction;* which latter is identical with the direction of the Dipping Needle, or with the angle of the Inclination itself. We shall briefly describe the three primary instruments, constructed by Gauss, in accordance with the foregoing views, and also indicate the mode of using them.

1. *The Declinometer, or Declination-Magnetometer.*—This fine instrument consists essentially in a Magnetic Bar of high directive force, suspended at its centre of gravity by a very long thread, and protected carefully from all disturbance originating in aerial currents, by being judiciously enclosed. The position of such a bar can be the resultant of only two directive forces, —viz., that acting in the Magnetic meridian of the place, and the *torsion* of the suspending thread. For the elimination of all the force of torsion that could be eliminated, and the measurement of what inevitably remained, the Inventor provided simple and effective expedients; so that the position of the bar might be finally held to indicate simply and purely the *declination* of the free needle. But the most valuable and original portion of this apparatus—as in fact of all the instruments inaugurated by Gauss in relation to such purposes —consists in its peculiar adaptation to *nicest determinations of the position of the magnetic bar at any given moment of time.* This measurement is effected, in the following way, by a theodolite placed at a considerable distance from the bar:—The bar itself carries a small mirror, nicely adjusted on its extremity nearest the telescope; and at the object end of the telescope a scale of equal parts is placed horizontally, in such a manner, that its image, reflected back from the mirror, can be read through the telescope. It is scarcely necessary to say that the division of the reflected scale, coinciding with the optical axis of the telescope, must, if the latter be rightly adjusted, indicate at any time the azimuth of the magnetic bar; but it ought to be added, that the effect of the arrangements now indicated, is such, that the azimuth in question may be determined to tenths of seconds of space. Referring for further details as to construction, &c., to Gauss's own memoir, reported in the second volume of *Taylor's Scientific Memoirs*, we proceed to indicate the mode in which the admirable instrument now described fulfils its three grand purposes.—1. The azimuth of the Needle, or the momentary direction of the Magnetic meridian, can thus, as we have just indicated, be at any instant precisely fixed. The method proposed by Gauss, and universally adopted, is as follows:—The Magnetic bar, although constantly in motion, must yet at any given moment oscillate regularly on either side of the magnetic meridian at that moment. Now, as the time occupied by these oscillations (supposing the horizontal intensity invariable for the moment), is as uniform as that of the oscillations of a pendulum, it is clear that the *mean* of any two positions of the needle corresponding to two instants separated by an interval of time exactly equal to the time of an oscillation, must coincide with the magnetic meridian at that moment. Or, to make the determination as accurate as it can be made, suppose it were required to fix the magnetic meridian from any given moment T, and suppose t the ascertained time of an oscillation, then if the degree on the scale corresponding with the optical axis of the telescope be marked for such successive times as

$$T - \tfrac{5}{2}t,\ T - \tfrac{3}{2}t,\ T - \tfrac{1}{2}t,$$
$$T + \tfrac{1}{2}t,\ T + \tfrac{3}{2}t,\ T + \tfrac{5}{2}t,$$

the mean of these will give the time T and the corresponding mark on the scale as required. In this simple manner—simpler in practice, even than in description—the absolute position of the Magnetic meridian can be ascertained by Gauss's instrument for any moment; and through its remarkable efficiency, facts have been already accumulated in distant regions of the globe, quite as worthy to serve as foundations in our research of the laws of Horary, Seasonal, and Secular Variations in the Magnetic Declination, as any that have been accumulated respecting the diurnal or annual progress of Terrestrial Temperature.—2. But another object is required to be accomplished by any Declinometer, than that it should indicate momentary or horary variations. It is needful to ascertain the mean or absolute Declination of the Needle at the place. This aim was, of course, readily, although roughly, fulfilled by the connection of the theodolite with a well ascertained *meridian-mark;* but the more delicate question still remained as to the mode of eliminating the minuter horary and seasonal variations. It is impossible to do this absolutely. We cannot determine, otherwise than approximately, the mean temperature of a day or a month or a year, on the basis of observations at fixed intervals. But a series of excellent discussions enabled Gauss to conclude that the mean of the magnetic

meridians taken at *eight* in the morning and at *one* in the afternoon, might be assumed as the absolute Magnetic Declination of the year. Rather unfortunately, in some important respects, the time by which observers reckoned in all the elaborate system of Observations soon to be noticed, was Göttingen mean time:—it ought to have been *Apparent Solar Time.* Nevertheless, the mean Magnetic Declination has now been determined absolutely for various and distant places; and we have obtained data from which to start in all future speculation as to *secular variations.* —3. The Declinometer is also the best instrument for determining, in absolute measure, the Horizontal component of the earth's Magnetic Intensity. Exactly as the periods of the oscillations of an Invariable Pendulum enable us to compare the force of Gravity at different places on our globe's surface, the periods of the oscillations of a freely suspended invariable Magnet would yield similar conclusions as to the Horizontal Magnetic Force. But although this easy method has still to be mainly relied on, as to the magnetic influence manifested at places not easily accessible, it is evident that it cannot answer the purposes of perfectly accurate Science, because no practical means exist of ensuring the invariability of the proper Magnetism of the oscillating bar. The attention of inquirers accordingly turned to the research whether a method could be devised of readily determining for each separate locality, the *absolute* instead of the *comparative* value of the element in question; and Gauss finally originated a process which with some modification has since been universally adopted. The process in principle is this:—*First*, observe the time of vibration of a freely suspended horizontal magnet under the influence of the Earth alone,—this will give the *product* of the horizontal component of the Earth's magnetic force, into the moment of the free magnetism of the bar. *Secondly*, employ this same magnet to act upon another also freely suspended, and note the effects of its action combined with that of the Earth,—this will give the *ratio* of the same quantities whose *product* had just been discovered:—whence by easy calculation, the value in absolute measure of the Horizontal Intensity. Gauss placed the deflecting magnet, with its axis in a right line passing through the centre of the suspended bar of the magnetometer and perpendicular to the magnetic meridian,—in which case the tangent of the angle of deflection is equal to the ratio of two forces; and it merely remained to deduce from that ratio, the ratio of the magnetic moment of the deflecting bar to the earth's force. For details the student is referred to Gauss's own essay, "*Intensitas vis magneticæ terrestris ad mensuram absolutam revocata,*" or to other admirable memoirs by Professors Lloyd of Dublin, and Lamont of Munich. The valuable collections of Mr. Richard Taylor, are also quite a repertory on this important subject.

2. *The Bifilar Magnetometer.*—The instrument thus designated has, for its exclusive object, to determine the *variations* of the horizontal component of the Terrestrial Magnetic Force—that component whose absolute amount may, as we have just seen, be found at any time by aid of the *Declinometer.* It is simple in principle, easy of management, and in accuracy all that can be desired. It sprinciple is as follows:—Suppose a strongly magnetized bar, constrained into a position at right angles to the magnetic meridian, it is plain that the horizontal Magnetic Force will act with all its energy, and under the most advantageous circumstances, to drag the bar out of this transverse position and towards its natural one, which of course is coincident with the magnetic meridian of the place. Now, the contesting or constraining force, is the force of *torsion*, applied in a way, the merit of suggesting which is unquestionably due to Sir William Snow Harris. The magnet bar is suspended by two equi-distant wires, two ends of which are attached to its sides at its centre (the bar lying *flat*), and the upper ends, to the opposite sides of a circle whose diameter is equal to the breadth of the bar, and which can be turned round its centre. The amount of constraining force that can thus be exercised on the position of the magnetic bar, is evidently very great; and no large quantity of torsion communicated by turning the upper circle, suffices to place that bar at right angles to the magnetic meridian. It is clearly within reach of computation to determine exactly this force of torsion, whatever be the position of the bar; and as the intensity of the earth's horizontal magnetic force, is the only power opposed to it, or which can make the bar shift, it is easy to see that these shiftings of the bar, accurately measured, must directly indicate the *variations* of that force. To effect the requisite measurements, Gauss applied the same methods which had been found so effective in the case of the Declinometer; but we must refer to the memoirs of this distinguished geometer and of his companion Weber—to the contributions of Dr. Lloyd—and very especially to the unrivalled labours of the Observatory of Greenwich, for details as to the precautions required, and the *corrections*, essential to the reduction of the primary observations. Suffice it, that through aid of science and personal skill, the observer is now omnipotent here. Not a vestige of change, impressed by any cause, on the horizontal component of the Earth's Magnetic Force, can at present escape him.

3. *The Balance Magnetometer.*—The object of this Magnetometer, is correlative with that of the Bifilar Instrument. It proposes to note and estimate the momentary variations of the Vertical component of the total Magnetic Force. It has been already indicated that, placed in a plane at right angles to the plane of the Magnetic Meridian, the position of the needle must always be the *vertical* one, unless at the *Magnetic Equator*,

where the laws of gravity alone are obeyed by it. Turn, then, an energetic Inclination Needle into this plane, and load one arm of it until the needle assume the horizontal position. The bar will clearly diverge, upwards or downwards, from the horizontal line, just as the vertical component force changes; and by aid of a mirror placed on its axis capable of reflecting scales, the most evanescent of these variations may be observed as accurately as has been shown to be practicable with the other elements we have discussed.—See again the Memoirs of Professor Lloyd.

The scheme of observing, as detailed above, and as inaugurated by Gauss, evidently requires but one supplementing, so that it be complete. No provision is made for determining the *absolute vertical force.* But with a clear determination of the horizontal component in absolute measure, this latter may be inferred from absolute determinations of the *Inclination.* And although *variations* of Inclination cannot be correctly or conveniently found by ordinary Inclination instruments, this absolute determination is not difficult. The whole phenomena therefore are now within reach of science.

II.

The Range of Observation now accomplished.—It is impossible to narrate here, how much the science of Terrestrial Magnetism owes to the earlier observers; in which class we include all who wrought previous to the period of the Reform by Gauss. Many able men from Halley's time downwards, using well their opportunities, have combined to establish facts of paramount importance towards the discovery of the Laws of this remarkable influence. Let us name especially Wilcke, Hansteen, Rossel, Duperrey, Erman, Barlow, Sir James Clarke Ross, and Colonel Edward Sabine. But that invention of uniform, competent, and accessible instruments, which has just been commemorated, opened a field much too extensive for culture by separate efforts; inasmuch as it rendered practicable to pursue continuous, systematic, and related observations in very different and remote places on the Earth. A society of private observers, headed and urged on by Gauss, early confronted the arduous enterprise. This society adopted the system pursued at Göttingen as their normal one; and its members wrought effectively and in harmony. Many important conclusions were soon arrived at. Certain general laws appeared to dawn; the simultaneity of magnetic perturbations within considerable districts, was confirmed, as well as the influence of distant auroras. But no earnestness of private endeavour, no energy of personal enthusiasm, was adequate to cope with a problem essentially so vast. The field of that inquiry is not limited to one continent, but embraces the entire globe; and the satisfactory cultivation of it, could issue only through the willing and effective aid of the most powerful governments of the world. In 1819, the illustrious Humboldt, under a sense of this necessity, applied for the interference of the Emperor of Russia; and, with such success, that magnetic establishments were immediately formed in different parts of the Russian Empire —as far as China. Assisted by the Royal Society of London and the British Association for the Advancement of Science, the same celebrated and persevering individual afterwards laid the case and its claims before the British Government; and the memorial was responded to with alacrity, promptness, and liberality. Two complete magnetic Observatories were established at Dublin and Greenwich; and for distant stations, those places were chosen that seemed likely to be the theatres of the more critical variations of magnetic action,—places in opposite positions in respect to the magnetic Poles and to the magnetic and geographical Equators. One observatory thoroughly appointed was established at *Toronto* in Canada; another at *Van Diemen's Land;* places almost antipodal, and therefore influenced by opposite seasons, and in the neighbourhood besides of the two points of maximum magnetic Energy. A third station was the *Cape of Good Hope*—a point long recognized as remarkable for the well-pronounced secular change of the magnetic elements there: and the fourth observatory was established at *St. Helena*, an island in immediate proximity with the magnetic and geographical Equators, and near the line of minimum force. The instruments supplied to these Observatories were of the best dimensions, as that question was understood at the time; and no available contrivance or expense was spared, to endow them with the highest precision. A considerable military service, under the general direction of Colonel Sabine, was organized at each station; and the observers succeeded each other, so that there was no interruption of work by day or night. The action at all the places was simultaneous—being regulated by Göttingen time. At first, observations were made once every two hours; afterwards at every Göttingen hour; and when extraordinary perturbations were noticed in the needle, its positions were marked, *at least*, every five minutes. Regular work was sustained in all these observatories for several years—in no case for less than five years. Not less important were the efforts of the Russian Government. The results of the labours of the commission placed under direction of M. Kupffer, and conducted in exact agreement with the foregoing system, fill up many volumes—besides the special one published in 1852, containing the pure or reduced observations made at Petersburg, Catterinenburg, Barnaoul, Nertchinsk, and Sitka. Add to these, the stores accumulated in Italy; in Germany, especially by Gauss, Weber, and Lamont; in Holland, Sweden, and the United States of America; in Brussels by the indefatigable Quetelet; and the long series made at Paris by the illustrious Arago. Nor in enumerating the

sources, from which general Laws may now be deduced, can we omit to refer to private observers,—foremost among whom stands the venerable Sir Thomas Makdougall Brisbane, to whose ever wakeful munificence we owe the valuable volumes from Makerstoun. Much more had to be accomplished, however, than the simple collection of rude facts. As in the case of Astronomy every one of these facts had to be reduced or purified, before its effective significance could appear; and the reduced facts themselves required then to be classified, compared, and discussed. Many Physicists in the various countries named, have laboured with high success in this department; but it were wrong not to signalize the very great debt under which the whole subject of Terrestrial Magnetism lies to Colonel Edward Sabine. Under his able directions, the vast contributions of all the British observatories, have been brought into a manageable shape,—their bulk, when reduced, filling many quarto volumes. To these volumes Colonel Sabine has prefixed valuable introductions, as well as graphical representations of the variations at each place. These variations he has compared also with those recorded in other parts of the world; and, the whole task has been accomplished not only with remarkable sagacity, but also with a freedom from theoretical bias of rarest occurrence, although most necessary when one undertakes to lay foundations for the *first* or empirical Laws of any Science. This extensive, complex, and powerful organization has in the meantime suspended its labours. The reason for temporary suspension, is thoroughly satisfactory. At the commencement of this great work, no exact laws or even outlines or foreshadowings of exact Laws in Terrestrial Magnetism, could safely be said to have been ascertained. The best mode of observing therefore, so that the highest or absolute laws be reached, was likewise necessarily unknown. *Now* the ground is cleared; or at least in the act of being so; and no long time can elapse, ere those special points shall come out, towards the definition of which, future observation ought to be directed. Doubtless our Inquirers will then start afresh, if with restricted, certainly with more direct and definite aims, and with means adjusted to the new and special task. Meanwhile Greenwich and a few cardinal stations continue their activity—having bestowed on their former instrumental resources, the inestimable advantage of self-registry by photography.—In the following sections of this article we shall describe those general Laws, whose discovery has been the first fruits of all these elaborate and meritorious investigations. The seed sown so carefully has certainly not remained barren in the ground.

III.

General Results of Observations concerning the Magnetic Force of the Earth.—The important subject now to occupy us, admits of division into two distinct parts;—the *first* taking account of all information hitherto obtained regarding the Mean Values of the Three Magnetic Elements, viz., the *Declination*, the *Dip*, and the *Intensity*, and of their apparent secular variations;—the *second* recording the Laws indicated or ascertained, which seem to govern their diurnal and annual variations, and others of comparatively short periods. The former portion of the subject may be designated as that of *Mean Values*; the latter as that of *Variations*;—to which we must add a third section, viz., *Irregular Changes*.

I. *Mean Values, and their apparent Secular Variations.*—The honour unquestionably belongs to Dr. Halley of first attempting to collect and co-ordinate all accessible facts with regard to the Telluric Magnetic Force, and to present these graphically on maps, by that inestimable expedient which, as carried out by Humboldt in the case of Isothermal Lines, has acted so powerfully in advancing the study of the distribution of temperature over our globe. It cannot be alleged, any more than in maps of the Isothermals, that every portion of these lines may equally vindicate the claim of being authoritatively determined; neither are the maps of the different elements entitled to equal credit: nevertheless, a vast mass of reliable information has without doubt been accumulated and represented, which must exert a powerful influence on the progress of knowledge as to the absolute nature of this remarkable Force. It is not within our present reach to reproduce all the valuable maps that are now part and parcel of our best treatises on Magnetism, and which enter into every adequate Physical Atlas. To manifest something of the direction of the Earth's Magnetic Force, the accompanying sketch of our globe's *Magnetic Meridians* will suffice. The term *Magnetic Meridian* has been employed variously: the signification now attached to it is the following;—suppose one sets out from any locality whatever, and follows, first towards the North and next towards the South, the direction of a compass or Free Magnetic Needle, the curve he would trace is the Magnetic Meridian of all those places. These curves or meridians seem according to present observations, to converge towards, and apparently terminate in two points in each hemisphere, hence generally called the four Magnetic Poles, or the corresponding pairs of Magnetic Poles. The well defined cross line at right angles to all these Magnetic Meridians may, after the analogy of the Geographic Equator, be named the *Magnetic Equator*: but of this there is another definition, viz., the line across the surface of the globe, *where the Dipping Needle remains horizontal.* This latter great circle or curve is represented by the dotted line in the map. This curve does not differ much from that of the Magnetic Equator as first defined. And it is further most worthy of remark that the American Pole, as indicat

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by the convergence of these meridians, is very nearly the spot at which Sir James Clarke Ross found the angle of Inclination to be 90°, or rather 89° 59′. It would appear, indeed, that the critical points on the surface of the earth, might at present be determined by either element indifferently. Not so, however, with regard to the *Intensity*. This is the element the most difficult of all to determine. It needs skill in *experiment*, not mere casual *observation;* and few even of our best navigators have, until quite recent times, had that preliminary acquaintance with physics, apart from which no man can, in reference to so nice a point, determine well. The only reliable map, or rather the elements of the only reliable map yet in our possession, we owe to Colonel Edward Sabine. His excellent memoir in the *Transactions of the British Association*, has been corrected and added to only by himself, in those valuable essays prefixed to the volumes containing the reduced observations of the Magnetic Establishments in our Colonies. We cannot at present discuss those maps of Intensity: suffice it that the Intensity pole is neither the Declination nor the Inclination pole.—The law of the increase of Intensity, as we pass from Equator to Pole is, under certain limitations, approximately represented by the formula of Biot.

$$I = \sqrt{1 + 3 \sin^2 \lambda}.$$

This formula, indeed, would be a perfect expression of the law of Magnetic Intensity if the Earth were perfectly homogeneous, or if the magnetism of a place depended solely on its latitude.—The *Intensity Poles*, are manifestly the true *Magnetic Poles*—those points or regions, whose determination is of greatest importance towards a complete theory of Terrestrial Magnetism; and just maps of Isodynamic Lines, are, therefore, of all others our *desideranda*. But the earliest steps have been effectively taken.—The maps of the Magnetic Elements now made, are, of course, stand-points, from which the future physicist must contemplate the corresponding phenomena of his age. No satisfactory knowledge as to the general laws of the secular changes of those elements ought to be expected, in our present mere commencements of accurate Observation: but the profound interest of Navigation and Commerce in the state of the Needle has given rise nevertheless, to the preservation of records of greatest value in their bearing on the problem of the secular changes of the Declination. In the cut subjoined, the line of *no variation*, at different epochs, is distinguished:—at those epochs, the declination needle departed from the true north-easterly or westerly,

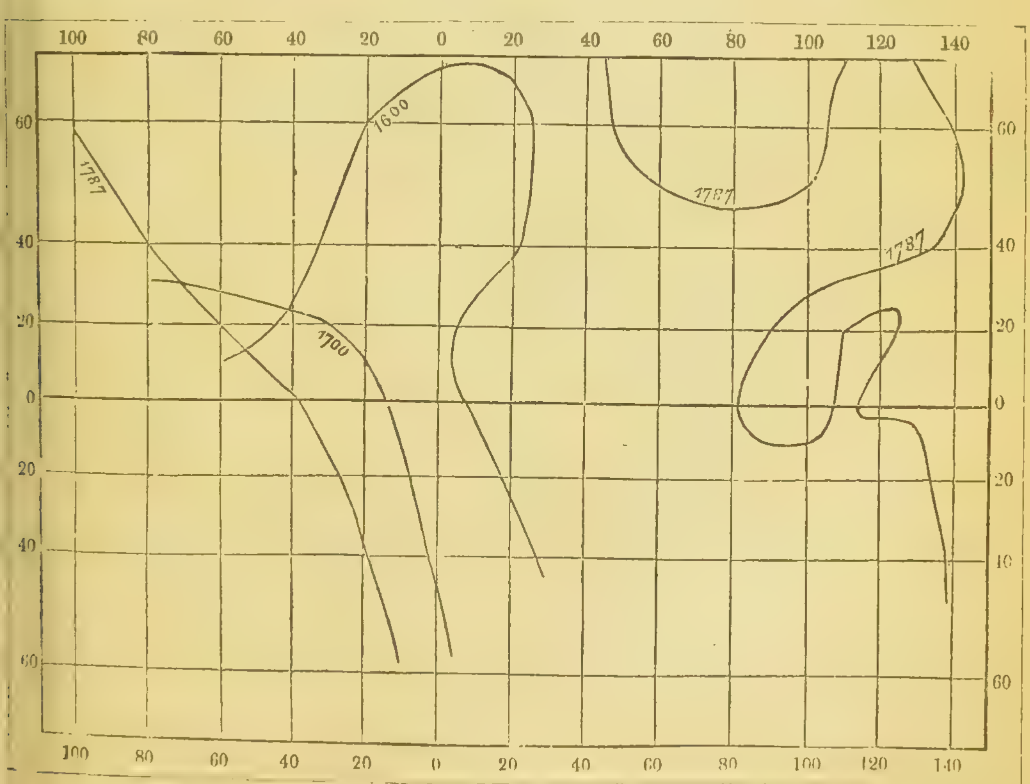

Fig. 3.

as indicated by the line of no variation. The most cursory glance at this cut, will show that grand changes have been proceeding, and that the magnetism of our globe is in nowise permanent. Between 1600 and 1700, for instance, the line of no-declination seems to have undergone a very complex alteration—the double curve of the former date having assumed an apparently simple curve throughout all the space of the Atlantic; but after the year 1700, these changes, although thoroughly pronounced, manifest a certain order or regularity—the line in question gradually proceeding towards the west. On the supposition of the existence of *four poles* or points on the earth's surface towards which the Magnetic Needle is effectively directed, Hansteen has attempted to co-ordinate and account for the foregoing as well as their cognate changes. In earliest times, it was fancied that the earth was one magnet, or that its magnetic effects might be explained by the supposition that a strong magnet passes through its centre, not along the line of its polar axis. Halley first alleged that *on the ground of this mode of viewing phenomena*, we must imagine two great intense magnets or magnetic axes; and Hansteen superadded the idea, that the poles of these magnetic axes are not fixed, but perform a motion of revolution around the geographical poles of the globe. To the Northern Poles, for instance, he gave a period of revolution of 1,740 and 860 years respectively; and to the corresponding southern poles, periods of 4,609 and 1,304 years. Coupled with hypotheses concerning the relative intensity of these various supposed poles, the *theory* or rather *supposition* now offered, went in so far to comprehend the variations indicated on such maps as the foregoing: nevertheless it is sufficiently clear that, as a *physical* hypothesis, there is no basis for it whatsoever. As we shall see below, all notions having the conception of four poles as their basis, have been utterly discredited by the physical investigations of the illustrious Gauss; nor did Hansteen himself imagine that the arduous question, as to the nature of Terrestrial Magnetism, could be resolved by any scheme so artificial.

II. *The Variations.*—It is clear that the phase of the Telluric phenomenon now specified, is that one regarding which the greatest amount of information should be expected from recent personal and governmental activity. Secular changes—especially their laws—cannot be detected in one generation. But co-ordinated observations during even a few years at frequent diurnal intervals may suffice to detect the laws of the variations, and to point through these, to the proximate, if not the ultimate external cause of the magnetic phenomena of our globe. It cannot be alleged even now however, that the Laws of such Variations are absolutely determined. But already it is in our power to offer an approximation; and this we shall do by reprinting the Memoirs of Professor Secchi of the Collegio Romano, to whom, perhaps, as much as any other modern inquirer, we are indebted for fertile suggestions on this remarkable subject. Professor Secchi's papers have appeared in the London and Edinburgh *Philosophical Magazine.*

(1.) *On the Diurnal and Annual Variations of the Declination.*—The following is our *first Law.*—"The diurnal variations of the magnetic needle follow local time."—The first discoverers of the diurnal variations of the magnetic declination suspected that the needle followed the course of the sun, and therefore the true (or apparent) time of the place of observation; but when it was afterwards found, by comparative observations, that there were cotemporaneous variations at many different places, it was suspected that there might be simultaneity of perturbation throughout the globe. When however places of observation sufficiently distant were multiplied, it was found that the ordinary, or diurnal variations, followed in their march the hours of local time, and that even the extraordinary variations, as we shall see in the appropriate place, were not completely excluded from the operation of this law. To avoid speaking equivocally, however, the term "distance" must be understood in relation to the subject of which we are treating. The extent even of the whole of Europe, and still more distances of six or seven hundred miles, are very small compared to the entire circumference of the globe. In the same manner that many meteorological vicissitudes may be simultaneous for such extents, so may also the magnetic perturbations which might be produced by them; but as it can rarely happen that meteorological causes occupy the whole surface of the earth, so simultaneous perturbations produced by them and extending over the whole globe would be equally rare. In fact, if we inspect the magnetic curves, traced in Göttingen time, for Göttingen and Prague in Europe, and for places situated in Canada and the United States of America, we shall find that the places in each continent commonly agree very well with each other; but that agreement between the continents is seldom found, although their distance apart is not great compared to the whole globe. It is necessary, however, to discriminate accurately between two kinds of periodical variations; those which strictly follow local time, and those which in their periods occur at the same moment of absolute time at different stations. We shall speak of the latter subsequently; but in respect to the former, let it be regarded as fixed that local time is to be alone considered, and that if Göttingen time was at first adopted for all the observatories in common, it was for the sake of making out the law of the extraordinary movements and facilitating their calculation, rather than for the purpose of recognizing the law of the diurnal variations of which we are now speaking. It would indeed have been desirable to have adopted *true* or *apparent* local solar time, instead of mean time, in the observa

·tions, or at least in the reductions. The use of mean Göttingen time, besides the inconvenience of requiring the equation of time to be applied, has also another, which is, that it does not often happen that at two distant observatories the observations fall at even hours of local time. This is one of the points (and we shall see others presently) in which the discussion of past observations throws light on the system to be adopted in future. It is to be hoped that future observations will be made at even hours of apparent local time, and that those which have been made will be reduced to such hours. We have however found, and shall demonstrate in the sequel, that the phase of the diurnal oscillations depends more on the position of the sun relatively to the magnetic meridian of a given place (*i. e.*, relatively to the azimuth of the plane of the magnetic meridian) than on the relation to the geographic meridian. *Second Law.*—"The pole of the needle which is least distant from the sun makes a double diurnal excursion, in the following manner:—It is at its maximum of western excursion four or five hours before the sun passes the meridian of the place; it then turns eastward with increasing celerity, of which the maximum occurs near the passage of the sun through the magnetic meridian, and it reaches its limit of eastern excursion one or two hours after the said passage. As the sun declines, the needle returns; and as the sun passes the inferior meridian, there is repeated in the night the same variation as that which took place during the day, but restricted within narrower limits. The limiting hours of these changes vary with the seasons, and are generally earlier in summer and later in winter; and the magnitudes of the excursions are in the proportion of the diurnal to the nocturnal arc."

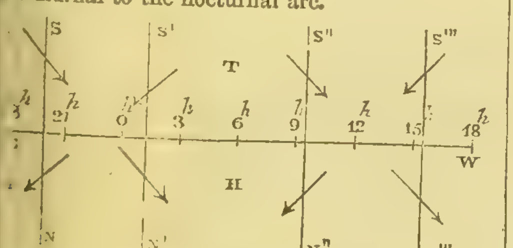

Fig. 4.

The woodcut will make this law better understood. Let E W be the equator or the parallel described by the sun, and T H two places situated in opposite hemispheres as respects the sun; between the hours of 19ʰ and 21ʰ (7 and 9 A.M.) the two needles will be in the positions shown on the line S N; from 1 to 2 P.M., in the position shown on the line S′ N′; between 9 and 10 P.M., as shown on the line S″ N″; and, finally, between 14ʰ and 15ʰ (2 and 3 A.M.), as on the line S‴ N‴. The case here represented is exactly that of Toronto (T), and Hobarton (H). Colonel Sabine describes the diurnal oscillation at Toronto as follows:—"The mean diurnal variation of the declination at Toronto, as derived from the two-hourly observations in 1841 and 1842, consists in an easterly movement of the north end of the magnet from two to ten hours inclusive; a small return movement towards the west then takes place till fourteen hours, when the easterly progression is resumed, and continues until twenty hours, at which time the north end of the magnet reaches its eastern limit. From twenty hours the movement is continuous towards the west until two hours, which is the period of the extreme western limit." (*Toronto Observations*, vol. i., p. 14.) And at Hobarton as follows:—"The north end of the magnet has two eastern and two western elongations or turning-points, at both periods of the year; from October till February the principal eastern elongation is at 2^h, and the minor one at 15^h; from April to August the hours of these turning-points become respectively 3^h and 16^h; from October to February the principal western elongation is between 20^h and 21^h, and the minor one at 11^h; whilst from April to August the corresponding phenomena occur at 22^h and 11^h." Then, comparing the figures which represent these movements with those of Toronto, he concludes that they are identical, only having opposite signs, *except that the turning-points or periods are earlier at Toronto than at Hobarton.* The opposition of these movements is shown in our figure in a manner easily to be remembered. The two stations may be regarded as [within limits, ED.] the type of all that happens out of the torrid zone. Within or near the tropics the law holds good, providing we have regard to the hemisphere in which the sun is, the places being considered as in the southern hemisphere when the sun is in the northern hemisphere, and in the northern hemisphere when he is in the southern. If there should sometimes appear to be an exception, it would be only an apparent one, as we shall soon demonstrate. In the meantime, to facilitate comparisons, we may establish the following:—*Corollary* I.—All the variations are the same in both hemispheres, providing we change the name of the pole influenced; and if we take as the type the north pole and northern hemisphere, we shall have identical variations for the south pole in the southern hemisphere; and the variations of the north pole in the southern hemisphere will be opposite to those in the northern hemisphere. *Remark.*—Perhaps, to avoid any misunderstanding, and the confusion of poles with hemispheres, and for greater convenience in the indication of the antagonistic forces of poles in which the pole *called north* is the *true south* pole of the needle, it might be better to retain the name of *marked* pole, formerly used by some, and especially by English writers, to designate the fundamental pole, to which all is referred, and which *in our* part of the world looks to the *north.* *Corollary* II.—As the points of inflexion of the diurnal curve depend on the sun's passage of the magnetic meridian, it follows that if two places in the

northern hemisphere have opposite magnetic declination, *i. e.*, the one east and the other west declination, the second will be in its phases later [*Qu.* earlier, ED.] than the other. If the two places which we are considering are in opposite hemispheres, this new opposition will have to be taken into account, that is to say, we shall have to make the product of the algebraical signs relatively to their positions and to their names. This rule will be useful to us presently. It is a consequence of the complete antagonism which exists in the two hemispheres relatively to magnetic phenomena. *Corollary* III.—A consequence of the dependence on the magnetic meridian is the advancement or retardation of the phases with the seasons, as in the course of the year the sun arrives at the same azimuth from the geographical meridian by describing a different horary angle, greater in winter and less in summer.—The needle, in its nocturnal oscillation, and especially in winter, makes an excursion which sometimes exceeds the diurnal one. This has sometimes caused it to be believed, that the maximum of deviation, especially of western deviation, was subject to great displacement. But the case is otherwise. The proper maxima of the semi-diurnal excursions always remain at nearly the same hours; but if it should happen that the nocturnal should exceed the diurnal, we are not therefore to say, without qualification, that the maximum occurs in the evening; the times and the periods are to be distinguished, and all will be clear; for if the *absolute* maximum may happen at night, the relative maxima however (eliminating the perturbations) follow constantly the period above enounced; and it is these relative maxima and minima which constitute the characteristic properties of the variations of the magnetic as distinguished from the meteorological period. *Remark.*—The two laws hitherto enounced are themselves no other than corollaries of another more general law, which we will now proceed to expose; but I have thought it well to premise them, and to enunciate them separately, in order to proceed afterwards with greater clearness. The following is this *third Law.* —"The diurnal excursion of the needle is the sum of two distinct excursions, of which the first depends solely on the horary angle, and the second depends besides on the sun's declination. These two fluctuations being variously superimposed upon each other, produce by their interferences all the phenomena of the ordinary diurnal and annual variations."—Nothing is in appearance more bizarre than the curve traced by the magnetic needle in a single day; but as there is no real irregularity in nature, it is natural to presume that the appearance of irregularity only arises from our being ignorant of the fixed periods, as well as of the accidental causes which influence the needle. Desiring to treat this subject with systematic order, it is necessary to restrict ourselves to the regular variations only. In order to give an idea of the annual variations of the declination without too much multiplying words, we will refer to Sabine's work (*Toronto*, vol. ii., p. 20) for the curves traced by him, representing the position of the needle in the two six-monthly periods when the sun is on either side of the equator, at the four observatories of Toronto, St. Helena, Cape of Good Hope, and Hobarton. In these figures the red line indicates the excursions of the needle in the months when the sun is in the northern signs, or in the tropic of Cancer; and the blue line the same in the months when the sun is in the southern signs, or in the tropic of Capricorn. These curves include only the hours of the day, as being the most marked. The north pole of the needle deviates to the east when the curve is above the axis of the abscissæ, and to the west when it is below it. From a simple inspection of these curves, we may draw the following conclusions:—At Toronto, the needle at 8 in the morning is throughout the year to the east of its mean position; and in the afternoon, towards 1 P.M., it is always to the west; 2d, the excursion is greater in summer than in winter, and the annual difference in this respect is represented by the distance between the two curves; 3d, in the intermediate months the needle is between the two limiting curves. For Hobarton we ge the same laws, but with contrary denomination as we have already said (under Law II., Corollar II.). For St. Helena there is the notable circum stance, that the curves are seen to bend alternatel south and north of the equator, moving wi the sun; yet it is not to be overlooked, that tl curve of the months of the June solstice wan the second inflexion, which it would require order to be symmetrical with the curve of t opposite six months. At the Cape of Good Ho the phases are transitional between those of S Helena and Hobarton. These curves are t graphical result of the observations, and we ha now only to see whether it is possible that th may have originated from more simple perio which, being separated from each other, m throw light on the physical cause of the phen menon. These curves are traced by taking t mean of the six months, and hence they app more regular than if taken from the differ months singly; for if we examine each of t constituent monthly curves separately, we sh find some peculiarities and notable differen which tend further to confirm the belief that these curves conceal simple periods, which bei superimposed give complicated results. T such periods being superimposed upon each ot may produce curves of irregular appearance, v not be doubted by any one who may have o seen the multifarious curves, obtained by superimposition of one or two waves, in the li machine invented by Wheatstone for represent the interferences of luminous undulations; an is just the application of these principles to

heory of terrestrial magnetism which reduces hese facts, in themselves highly intricate, to a surprising degree of simplicity. In order to ender more intelligible what we are about to ay, it will not be without its use if we conceive wave of which the elementary curve is the rdinary one of simple sines, having for its quation

$$y = k \sin (x + a),$$

nd a second wave of double period, and of the quation

$$y = k \sin (2x + a).$$

f we superimpose these two forms, we shall ave a figure distinct from either. We here supose the two components to have *equal excursions;* ut by giving different values to the constants hich enter into the curve, we may get the share elonging to the minor diurnal inflexions to be lmost sensibly rectilinear; and *vice versa* we ay have more exaggerated inflexions. In the quations of these curves we shall distinguish the onstants by special names for the sake of brevity nd clearness, calling k the modulus, the arc x e argument, and a the parameter. This being remised, we come to the demonstration of the w which has been enounced, which will be no her than a corollary of the observed facts. nd first, from an extended and comparative alysis of all the magnetic observations, the n is seen to be the principal cause, not only of e diurnal, but also of the annual variations; d we have only to form to ourselves a clear ea of the manner in which it operates. Colonel bine, in vol. ii. of the *Toronto Observations,* p.), briefly sums up the fundamental points, comring the curves which we have cited, and calls tention to two things,—1st, the opposition of e movements of the needle in the two obseratories situated beyond the tropics in the two posite hemispheres (*i. e.,* at Toronto and at obarton); and 2d, the opposite direction inced by the sun's passage of the equator in the clination of the needle at St. Helena and at the pe of Good Hope, which phases place beyond ubt the influence of the sun's declination. He es not, however, proceed farther with the anasis. Now it seemed to me that this germ might considerably more developed, and might beme fertile in very important consequences. It emed to me strange that the sun should act us oppositely by his change of declination in ese two places and not in the others, limiting lf in these last to only diminishing the fluctations. It was added, that the changes at St. lena and at the Cape not having reference to e sun's zenith-distances, his influence ought to due to an astronomical rather than to a geophical and local cause. It may, however, turally be expected that such a period is marked the many convolutions and superimpositions different causes acting on the needle; to excate it was not easy, and would have been ually impossible without the previous labours of Colonel Sabine, which I have happily found sufficient for the purpose.

(2.) *On the Variations of the other Magnetic Elements.*— As already explained, the other magnetic elements, where variations have been directly observed, are the horizontal and vertical components of the intensity, and the dip itself absolutely. We shall discuss these variations in their order.

a. Horizontal Force.—The component which we are now considering is that which is obtained from the bifilar magnetometer, arranged at right angles to the magnetic meridian. The variations may be expressed in the following manner. *General Laws.*—The bifilar magnetometer is subject to a horary variation of a double period, diurnal and semi-diurnal; in the semi-diurnal period the magnitude of the variation depends on the geographical latitude, and is zero at the equator; the phase depends on the angle which the sun makes with the magnetic meridian. We will demonstrate this by steps. Beginning with the stations of middle latitude, there is this simple law; the curve of the bifilar magnetometer is similar to that of the declinometer, but with a retardation of three hours. *Explanation.*—A glance at Colonel Sabine's figures in the second volume of the *Hobarton Observations,* plate 1, p. 5, for the declination, and at plate 4, p. 43, for the component of the bifilar magnetometer, will be sufficient to show, that while the minimum of the declination occurs between 20^h and 21^h, and the maximum at about 2^h, the minimum of the bifilar magnetometer occurs about 23^h, and the maximum between 4^h and 5^h. See also the figures in which this celebrated author makes the comparison between Hobarton and Toronto in the first volume of the *Hobarton Observations.* At p. 34, plate 1, the curves of the declination are shown; and at p. 54, plate 2, figs. 1 and 2, that of the horizontal force; the perfect agreement of the curves will be seen (though the scales of the abscissæ are different), and the same retardation between Hobarton and Toronto which has been already remarked in the declinations. Next let us consider the peculiarities of the equatorial observatories. At St. Helena a singular law holds in the horizontal force. It has a single simple period, and the only indication of a secondary period is that the axis of the abscissæ is not divided by the curve into equal parts, but the diurnal part is less extended than the nocturnal. See the *St. Helena Observations,* p. 30, plate 4, fig. 3. Here, then, the semi-diurnal period vanishes entirely or nearly so, and cannot be compared with that of the declination. But we shall presently see, in the theoretical law, the true explanation of this singular fact, and it will be one of the principal proofs of the theory which we are about to expound. In the variation of this component two very decided periods are evident, the diurnal and the annual. The diurnal maximum occurs be-

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tween 23^h and 0^h, and the minimum at 9^h or 10^h; but in May and June (the winter months), it occurs later, viz., at 11^h. The form of the curve shows a rapid increase and an equally rapid decrease. The annual variation is likewise remarkable for its simplicity, being a perfect curve of sines. See p. 28, plate 2, fig. 2. We shall see farther on, that at Bombay (lat. 18° 53′ N.), the phases approximate to those of St. Helena, except that the secondary periods are more sensible, though not much more so. From the two extreme cases which we have here considered, we may infer what would happen at an intermediate station, like the Cape of Good Hope; that there would be a diurnal and semi-diurnal period, but the latter considerably less developed than at Hobarton and Toronto. Observation confirms this; and fig. 1, p. 40, of the Cape of Good Hope volume shows it at a glance. The period, then, of the horizontal force is the resultant of two periods, the one diurnal, the other semi-diurnal; and the value of the semi-diurnal period is a minimum at the equator, and increases with the geographical latitude. The epochs of the changes depend in this case, too, on the hours at which the sun passes the magnetic meridian, and are somewhat advanced in the summer of the hemisphere and retarded in the winter. If from the middle latitudes we ascend to the pole, we shall find that the curve of the bifilar appears to be in advance of that of the declinometer: this peculiarity, which seems to complicate the law which we have enunciated, depends entirely on the co-efficients with which the diurnal and semi-diurnal period are alternately affected according to the latitude. The following are the laws relative to the annual variation of the diurnal means. So far as regards the monthly means, we have already remarked that at St. Helena there is evidently an annual period depending on the sun's declination; and in order to display the effects of the solar declination in the other observatories, it would be necessary to repeat the analysis made for the magnetic declination. But unfortunately, really perfect observations are as yet few, and hardly sufficient for the seasons. As far as regards Hobarton, the march appears to be not very unlike that of the declination. Thus during the summer, the diurnal variation has its greatest extension, and becomes successively less in spring and autumn, and is at its minimum in the winter. So far as regards the absolute value of this component, it is greater than the annual mean in summer, less in spring and autumn, and a minimum in the winter. It would not be difficult to show, from the nature of this curve, that here, too, is to be found the superposition of the two periods depending on the solar declination and on the horary angle, which are added together in summer, and of which one is subtracted from the other in winter. At Toronto the effects are similar in the respective seasons. At St. Helena the absolute maximum of the horizontal force occurs in the months of February, March, and April, and the minimum in August and September. In these variations it is not easy to separate that which is caused by the temperature from that which is strictly the magnetic period,—partly because the temperature exercises an influence on the bars, and if the variations are not accurately corrected there is a danger of error,—and partly, because as the temperature affects the force of all magnets, it may affect that of the earth also. Besides, as the variation of this component depends both on that of the inclination and on that of the total force, it is not easy by means of the horizontal observations alone to determine to which cause each phase must be attributed. A few observations, and those subject to some uncertainty, tend to show that at St. Helena the annual variation of the inclination is small, so that we must consider the variations of the horizontal force as depending entirely on those of the total force. At Makerstoun the values of the horizontal component have their maxima at the solstices, and their minima a little after the equinoxes. In general in this, as in the declination, the months of April and August are marked by the greatest diurnal excursions; this is attribnted by Mr. Broun to the extraordinary perturbations, but it probably depends on some other cause. But for the principal details on the epochs of the maxima and minima, we must refer to the original works, this memoir being already too long.

b. Vertical Component.—This is given by the balance magnetometer arranged at right angles to the magnetic meridian. It follows laws analogous to those of the horizontal force. At St. Helena the curve has a simple period, but with this difference from the curve shown by the bifilar, that the curve of the horizontal force is nearly a curve of cosines (reckoning from noon), and that of the vertical force a curve of sines. But the rudiments of a secondary period are seen in a slight undulation, which it makes at about 10^h, as also in the intervals of the intersection of the curve with the axis being greater from midnight to noon than from noon to midnight. The maximum occurs between 5^h and 6^h, and the minimum at 20^h. Its amount is smaller from October to March, during which period it is retarded, and greater from April to September, when it is in advance. At the Cape of Good Hope the march of the vertical force is so similar to that of the declination, that it is susceptible of the same analyses and gives the same results. In these curves the diurnal period always predominates, though somewhat modified in parts. See Sabine, *Obs. Cape*, p. 40, pl. 5. fig. 2. At Toronto and at Hobarton the usual antagonism displays itself in this component. The double period is developed as much as in the declination, and the hours of minima and maxima are almost identical with those of the declination. The doub

period displays itself at Hobarton in a somewhat singular manner, producing a maximum towards seven in the evening. But to follow out all these rregularities would be very tedious; we shall there- ore abstain, referring the reader to the works already quoted. We shall only add, that the ex- planation of many anomalies is to be found in the ollowing remarks, viz., that as the origin of the periods which are superposed depends on the dif- erent hours at which the sun passes the magnetic meridian, the semi-diurnal period has necessarily various positions with regard to the diurnal; and in this manner various inflections occur which it would be difficult to account for in any other way. For instance, the variations of the inclination given at the Cape of Good Hope, p. 44, pl. 6, can easily be decomposed by an experienced eye nto the usual principal periods, diurnal and semi- diurnal, which gain alternately on each other with varying parameters in the different months. We may therefore finish this discussion with the ollowing general conclusion: that "the horizontal omponent, as well as the vertical, may be de- omposed into a diurnal and semi-diurnal period, which depend on the declination of the sun and n the geographical latitude."

c. Inclination and Total Force.—Given the aws of the variation of the two preceding com- onents, that of the resultant or total force may asily be deduced; and the absolute magnetic clination being known, the variation of the in- ination may also be deduced from the variations the horizontal and vertical force. *General aw.*—"The phases of the inclination are ana- gous to those of the declination, but three hours arlier." *Explanation.*—If the maximum of the eclination is at 2^h, the maximum of the inclina- on would be at 23^h or thereabouts. This may e seen in the Makerstoun curves, &c., and also the Hobarton curves, the explanation of which e will give presently. As for the other pecu- arities, it will be sufficient to remark, without nalyzing each case in detail, that in general the axima of the horizontal force coincide with the inima of the inclination. Colonel Sabine calls tention to the analogy that exists between the riation of the inclination at Hobarton and oronto, places which are almost antipodal. In ese localities the variation of the inclination in th its periods is the same at almost precisely e same hours, with this difference only, that at obarton the south pole (the lower) is to be con- ered, and at Toronto the north (the lower). e total force at Toronto is subject to two riods, viz., the following:—

Principal maximum at..................	5^h
Principal minimum from	15^h to 16^h
Secondary maximum from	18^h to 20^h
Secondary minimum from	22^h to 23^h

cording to this distinguished writer, an ana- ous double period is wanting at Hobarton, l the total force has a simple progression with minimum at 20^h or 21^h, and the maximum between^h and 6^h, the intermediate march being continued without interruption. But on carefully examining the curves themselves, given by him at p. 58, pl. 3, it will be seen that the simple period exists only in appearance, and that in certain months the secondary period is very ob- vious, and rudiments of it exist in all, though to a very small extent. This difference is certainly owing to the great difference of latitude and mag- netic force between the two places. There are not yet a sufficiency of published observations at St. Helena to determine this law; but a copious series of observations at the Cape show a period in the variations of the total force almost com- plementary to that of the declination. The simi- larity of the two kinds of curves, which generally differ about three hours in their phases, renders a more complete analysis unnecessary.

d. Complex Period of the Needle.—The pro- cess which we have described for determining the motions of the needle consists in a series of decompositions of the forces, rendered necessary from the mode in which the magnetic bars are supported. The laws of the variations of the components being determined, we may deduce from them what would be the motion of a needle, not on an axis, but suspended by a *single point*, which would be its centre of gravity, and free to obey every magnetic variation in whatever direc- tion it took place. To give an idea of the com- bined motions which the needle makes in a complete oscillation, we may refer to two figures in particular in Sabine's plate, *Hobarton Obser- vations*, vol. i., pl. 3, the first of which belongs to December at Hobarton, the other to June at the same place. The principle upon which these figures are traced is the following:—The point where the two axes intersect represents the mean diurnal position of the needle in declination as well as in inclination. Along the horizontal axis a distance is taken representing the variation of the declination for a given hour, and from the point so obtained, an ordinate is erected representing on the same scale the variation of the inclination for that hour. Thus are obtained the figures to which we have referred. In Colonel Sabine's plates, he has given the curves for each month of the year, and they are all extremely instructive, but the two to which we have particularly re- ferred, the march of the curve in the two extreme months of the year, is well shown. It may be seen from these, and still better from the whole series, that the oscillation of the needle has always a double period, diurnal and nocturnal, but their respective lengths vary with the season. The diurnal period, considerable in summer, is con- tracted in winter, and the nocturnal period, short and hardly discernible in summer, is greatly de- veloped in winter; in this will be seen the fact, elsewhere noticed, that the absolute nocturnal minimum is greater than the diurnal minimum, and hence we see the cause of the error of those who consider that there is a single period in winter,

But two things are particularly to be observed in these curves. 1st, The nocturnal loop is always in diametrical opposition to the point of noon; from this it appears that the phases succeed each other near the lower meridian with the same march as near the upper. 2d, That the magnitude of each loop in the opposite seasons, diurnal as well as nocturnal, is in a constant proportion to the one diametrically opposed to it, viz., between $\frac{1}{7}$th and $\frac{1}{6}$th; thus, for example, the loop of the diurnal curve for December becoming the nocturnal loop in June, is diminished to about $\frac{1}{6}$th. In like manner the diurnal loop of June, when it becomes the nocturnal loop in December, is diminished to about $\frac{1}{6}$th. This constant proportion, which is observed in all the months, must not be overlooked; and physically considered, it must depend on the manner in which the influence of the solar magnetism operates across the earth. 3d, The appearance of these curves is that which would arise from the superposition of two circular spirals with different moduli, the one having a simple, the other a double period. The curves which are seen in Wheatstone's undulation machine, when two spirals are superposed, the one half the length of the other (in which case the projection of the resultant at right angles to the axis of the spirals forms a kind of *c*), are evidently of the same kind as the present. Mr. Broun has given analogous curves for Makerstoun, and a glance at these, as in the case of Hobarton, will show the same law, though somewhat more complicated from having grouped too many months together, and from the higher latitude and more frequent disturbances. Among the points to be remarked in these curves is the following:—"Tracing in them the direction of the magnetic meridian (that is to say, noting the hour at which the sun passes it), it is seen that the greatest velocity of the needle occurs when the sun passes through this plane, and that the centre of the nocturnal loop is to be found in the same line, or very near to it, and that the movements of the needle in inclination are complementary (but with a distance of 3^h) to those of the declination." From these facts we conclude that "a perfectly free needle would describe during the day a species of double spiral produced by a compound circular motion having two periods, the one diurnal, and the other semi-diurnal; or of two periods, the one while the sun is above the horizon, and the other while it is below; the excursions of which are in the proportion that the diurnal arcs bear to the nocturnal, and have for principal axis the local magnetic meridian." Lastly, there is a fundamental characteristic of all the principal elementary periods, which consists in the *maximum and minimum being about six hours distant from one another.*

e. Total Force.—Colonel Sabine has investigated whether the maxima and minima of the total force vary during the year; from the discussion of the observations at Hobarton and Toronto he has arrived at the conclusion that the total force has its maximum in the months of December and January in both hemispheres, although these correspond to opposite seasons. Such a law corresponds too nearly with the change of the distance of the earth from the sun to admit of our doubting that it depends upon it. We have then that "*the disturbing force of the sun increases as its distance from us diminishes, and does not depend on the temperature of the seasons.*" The exact determinations of this force hitherto obtained are too few to enable us to get out the rigorous expression of this law, that is, whether it is inversely as the square of the distance; but the fact appears to be established, and will perhaps be brought out by the discussion of other observations, particularly if care be taken to eliminate from them the periodical changes which depend on the seasons, and the secular changes.

III. *Irregular Changes and General Reflections.*—We shall now briefly discuss the different hypotheses which have been proposed to account for the diurnal magnetic period; taking the opportunity of referring to the *extraordinary variations.* Justice must be done to the enlightened spirit of modern physicists, who, intent on the study of facts and their laws, care little to construct hypotheses; from this it arises, that whatever has been proposed has been rather by way of conjecture than with any real endeavour to establish a theory. We, too, in the same spirit, and merely for the purpose if possible of combining facts, have supposed the sun to act as a great magnet. The explanations hitherto proposed may be reduced either to thermo-electric currents induced by the sun in the different strata of the earth, or to the electricity developed in the meteorological changes of which the sun is the principal cause. A single reflection seems to exclude these from being *principal* causes of the magnetic diurnal period. The characteristic fact, as we have already noticed, is that the magnetic elements have a double period, diurnal and nocturnal. Now temperature and the other causes suggested have a simple period, with greater or less modifications it is true, but not so constantly repeated when the sun is below the horizon as the magnetic period is in all climates and in all seasons. This appears to us a law peculiarly characteristic of magnetism, which shows it to be essentially distinct in cause, and to have a separate origin from meteorological phenomena; in the same way that the semi-diurnal lunar period in the tides is a proof of a special action of our satellite on the waters of the ocean, which is universal gravitation. And just as the fact that the tide is more or less retarded after the moon passes the meridian, is not a sufficient objection to destroy belief in this cause, so some irregularities of a like nature observed in the magnetic period will not be sufficient to disprove the reality of the magnetic action, if we believe the pro

iduced to be otherwise sufficient. What has ed some to consider the magnetic period as simple one, has been, seeing that in certain seaons the extreme minima occur at night. The rror arises from not distinguishing the absolute rom the relative maxima; but these are the rue characteristics of the phenomenon, and ought o be looked upon as decisive in the matter. To his proof in support of the solar magnetic theory, nay be added another, already noticed by Col. abine, and worked out by us in a former part f this Memoir, viz., the opposite action of the m according to its declination, the inversion ccurring exactly at the epoch of the equinoxes; d here another difference will be seen between e effects of thermical and meteorological causes, d the magnetic effect of the sun. The former not reach their extremes for a considerable me after the corresponding astronomical phases, hile the latter have an almost exact coincidence ith them. We do not pretend, however, that ere are not considerable difficulties in the way this hypothesis; and although it explains very ell certain very singular facts,—as, for example, e interval of six hours between the diurnal axima and minima, a fact the explanation of ich has never, as far as I am aware, been even empted on any other hypothesis, and which t is so marked in all the magnetic variations the mean latitudes; also the singular exception ich it suffers at the equator, becoming simple the horizontal and for the vertical components, d various other points,—yet we must confess t there are some irregularities which our mulæ do not explain. Of this nature is the t, that at St. Helena, and generally under equator, the period for the declination of the dle appears to be rather eight hours than lve, so that it presents sometimes three xima. Without repeating here what we have l elsewhere in general terms, viz., that these iods may find their explanation in those terms he formulæ which we have neglected, we may that this fact may simply depend on the conration and nature of the ground near to the es of observation. Thus, for example, at St. ena, an island situated in the midst of the antic, and entirely volcanic, the distribution nagnetism must be very different from what s at a place in the interior of a continent; we know, in fact, that the isogonal lines dly change their direction in passing from to continents, and this explanation may also ly to equatorial stations near the coast. We w, too, how much the vicinity of magnetic es may influence the diurnal variation of the lle. This explanation seems to be confirmed he fact that the diurnal curves derived from declination at St. Helena resemble much more ly those of other countries than the annual nal mean. In fact, the latter is exclusively to the horary angle, and therefore more strictly ndent on the distribution of the earth's magnetism round the place of observation. It may also be said that the small maxima in the morning and evening, which are in truth for the most part only indicated, are only a portion of the regular period cut short midway by the discontinuity introduced by the passage of the sun from above to below the horizon, as we have elsewhere remarked in regard to the periods observed towards evening in high latitudes. For these reasons we have urged that a complete explanation of the phenomenon depends on the law of the distribution of magnetism on the globe. It may not be useless to state here what is habitually observed at Bombay, that being a place situated to the north of the equator, and in a latitude not very dissimilar in amount to St. Helena, the lat. being $18^\circ\ 53'\ 30''$ N. From the observations made at this place and reduced by Mr. Montriou, an oscillation results analogous to that of other countries; having an eastern maximum a little before 8^h, and a minimum between noon and 1 P.M. Besides which, there are two other small oscillations, one near sunrise, the other near sunset; it is evident that this is the nocturnal period interrupted by the interposition of the earth. During the night the needle has a very small oscillation. The horizontal force has a simple period, but with disturbances which indicate the commencement of a secondary period; the vertical force shows a tendency to a like period. When the observations have been continued for a greater number of years, more certain results will be obtained. See *Obs. Magn.* and *Meteor.*, at the Obs. of Bombay for the year 1847, part 1, p. 493, and plate 1. The editor of the *Bombay Observations* then concludes:—"The presence of the sun seems to produce great magnetic variations in the day-time, and it is otherwise manifest that it is not on account of the heat only of that body; for if this was the case, the curves of the temperature would be similar to the magnetic curves. Besides, the presence of the sun begins to be felt two hours before sunrise, and lasts almost as long after sunset, so that the solar magnetic influence appears to be quite independent of the temperature of the place." But we are very far from denying that meteorological causes may often affect the needle; we know that every meteorological change is accompanied by a change more or less marked in the vapour of the atmosphere, and therefore by a development of electricity. But when the needle is usually seen to complete its regular oscillation tranquilly in the midst of the most violent storms, and during tempests loaded with electricity, with tremendous thunder and lightning, it may well be asked, *What are the conditions under which electricity must develop itself in order to affect the needle?* That such an action does exist, however, appears to be proved by the fact, that in our climates the needle performs its oscillation with the greatest regularity during calm and serene days, and that on change of weather this regularity is invariably disturbed.

We have, in proof of this, a year's observations at Rome: and it would be well to discuss magnetic observations more from the meteorological point of view than has hitherto been done. From the few observations which we have made, it appears that light and passing overcloudings have more effect on the needle than tempests themselves. At Rome the perturbations exhibit themselves in that particular state of the atmosphere in which there are slightly phosphorescent clouds having at night the appearance of the rudiments of the aurora borealis. This fact was observed by us a second time on the evening of the 27th of July. We were making some observations on stars in the meridian, when towards half-past nine we were interrupted by a slight overclouding coming from the north; a very rare occurrence, since generally with us the sky begins to be overcast in the south-west. While we were waiting for it to clear, the cloud appeared slightly luminous at the edges, so that there seemed to be a diffusion of the milky way in unwonted parts of the heavens. Soon after this it cleared, and the observations were continued; but almost immediately the same overclouding recommenced with the same luminous appearance. I then remembered the fact observed before, that a similar state of the atmosphere had been accompanied by magnetic perturbations, and on going to look at the magnetometer I found it more than 20 divisions (about $7\frac{1}{2}'$) out of its usual position, and the regular observation made at 9·35 had been marked by the observer as being an extraordinary one for that hour. This was the more striking from the needle having performed its diurnal oscillation with the greatest regularity during the whole of the preceding season. This was, without doubt, a phenomenon of the kind which accompanies the aurora borealis. But it may be asked, was the condensation of vapours the cause, or the effect, of the perturbation? It is generally considered to be most probable that the perturbation is the effect; but is this certain? M. De la Rive has proposed, in his Memoir on the Aurora Borealis, a theory which accounts with some felicity for the effects of atmospheric electricity on the needle; but it may be doubted whether this cause is sufficient to explain all the facts to which the author would apply it. An accurate study of the laws to which the extraordinary perturbations of the needle are subject, combined with the study of the aurora borealis, can alone throw light on this question. All that we know with certainty on this subject is, like so much besides, due to Colonel Sabine. He has collected the principal results at which he has arrived from the discussion of the *Hobarton and Toronto Observations* in a memoir inserted in the *Philosophical Transactions* (March, 1852), from which we will give a short extract, as well to complete the exposition of the laws of the magnetic changes, as to obtain some light for guidance in future researches. A comparison of the Toronto and Hobarton observations establishes, that even the extraordinary perturbations, though occurring at all hours of the day, yet when taken in a mass, have a regular period, which depends on the local time, and have opposite directions in the opposite hemispheres; so that the perturbations which cause an easterly deviation at Toronto, cause a westerly deviation at Hobarton, in conformity with the complete magnetic antagonism at the two stations. This fact is elicited without difficulty from the coincidence of the perturbations observed at the two places in the same day, with a difference in local time corresponding to their difference in longitude. The general result is, that easterly perturbations at Toronto and westerly perturbations at Hobarton, have their minimum in number and magnitude during the day and their maximum during the night. This maximum occurs at Hobarton between 10^h and 11^h, and at Toronto at 9^h. This difference in time, as we have already remarked, occurs in all the other magnetic changes. The minimum occurs at Hobarton between 5 and 6 A.M., and at Toronto between 2 and 3 P.M. The easterly perturbations at Hobarton and the westerly at Toronto, have a distinct period. Their maximum at Toronto is at 5 A.M., and at Hobarton at 6 A.M.; the minimum at Toronto is between 9 and 10 P.M., and at Hobarton at 10 P.M. Taking the perturbations in mass, and laying down the curve representing their mean effect on the curve of the diurnal oscillation of the needle, the following law is elicited:—"The morning perturbations tend to diminish the ordinary excursion of the local period, and the evening ones to augment it." This law may be enunciated in another way. "The pole which is turned to the sun is by the mean effect of the perturbations moved towards the east from 5 A.M. to 5 P.M. About 6 A.M. and 6 P.M. it passes zero, and the rest of the day is moved to the west. The maximum movement in the morning is at about 7, and in the evening at about 9. In both places a secondary minimum towards the west is observed at noon." In other respects the curves are tolerably regular and of the usual form; but at Hobarton the principal maximum and minimum are less marked than at Toronto; the march of the two curves is in direct opposition at the two stations. These conclusions agree with those of Mr. Broun at Makerstoun, as may be seen in the results for 1846, p. 87, plate 1. The following are the results with respect to the frequency and magnitude of the perturbations in the different months of the year:—"The mean value of a perturbation is a maximum in the equinoctial months, less in the winter months, and a minimum in the summer months." At Hobarton the difference between the summer and the equinoctial months is scarcely perceptible. The proportions of the frequency and of the magnitude of the perturbations in each month relatively to the sum of those observed in a year

come out a minimum in the winter months, a maximum in the equinoctial months, and intermediate in the summer months. These conclu-sions however, may be somewhat varied by the use of different systems of reduction, depending chiefly on the definition of an extraordinary perturbation. This is not the case in the laws of the perturbations taken with reference to the day, because in that case all the methods of reduction bring out the same result. Thus the same result is seen in the curves given by Mr. Broun, whose method of reduction is different from that used by Colonel Sabine. But a most singular fact, which has been discovered in these researches, is the start which the mean annual values of the perturbations take between the years 1845 and 1846, when they are almost doubled. This being a fact of the greatest importance, Colonel Sabine has endeavoured to place it beyond doubt by the best possible proofs. The following is the table given by him (p. 115):—

Year.	Ratios of the number of perturbations.	Ratios of the aggregate values.
1843	0·60	0·52
1844	0·78	0·78
1845	0·72	0 65
1846	1·20	1·15
1847	1·28	1·42
1848	1·43	1·52

he circumstance of the last three years having atios almost double of the first three, appears not be accidental, particularly when we observe hat in the two observatories, which are almost ntipodal, the same fact appears, and that during l the six years the same instruments were em-oyed. Besides, in the same years the observed urnal excursions of the declination, of the incli-ation, and of the total force, have sensibly in-reased beyond the limit of any probable error. astly, the same fact, in regard to the diurnal cursion of the declination, results from the servations of Dr. Lamont at Munich in Ba-aria. Further observations may throw much ght on this subject. For the present we can ly say with Colonel Sabine, that so general a ange in the march of all the magnetic elements mands a proportionate cause; and that as these not arise from the ordinary effects of the mate, which in these years have exhibited no traordinary change, it is necessary to seek me other cause. Colonel Sabine also points t the singular coincidence between the years of maximum and minimum of these magnetic anges and those of the maximum and mini-m number of the solar spots, observed by hwabe in these years. This number was a nimum in 1833 and 1843, and a maximum in 28, 1837, and 1848. From these and from er observations, Wolf has deduced a decennial iod in the changes of these spots; and it re-ins to investigate whether a like march can be ced in the older magnetic observations. The blication of Arago's observations has come op-tunely for this comparison. From the table at p. 500-501, vol. i. of his Scientific Works, the declination needle appears to have had a minimum excursion in 1823 and 1824. Before that it was greater, and having reached this minimum, it increased continuously until it arrived at a maximum in 1829. These epochs correspond with those derived from the period observed in the solar spots, which was a maximum in 1828 and a minimum in 1823. From the Göttingen observations, we find a maximum in the excursions of the declination needle in 1836-37. This maximum also coincides with a maximum of the solar spots in Schwabe's table. Hence Colonel Sabine thinks it not impossible that changes in the solar atmosphere may extend their influence to the earth in the form of magnetic action. The truth is, that to consider the whole complexity of magnetic perturbations as a mere meteorological effect, appears to be assigning to them a cause not adequate to the effect. The fact mentioned above, that the maxima of the perturbations at Hobarton succeed each other with the same retardation as the other magnetic phases, is one which cannot be explained either by the retardation of the effect of temperatures, or by the condensation of vapour. We cannot conceive how these should account for the general retardation of one hour. It is then a purely magnetic fact, the explanation of which depends on that of the physical cause of solar and terrestrial magnetism. The same may be said of the greater perturbations at the epochs of the equinoxes, which certainly bear no relation to the state of the atmosphere or to the solar heat. Colonel Sabine makes the acute observation, that the coincidence of the solar spots with the maximum of the perturbations demands a cosmical cause, depending on that body. We may be permitted to refer here to the hypothesis of Mairan on the solar atmosphere, and on its relation to the zodiacal light and the aurora borealis, and therefore to the magnetic perturbations. We are far from admitting the theory as proved, since it appears impossible to admit that the solar atmosphere extends so far as half the radius of the orbit of Mercury, whence it is rather to be inferred that the zodiacal light depends on a nebulous ring circulating round the sun between Venus and the Earth. But whatever hypothesis be adopted, there are various coincidences which may be deserving of regard. Mairan had, even in his day, remarked the greater frequency of the aurora borealis at the equinoxes, the epochs at which the zodiacal light is most visible. Neither had the relation between the greater frequency of the aurora borealis and the epochs of the greater solar spots escaped him, a relation already remarked by Cassini. The paucity of observations at that time permitted suggestions to be made which have been since proved to be groundless; but in general such coincidences are worthy of consideration. Modern observations of the eclipses of the sun, of the protuberances and of the corona, as also of the

spots, of the temperature of various parts of the disc, as well as photographic impressions, have placed beyond doubt the existence of the solar atmosphere even beyond the zodiacal light. In reading Mairan, one cannot help seeing the serious difficulty which he finds in explaining why the auroras have their maximum at the equinoxes, and not at the epochs at which the earth passes through the nodes of the solar atmosphere; but do we truly know the place of the nodes of the zodiacal light? He assumes that they are the same as those of the solar equator, but this is not proved; and if the zodiacal light constitutes a ring, it might well be otherwise. On the magnetic hypothesis, the greater frequency of the aurora at the epochs of the equinoxes would have relation *to the position of the poles of the sun with reference to the earth*, these poles being in fact more directed towards the earth at the equinoxes, and being more or less oblique to it at other times. Those who hold the theory of the production of electricity by vapours, may say that these become rarefied in the morning and condensed in the evening; and hence may arise opposite electric states, the fluid passing in the morning from the earth to the atmosphere, and in the evening from the atmosphere to the earth. This may be true; but why should this condensation always take place at nine in the evening? The hygrometric curves of the different months show at all events a variation in the hour of maximum according to the seasons. A hypothesis, however, can be found which would conciliate the various facts, viz., that atmospheric changes may generate electricity, but that the direction of the current, which of itself would be indeterminate, may be determined by the magnetic action of the sun. But to expand this further into a hypothesis would be at present premature. We shall only say that it is not improbable that the earth is subject to the magnetic action of the sun in a manner unknown to us; but now that magnetic phenomena are developing themselves under so many aspects, we may hope that the explanation of these mysterious actions will not be withheld. Not only magnetism, but diamagnetism also may co-operate, and still more the induced currents which exist in bodies of every kind. Two things only I wish to notice. First, the value assigned by Gauss to the magnetism of a cubic metre of the earth, is such as to make one believe that the whole mass of the earth is really magnetic, and that this force results not only from ferruginous substances, but from the whole globe itself. He proves, in fact, that the eighth part of a cubic metre of the earth has a magnetic moment equal to that which is possessed by a bar of steel 1 lb. in weight and 30 centims. in length, magnetized to saturation. He justly observes, that such a result must surprise physicists, and that it would require 8,464 trillions of such bars to represent in space the magnetic force of the earth! The other is, that magnetism may act upon bodies in a manner quite surprising, and of which we are very far from forming an idea before seeing its effects. The marvellous experiment performed with Ruhmkorff's apparatus, in which a cube of brass, two centims. in the side, rotating with the greatest rapidity, is struck motionless, if I may use the expression, by an invisible force at the moment of the completion of the circuit of the great electrical magnet between the poles of which it is situated, and without being drawn to one side or the other, remains there fixed, in spite of the powerful torsion of the wire which tends to cause it to rotate,—and resumes its rapid motion when the current ceases,—proves that non-magnetic bodies in motion may, under the influence of a magnet, give rise to phenomena of the most mysterious nature. An action of this kind must take place between the earth and the sun, and thus perhaps may be explained some of those anomalies which still present no small difficulties to every theory which is proposed. —The matters now discussed at length may be summed up as follows:—1. The action of the sun upon the needle is opposite, according as the sun is north or south of the equator.—2. The action of the sun on the declination needle has a period, in part but not entirely, analogous to that of the temperature and of the annual and diurnal meteorological changes.—3. The periods of the horizontal and vertical components, following the law of the geographical latitude, and occurring at hours wholly different from the variations of temperature, show a different origin from these. Therefore, if the coincidence in time, in the variations of the temperature and declination, have contributed to the belief of the existence between these two of a mutual relation of cause and effect, the study of the other components makes this coincidence disappear, and therefore destroys every foundation of the hypothesis.—4. All the phenomena hitherto known of the diurnal magnetic variations may be explained by supposing *that the sun acts upon the earth as a very powerful magnet at a great distance.*"

IV.

General Theory of Terrestrial Magnetism.—The mass of results presented above sufficiently manifests how wide and important the subject of Terrestrial Magnetism has now become. Nor can the inference be missed, that with all we have learnt, the time has not arrived for final conclusions as to the seats and modes of those actions, which are expressed by the habitudes of the Free Needle. Those are evidently *cosmical.* It appears almost established by Bessel (see Comets), that the forms of cometic bodies are due largely if not entirely to the energy of a *Polar Force* originating in the Sun; and it cannot be doubted that to the magnetic power of this great orb, the phenomena of Terrestrial Magnetism are mainly owing. It has been rendered probable, too, especially by the Makerstoun

observations, that our satellite plays a certain part in reference to the oscillations of the Needle; so that for any complete theory we must be satisfied to wait.—But there is another and simpler view that may be taken of the theoretical part of the problem of Terrestrial Magnetism. It is one thing to speculate regarding Physical Causes; but usually, it is far more within our reach to determine the abstract laws of a phenomenon, irrespective of hypotheses regarding its Physical Cause. The method of procedure in science is a mixed one, and determined in each case by circumstances. Without the substitution of the physical hypothesis of undulations, we should have made but poor way in Optics: on the other hand, Newton's discovery of the Law of Universal Gravitation, had not, even in his mind, any relation whatsoever to those physical speculations as to the origin of gravitation, in which it appears that he was fain at times to indulge. The Newton of Terrestrial Magnetism, is unquestionably Gauss;—not that he has definitively lighted on ultimate laws, but that he has used Newton's purest method, and evidently come very near those Laws: nor can it be questioned that during the later years of his life he had no rival, nor since his death any adequate successor,—(exception taken of our own Sir W. R. Hamilton)—in the power to pierce, by analytic energy and skill, to the root of a class of cognate but apparently diverse phenomena. Much as Gauss has accomplished, his greatness will not be known, until we obtain those posthumous fragments promised by Lejèune Dirichlet. France has recently collected and republished all the works of her Laplace: why should not Hanover similarly honour herself by offering to the scientific world an accessible collection of the equally remarkable efforts of Gauss? Previous to the writings of the illustrious mathematician of Göttingen, all physicists had proceeded to explain the more general phenomena of Terrestrial Magnetism—and these only —by aid of a physical sub-sumption. Gilbert fancied the earth a magnetic bar with specific poles; Halley claimed two magnets with four corresponding poles; Euler reclaimed for Gilbert; Hansteen reverted to Halley,—correcting his specific views, by aid of more abundant facts; Barlow and others have taken as their foundation the idea of *Thermo-Electric Currents*, and with a success sufficient to manifest that the physical cause or ground-work on which they build is by no means a hollow or fantastic one.—But all these speculations were extremely general. Sometimes, as in the case of Hansteen's rotating poles, it was an *ignotum per ignotius:* in other cases, the *minutiæ* were not explained, and we were thrown back on theories and hypotheses quite as puzzling in themselves as the phenomena of Terrestrial Magnetism. Gauss freed himself, *ab initio.* from all speculation. "Certain definite facts regarding the Magnetic Force of the earth have been determined by observation. It matters not whether that is an inherent Force or an induced Force; the law of the decrease of the Energy of that force in relation to distance, is as well known as the Law of Gravitation:—how, then, can we represent the *resultant* of the magnetic influences of all the atoms of such a globe as the earth, whether these influences be induced or innate? In encountering such a problem, Gauss undertook—(what had never been laid before)—to show an abstract or necessary ground, irrespective of hypotheses, for the *mean conditions* of all the Magnetic Elements, viz.: *Declination*, *Inclination*, and *Intensity*. Of the memoir of the distinguished philosopher of Göttingen we can give but the briefest sketch. Recognizing it as possible, at the outset, that the causes of the irregular and momentary changes of the Magnetic Needle, may be either external or internal, Gauss remarks that they and the energies that produce them are exceedingly small in comparison with phenomena due to the general directive Magnetic Force of the Earth; and that this directive Force itself is a force essentially represented and defined by the *Mean Values*. But such a general force, or the Terrestrial Magnetic force as thus manifested, must be the mere *resultant* or collective action of all the magnetized particles of the earth's mass. No matter what the physical cause of magnetism—whether that be a separation of magnetic fluids, or an arrangement of Galvanic Currents—it is enough that it is a *polarity* characterized by equivalent repulsions and attractions varying inversely as the square of the distance. Suppose, then, that the whole volume of the earth is divided into infinitely small elements—and that each element contains a quantity of free magnetism, it is evident that the total force of the earth must be expressible by some definite formula or function. Unfortunately the law of the distribution of magnetism within the earth is utterly unknown; that is, we cannot connect the quantity of magnetism in any of the foregoing infinitely small elements, with the position of that element. The form of the *Potential Function* is on this account quite undetermined and indeterminable: but Gauss, by an exercise of extraordinary sagacity and skill, demonstrated, that whatever its form, the function in question must—because of the conditions it is required to satisfy—enjoy certain properties that in themselves are prolific in positive results. For instance, after restricting the term "Magnetic Pole" to those places of the earth where the horizontal intensity vanishes, and where the dip is 90°, he shows that there cannot—under any consistency with actual phenomena—be more than ONE *North Pole* and ONE *South Pole.* And he deduces, further, the two following remarkable theorems;—1. *If we know the component of the horizontal magnetic force directed towards the north for the whole surface of the earth, then the component towards the west or towards the east follows of itself:* and, 2d.

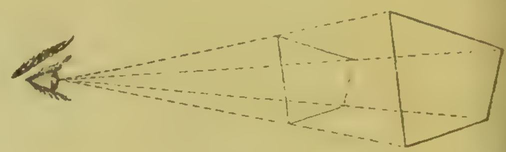
Almost *e converso, If we know the component of the horizontal magnetic force in the direction towards the west for the whole of the earth's surface*, and *the component towards the north for all points of some one line extending from the north pole to the south pole, the latter component for the whole of the earth's surface follows of itself.* This same knowledge of the potential function furnished by the horizontal component for all points of the earth's surface, Gauss likewise showed to be sufficient to give its value for all points of external space. But the portion of this remarkable memoir which excited the most sanguine hopes as to the ultimate and full solution of the great problem of terrestrial magnetism, was those successive theorems, in which it seemed demonstrated that the component of the magnetic force of the earth for *any point* of the earth's surface may be obtained by combining with given functions of the latitude and longitude of that point, certain constant co-efficients—not probably exceeding twenty-four in number—that could be deduced from a *sufficient number* of the observed values of those components *in different and assigned localities.* The calculations that require to be accomplished for the verification or rather the realization of Gauss's formulæ are exceedingly laborious, but with the aid of Weber, and a few assistants, he undertook them; and the actual conclusions are embodied in his well-known *Atlas des Erdmagnetismus.* The labour mainly consists in the calculation of the *co-efficients* (now universally named the *Gaussian co-efficients*); and this was again undertaken and greatly extended, by Petersen and the younger Erman, at the expense of the British Association. The results are not at present, in all cases, quite accordant with observation; but whether such discrepancies spring from defects in the theory, or from the limited number yet existing of thoroughly accurate experimental determinations, must be regarded an unsettled point. This much, however, is certain,—the foundations of a purely rational theory of Terrestrial Magnetism have been laid by Gauss. No other mode of viewing the great question can be made possible, unless signal advances in knowledge shall enable us to adopt as our point of departure, definite and established *Physical Causes.*

Magnetism-Thermo. See THERMO-MAGNETISM.

Magnetometer. A name given to instruments intended to measure any of the magnetic elements. See foregoing article.

Magnifying Power. See DYNAMETER.

Magnitude or Size. An attribute of bodies that might seem the simplest, but which is very far from being so; and regarding which it is not easy to explain the origin of our notions. Our notion of the magnitude of an external object is not a primary but a derived one. We infer the size of that object from our belief as to its distance from us. Our primary sensations tell us concerning only one attribute of external things—viz., their *form*, or rather their *projected form*; for the conception of solidity is also a secondary one. Metaphysicians have gone quite wrong in this matter, through oversight of the essential physical conditions or operations involved in those sensations that lead to our conception of an external object. The true and simple fact is this:—



A certain set of luminous points sends rays to the eye. These rays make, on meeting at the retina, definite angles, whereby the *form* of the body bounded by these points becomes apparent; but the absolute size or absolute distance between the bounding points is evidently indeterminate—that depending on the distance of the system of points from the eye. The hypothesis of distance therefore, enters as an essential element into our conception of actual magnitude; nor can we compare actual magnitudes, or apparent magnitudes at all—we simply compare *their projections on the same plane.* The only absolute attribute of external bodies is their FORM, or rather the form of their *projections.* This is revealed absolutely by the simple sensation of *vision;* everything else (excepting colour) is inferential and secondary. See PARALLEL LINES.

Malleability. That property of metals which permits them to be beaten out under the hammer or extended in any way beneath pressure. The property exists in some metals to a very great extent. It appears to bear some relation, though not that of perfect proportionality, to the ductility. Thus, the following is the order of several metals at ordinary temperatures for these two qualities:

Ductility.	Malleability.
Gold.	Gold.
Silver.	Silver.
Platinum.	Copper.
Iron.	Tin.
Copper.	Platinum.
Zinc.	Lead.
Tin.	Zinc.
Lead.	Iron.

It appears that the malleability and ductility are both increased, though probably not proportionally, by increase of temperature. A very striking instance of malleability is to be found in gold, which is beat into gold leaf so thin that less than five grains will cover 270 square inches, giving a thickness of not more than $\frac{1}{200,000}$th part of an inch.

Manometer. An instrument for measuring the density or rarity of gases. As long as the temperature of the gas remains unchanged, this is proportional to the elastic force, so that the measure of this latter is sufficient indication of the

value of the former. The most general principle upon which manometers are constructed appears o be to imprison air or gas of known elasti- ity, within a tube closed by a fluid with which he gas does not combine chemically on juxta- osition, and to allow the gas to press on this luid. The amount of the motion impressed on his fluid will manifestly indicate the elasticity f the gas. Thus, we can suppose an upright ube, containing air of known temperature and ressure—less than the atmospheric—standing ith its open mouth in a vessel of mercury, and aving therefore a mercurial column of some height ithin the tube. The slightest change in the elas- city of surrounding air will make the mercury se or fall. Other manometers, more accurate ut much more complicated, can be made, when vacuum or an approximate vacuum is obtained.

Marriotte's Law. Sometimes also called oyle's Law, from the claim preferred in behalf of at celebrated English physicist to the dis- overy. This law is the foundation of the whole eory of elastic fluids. It is, that in an elastic uid, subjected to compression, and kept at con- ant temperature, the product of the pressure d volume is a constant quantity, or the volume inversely proportional to the pressure.—In termining, therefore, the volumes of gases for e sake of comparison, it must be always re- embered that they are to be reduced to one andard pressure—thirty inches of mercury.— many gases—like air, oxygen, &c.—this rule ems to hold exactly; but in those which come liquefied under severe pressure, departures om its strict expression are noticeable, and that e more, the more closely we approach the point which liquefaction commences. This must be pt in mind when theoretical results are brought o practical application. Thus, suppose we start th 1,000 volumes of air, and 1,000 volumes of lphurous acid gas (the latter of which is uefied by two atmospheres of pressure), we all find by applying equal pressures the follow- g rate observed:—

Air, ...	1,000	853	559	314
Acid, ...	1,000	851	554	301

e same discrepancy holds with other liquefi- le gases, though in none to so great an extent.

Mars. The fourth planet in our system count- them in order from the Sun. It lies, of course, mediately outside the orbit of the Earth. The ments of this planet having been already ven under ELEMENTS, they need not be re- nted here. Mars seems to be a globe consider- y smaller than the earth, its diameter being htly upwards of 4,000 miles, or about twice t of our Moon. It turns on its axis once in enty-four hours, thirty-seven minutes, and enty-two seconds, so that its day and night are y-one minutes, eighteen seconds longer than s; and its sidereal period or its year is com- ted in 686·979 days. The equator of Mars is ique to the plane of its orbit like that of the Earth, the obliquity being 28° 42′; from which it follows that the surface of this planet must be diversified by the recurrence of seasons quite ana- logous to those which affect our globe. When Mars is nearest us, or when in opposition, it is distant from the Earth only by half the interval that separates us from the Sun; and at these periods, we can examine its surface with much effect by aid of our telescopes. Fortunately, this inspection is favoured also by the fact that Mars has only a small atmosphere. The physical features of this planet are better known accord- ingly than those of any of our companion orbs, excepting in the case of the Moon. Several accurate maps of its surface have been drawn, and all concur in showing the clear outline of continents and oceans—as well as bright spots around the poles, corresponding in all respects with our Arctic and Antarctic fields of ice. A good many years ago, Sir John Herschel gave a drawing of one face of Mars, that has been exten- sively copied and reproduced in popular Astro- nomical works. In the two figs., 1 and 2, Plate II., are pictures of two faces of Mars, differing by about 90° of longitude, represented by the pencil of Captain Jacob of Madras. The planet may be seen in that latitude, near the Zenith, and of course under the most favourable circumstances; and Captain Jacob's accuracy is beyond all question. These two plates, therefore, may confidently be taken as correct maps of those two sides of Mars,—the darker spaces are certainly the oceans;—in the second figure there seems a long tapering *Mediterranean.* It cannot be doubted that the difference of gravity on the surface of Mars, and certain other circumstances forbid us to view it as an exact physical counter- part of our own globe. Nevertheless, the differ- ences are quite too small to constitute it in any wise improbable that the general functions of the two orbs are closely corresponding; and an in- habitant of Mars would have neither more nor less difficulty in proving the Earth uninhabited, than *we* should find were we disposed to sustain the converse thesis.—To the naked eye, Mars appears a dull reddish star. It is the only one of the superior planets, the sphericity of whose disc is at all affected by its positive relations to us and the sun. Of course, it has no phases like those belonging to the Moon, or to Mercury, and Venus, but it sometimes appears *gibbous.* The cause of this, the student may make out by aid of a very simple construction.—Mars enters by the perturbations it suffers, and the corresponding actions it induces, as an essential element of the solar system. One of its most interesting rela- tions in this respect has been already explained under ASTEROIDS.

Mass. The quantity of matter which a body contains is called its mass. The only measure we have of this, is the body's weight. We know the force of gravity, and we suppose that it acts equally on all kinds of matter. Hence, when

two bodies are equal in weight, we suppose them to have equal amounts of matter; although it would evidently happen, if this supposition were untrue, that there might be unequal weights and yet equal quantities of matter. There is, however, so much reason to believe all matter at the surface of the earth to be equally acted on by gravity, that we do not hesitate to adopt such a hypothesis. Upon this principle then, the weight of a body is proportional to the product of the mass or quantity of matter by the value of gravity at the place. Another measure of mass seeming at first to depend on different principles—is, the product of the volume by the density. But when we remember that density can be measured through weight alone, we find it in reality the same definition, and built on the same postulate.

Massilia. One of the Asteroids. For Elements, &c., see Asteroids.

Materials, Strength of;—the power by which a substance resists any effort to destroy the cohesion of its parts. It is clear that the ultimate cause of all phenomena belonging to such a question lies in the nature of the molecular forces constituting the body; and, further, that the powers of all *regular* bodies to resist external violence must be related in some manner to their crystallographic axes. The student is referred to article Strain. This ultimate theory is far from complete, or in any other than an initial state; but there are signs of its speedy and rapid advance. It is very important for architects to know the exact forces which can be safely applied to the substances with which they work. The theory of the points where, and the directions in which these strains and pressures should be applied, belongs to the subject of practical mechanics, and must be treated mathematically. We think it better to refer to the elaborate works of Barlow, Rennie, Tredgold, Eaton Hodgkinson and Fairbairn, on this subject, than to attempt to give a notion of their results in our limited space. Barlow shows that there are four distinct strains to which any body may be exposed—1st, It may be pulled or torn asunder by a stretching force applied in the direction of its fibres, as ropes, stretchers, tie-beams, &c., are strained; 2d, It may be broken across by a transverse strain, or by a force acting either perpendicularly or obliquely to its length, as in levers, joists, &c.; 3d, It may be crushed, as pillars, door-posts, and truss-beams are, by a force in the direction of its length; 4th. It may be twisted or wrenched by a force acting in a circular direction, as in the case of an axle of a wheel.—The following tables from these experimental works, give an idea of the capacity of different bodies to resist some of these forces. *First*, the power which the following bodies are capable of opposing, by their cohesive forces, when that power is applied in the direction of their length, taking a rod of each body, one square inch in section—

English oak,	8,000 to 12,000 lbs.
Fir.	11,000 to 13,185 —
Beech,	11,500 lbs.
Mahogany,	8 000 —
Teak,	15,000 —
Cast steel,	134,256 —
Iron wire,	93,964 —
Swedish bar-iron,	72,064 —
Cast iron,	18 656 to 19,488 —
Wrought copper,	33,792 —
Platinum wire,	52,987 —
Silver,	38,257 —
Gold,	30 888 —
Zinc,	22,551 —
Tin,	7,129 —
Lead,	3,146 —
Rope, one inch circumference, bore at the same rate as that one of one square inch section, and the same materials should bear, ...	12,566 lbs.
White line, hand spun, 2-inch round,	7,037 —
Do. machinery,	12,582 —
Rope, 3-inch round.	7,858 —
Do. 4-inch round,	4,845 —
Cable, 14½-inch-round,	5,355 —
Do. 23-inch round,	6,069 —

Secondly; the strength of a pillar of one-inch section, of the following materials, or the maximum force it will bear without being crushed or broken outward—

Elm.	1,284 lbs.
White deal,	1,928 —
Oak,	3,860 —
Chalk,	334 —
Red brick,	538 —
Portland stone,	3,047 —
Limestone,	5,903 —
Aberdeen granite,	7,276 —
Cast iron,	631,376 —

An excellent article upon the subject of the strength of materials is contained in the *Penny Cyclopædia.*

Mathematics. As with all true and thoroughly cultivated branches of knowledge, Mathematics may be separated into two divisions, one of which expiscates *principles* or *methods*, and the other *rules* or *applications.* The latter comprehends everything we include under the term Practical Mathematics: it is with the former only that the few lines we can spare in this place are concerned. The science of Mathematics, in its most general form, may be defined as follows;—all the parts or constituent quantities of any phenomenon being connected by definite relations, general methods may be established by whose aid one set of these parts or quantities may be deduced, when the other parts or quantities are known:—such general methods form the sum of all possible mathematics. Viewed in this light, the subject in question ought to be the basis of all scientific study: although it were wrong to allege that the culture afforded by Mathematics is necessarily complete, or even by itself a very satisfactory culture. By far the most important portion of our present knowledge consists in a certain insight into our *intuitions*, or into the nature of primary facts: Mathematics, in its best sense, simply instructs us how to deal with the ascertained and definite relations of these truths or facts. It demands an eminent logical faculty; but success in it, need not involve powers of profound thought. This latter remark applies, however, solely to the

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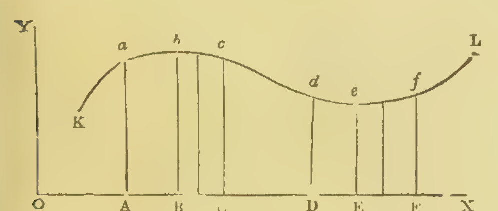
ordinary Mathematician; the greatest men require to discover, in the course of their original researches, *the nature of the relations* whose consequences they deduce. Let it not be alleged however, that the usual study of Euclid can bring more than a certain limited discipline to the mind.—Mathematics are divisible into two great branches—the *concrete* and the *abstract*. Concrete Mathematics is occupied with the peculiarity of the objects whose relations are being determined: for instance, we never forget in the course of purely geometrical reasoning the nature of the special magnitudes or quantities regarding which we are reasoning. In abstract mathematics, on the other hand, we deal simply with established numerical relations; and through the longest process of deduction, the reasoner never requires to recollect whether he is dealing with *spaces*, with *times*, or with *forces*. These two divisions are, in the modern analysis, closely intermingled; but it is not without great pleasure and hope, that we discern the recent large extension of concrete mathematics, through the revival and fresh discovery of general methods in geometry.—The different portions of mathematics are treated at greater or less length under various articles in this Dictionary.

Maxima and Minima. A function of a single variable is at a maximum state, when it is greater than both the state which immediately precedes and the state which immediately follows it; and it is at minimum state, when it is less than both the state which immediately precedes, and the state which immediately follows it.



Thus if we regard the ordinate of any point of the curve K L as a function of the corresponding abscissa, we shall have B b a maximum, because A a $<$ B b $>$ B c; and E e a minimum, because D d $>$ E e $<$ F f. In speaking of preceding and succeeding states, reference is had to the order of increase of the variable, so that one state of a function precedes another, when it corresponds to a less value of the independent variable, and one state succeeds or follows another when it corresponds to a greater value of the independent variable. A function of one variable may have any number of maximum and minimum states; but if the function is continuous, there must be a maximum state between any two minimum states, and a minimum between any two maximum states. For it is evident that, after a maximum, the function decreases as the variable increases: and since before it reaches a second maximum state, the function must again increase, it follows that there is some intermediate state at which the function ceases to decrease, and begins to increase: that state is a minimum. In like manner, it may be shown, that between each two minimum states, there must be a maximum state. Hence the number of maximum states of a function is either equal to the number of minimum states, or at most, differs from it by one. Just before reaching a maximum state, the function increases as the variable increases, hence its first differential co-efficient is positive; just after passing the maximum state, the function decreases as the variable increases, and consequently its differential co-efficient is negative; this shows that the sign of the differential co-efficient must change from plus to minus in passing through a maximum state. Just before reaching a minimum state, the function diminishes as the variable increases, and therefore its differential co-efficient is negative; just after passing the minimum state, the function increases as the variable increases, and consequently its differential co-efficient is positive; this shows that the sign of the differential co-efficient must change from minus to plus in passing through a minimum state. Here we see that a change of sign of the first differential co-efficient is the analytical characteristic of either a maximum or minimum state of a function of one variable. But a continuous function cannot change its sign except by becoming either o or ∞. These principles indicate a general rule for finding all those values of the variable, which can possibly make a function of one variable either a maximum or minimum. Differentiate the function, and find its first differential co-efficient, and place it equal to o, and also equal to ∞. Solve the resulting equations, and find the values of the variable; these values will be the only ones that can possibly make the given function either a maximum or a minimum; there may be amongst them some values that do not correspond either to a maximum or a minimum. It therefore becomes necessary to introduce some test to separate those which correspond to maxima, from those which correspond to minima states. There are two such tests:—First, substitute one of the roots minus an infinitely small quantity for the variable in the given function, and set the result aside; next substitute the root itself, and then the root plus an infinitely small quantity, and set the results aside. If the second result is greater than both the first and third results, this is a maximum state, and the root is the corresponding value of the variable; if it is less than both these states, it is a minimum, and the root is the corresponding value of the variable; if it is greater than one, and less than the other, it is neither a maximum nor a minimum, and the root is to be rejected. Test each root in this way in succession, rejecting all that do not correspond to maxima or minima.

Second, substitute the root minus an infinitely small quantity, and then plus an infinitely small quantity, for the variable in the expression for the first differential co-efficient, and note the signs of the result. If the first is positive and the second is negative, the root corresponds to a maximum, if the first is negative and the second positive, the root corresponds to a minimum, if they are both alike, the root corresponds neither to a maximum nor a minimum, and is to be rejected. The maxima and minima may be found by substituting the corresponding values of the variable in the function. The second test will, in general, be found most convenient. To illustrate the preceding rule, let it be required to find the maximum and minimum values of the function,

$$u = a - b x + x^2.$$

Differentiating we find

$$\frac{d u}{d x} = - b + 2 x;$$

and by the rule, $- b + 2 x = o$;

whence $x = \frac{b}{2}$, and $- b + 2 x = \infty$;

which gives no finite value for x.

Applying the first test to the root $x = \frac{b}{2}$, we find the relations:

for $\qquad x = \frac{b}{2} - h, \qquad$ we have

$$a - b\left(\frac{b}{2} - h\right) + \left(\frac{b}{2} - h\right)^2$$

$$= a - \frac{b^2}{4} + h^2 \text{ .... 1st result;}$$

for $\qquad x = \frac{b}{2}, \qquad$ we have

$$a - \frac{b^2}{4} \text{ ....2d result;}$$

for $\qquad x = \frac{b}{2} + h, \qquad$ we have

$$a - b\left(\frac{b}{2} + h\right) + \left(\frac{b}{2} + h\right)^2$$

$$= a^2 - \frac{b^2}{4} + h \text{ ......3d result.}$$

Whence

$$a - \frac{b^2}{4} + h^2 > a - \frac{b^2}{4} < a - \frac{b^2}{4} + h;$$

which shows that $x = \frac{b}{2}$ corresponds to a minimum, which minimum is $a - \frac{b^2}{4}$.

Applying the second test we find the following results:—

for $\qquad x = \frac{b}{2} - h, \qquad$ we have

$$- b + 2\left(\frac{b}{2} - h\right) = - 2 h, \text{ a negative result;}$$

for $\qquad x = \frac{b}{2} + h, \qquad$ we have

$$- b + 2\left(\frac{b}{2} + h\right) = 2 + h, \text{ a positive result.}$$

These results indicate that $x = \frac{b}{2}$ corresponds to a minimum which may be found by making $x = \frac{b}{2}$ in the given fraction. It is found equal to $a - \frac{b^2}{4}$, as before indicated. There is a practical rule which corresponds to those cases, in which the first differential co-efficient is equal to o, and which applies to a great majority of cases, founded on the form of the development of a function of one variable and its increment, according to the ascending powers of the increment. It is as follows:—Differentiate the given function, find its first differential co-efficient, and place it equal to o. Solve the resulting equation and find the roots, substitute each root in succession, in the second, third, fourth, &c., differential co-efficients, till one is found which does not reduce to o or ∞. If the first one which does not reduce to o or ∞, is of an odd order, the root does not correspond to either maximum or minimum; but if the first one which does not reduce to o or ∞ is of an even order, and becomes negative, the root corresponds to a maximum; if positive, to a minimum: all the roots which do not correspond to maxima or minima are to be rejected. Those which correspond to maxima and minima are to be substituted in succession in the function, and the corresponding results will be the maxima and minima required. It will not in general be found necessary to carry the substitution farther than the second differential co-efficient. To illustrate the rule, let it be required to find the maxima and minima values of the function

$$u = 3 a^2 x^3 - b^4 x + c^5.$$

Differentiating twice we find,

$$\frac{d u}{d x} = 9 a^2 x^2 - b^4, \text{ and } \frac{d^2 u}{d x^2} = 18 a^2 x.$$

From the equation $9 a^2 x^2 = b^4$,

we find $\qquad x = \pm \frac{b^2}{3 a}.$

The plus root substituted in $18 a^2 x$ gives $+ 6 a b^2$, and indicates a minimum. The minus root substituted in $18 a^2 x$ gives $- 6 a b^2$, and indicates a maximum. The roots in the function give

$$c^5 - \frac{2 b^6}{9 a} \text{ for the minimum,}$$

and $\qquad c^5 + \frac{2 b^6}{9 a}$ for the maximum.

It often happens that an important simplification can be made, in finding the value of the second

differential co-efficient corresponding to a root which is to be tested. This happens when the first differential co-efficient is composed of two factors, one of which placed equal to o, gives the root in question. The simplification is this: differentiate the factor corresponding to the root, multiply its differential co-efficient by the other factor, and in this product make the required substitution, the result will be the same as though the substitution had been made in the second differential co-efficient itself. This principle may be extended to the case where it is necessary to substitute in the successive differential co-efficients. To illustrate the method of making the simplification, let it be required to divide a quantity into two parts, such that the n^{th} power of one of them multiplied by the m^{th} power of the other shall be a maximum or minimum. Denote the given quantity by a, and one of the parts by x, the other one will be denoted by $a - x$, and we shall have for the function,

$$u = x^m (a - x)^n,$$

whence,

$$\frac{du}{dx} = m x^{m-1} (a - x)^n - n x^m (a - x)^{n-1}$$

$$= (m a - m x - n x)\, x^{n-1} (a - x)^{n-1},$$

and by placing each factor separately equal to o, we have

$$x = \frac{m a}{m + n},\ x = \text{o and } x = a.$$

The differential co-efficient of the first factor multiplied by the remaining factors, is

$$-(m + n)\, x^{n-1} (a - x)^{n-1};\ \text{for } x = \frac{m a}{m - n}$$

it reduces to

$$\frac{-m^{m-1} n^{n-1} a^{m+n-2}}{(m + n)^{m+n-3}},$$

a negative result; hence, this value corresponds to a maximum. The other values satisfy the equation of the problem, but do not conform to the conditions of it, and need not be considered. In seeking for the values of the variable which correspond to maxima and minima, any positive constant factor of the function may be omitted, without changing the final result. We may also throw off a radical sign, and be sure to find all the values of the variable; but in this case we may get some values that will make the power a maximum or minimum, but which will not make the root a maximum or minimum.—A function of *two variables* is at a maximum state, when it is greater than all the consecutive states, and it is at a minimum when it is less than all the consecutive states. Every function of two variables may be regarded as one of the rectangular co-ordinates of a point of a surface, of which the two variables are the other two co-ordinates; it is generally taken as the vertical one. We may conceive the idea of a maximum ordinate, if we consider a sphere lying upon a plane. The ordinate of the highest point is a maximum and that of the lowest point a minimum. The practical rule for finding the maximum and minimum states of such a function is as follows:—Differentiate the function, and find the partial differential co-efficients of the first order, and also the partial differential co-efficients of the second order. Place the partial differential co-efficients of the first order separately equal to o, combine the resulting equations, and find the values of the variables. Substitute these in each of the three partial differential co-efficients of the second order, and find the results. Multiply the first and third results together, and square the second, then if the product of the first and third is greater than, or equal to, the square of the second, there will be either a maximum or minimum; a maximum when the first result is negative, a minimum when it is positive. For example, let it be required to find the maxima and minima of the function

$$u = x^3 y^2 (a - x - y).$$

Differentiating, we have

$$\frac{du}{dx} = x^2 y^2 (3 a - 3 y - 4 x)$$

and $$\frac{du}{dy} = x^3 y (2 a - 3 y - 2 x).$$

Placing these separately equal to o

$$3 a - 3 y - 4 x = o$$

and $$2 a - 3 y - 2 x = o$$

whence, by combination,

$$x = \frac{a}{2}\ \ y = \frac{a}{3}.$$

We have also

$$\frac{d^2 u}{dx^2} = 2 x y^2 (3 a - 3 y - 6 x),$$

$$\frac{d^2 u}{dx\, dy} = x^2 y (6 a - 9 y - 8 x),$$

and $$\frac{d^2 u}{dy^2} = x^3 (2 a - 6 y - 2 x).$$

Substituting in these the values of x and y deduced above, and applying the rule, we get

$-\frac{a^4}{9}$,.........1st result;

$-\frac{a^4}{12}$,.........2d result;

$-\frac{a^4}{8}$,.........3d result;

and since

$$\left(-\frac{a^4}{9}\right)\left(-\frac{a^4}{8}\right) > \left(-\frac{a^4}{12}\right)^2,$$

or

$$\frac{a^8}{72} > \frac{a^8}{144}$$

the deduced values correspond to either a maximum or a minimum, and since the first result is negative, it is a maximum. Substituting these

values in the function, the maximum value is found equal to

$$\frac{a^6}{432}.$$

Mean. The established modes of eliminating the mean or most correct value out of a number of observations (of course slightly discordant) of *the same object or event*, have been discussed under ERRORS, and SQUARES THE LEAST. It is of importance, however, to ascertain the principles on which physicists generally concur that the mean value of a variable—let us say of a meteorological element—for any day, may be concluded by taking the *arithmetical mean* of any number of observed values obtained at *equal intervals* throughout the twenty-four hours. It is unnecessary to remark that whatever the principle of this method, the degree of approximation must increase with the number of such observations.—Now, any *periodical* function u of the variable v, may, as is well known, be represented as below.

$$u = a_0 + a_1 \sin (v + \alpha_1) + a_2 \sin (2\, v + \alpha_2) + \&c.$$

where a_0 is the true mean, and of course it may be expressed

$$a_0 = \frac{1}{2\,\pi} \int_{-\pi}^{\pi} u\, d\, v.$$

But if v_1, v_2, v_3, &c., denote the values of the function n, corresponding to those of the variable

$$v,\ v + \frac{2\,\pi}{n},\ v + \frac{4\,\pi}{n}, \ldots\ldots v + \frac{2\,(n-1)\,\pi}{n}$$

it may be shown that their arithmetical mean is equal to

$$a_0 + a_n \sin (n\, v + \alpha_n) + a_{2n} \sin (2\, n\, v + \alpha_{2n}) + \&c.$$

whatever be the value of v. Hence, as the original series is always convergent, we have, when the number is sufficiently great,

$$a_0 = \frac{1}{n} (v_1 + v_2 + v_3 + \&c., + v_n)$$

nearly. So that when the period in question is *a day*, we learn that the *daily mean value* of the observed element, will be given by the mean of two equi-distant observations nearly, when a_2 and the higher co-efficients are negligible; by the mean of *three*, when a_3 and the higher co-efficients are negligible, and so on.—In the case of temperature the co-efficient a_2 is very small,—the curve which represents the diurnal changes of temperature being nearly the *curve of sines;* hence as is known to meteorologists the mean of the temperatures of any two *homonymous* hours is nearly the mean temperature of the day.—In *all* the periodical functions concerned in Magnetism and Meteorology the co-efficient a_3 is small. Therefore, the daily mean values of these functions will be given very nearly by the mean of any *three* equi-distant observed values.—In selecting such hours for a continuous system of observations, we should prefer those corresponding most nearly with the *maxima* and *minima* of the observed elements, so as to obtain also the *daily range.* This condition is fulfilled in the case of magnetic declination very nearly by the hours 6 A.M., 2 P.M., 10 P.M.; which give, moreover, the maximum and minimum of temperature, of the tension of vapour nearly, and the maximum pressure of the gaseous atmosphere. If we were to add the intermediate hours of 10 A.M. and 6 P.M., we should also have the chief maxima and minima of the other two magnetical elements.—These five hours, therefore, are, beyond all others, preferable, as the periods of such observations. If the course of the diurnal curve has already been determined by a more extended system of observations, the foregoing recommendations will not apply. The mean of the day may then be inferred from observations taken at *any hour;* and such hours should be chosen, chiefly if not exclusively, with reference to the diurnal range of the elements.—See a very ample investigation of the whole of this important subject by Professor Lloyd of Dublin.

Measures. See WEIGHTS and MEASURES.

Mechanics. The science which treats of forces and their applications. The tendency of force acting upon matter is to produce motion, but two such tendencies may oppose one another, as the direction of the motions which they seek to produce, may differ. When two do not completely counteract one another, it is possible that three or four or any number of forces—so many of them acting in general direction and so many in another, so many for instance trying to produce motion eastward, and so many motion westward —may produce no motion whatever. From the application of any number of forces there may be rest produced, and it is quite evident that there may be motion. The science of mechanics treats these two cases under distinct heads, the one constituting STATICS, or the doctrine of the equilibration of forces,—and the other DYNAMICS, or the doctrine of motion produced by forces. See those articles and others *passim*.

Mechanical Effect. A term given to the measure of effective power. It is the power to raise a certain weight through a foot space in a definite time.

Mechanical Powers, are six standard machines which enable us to apply large forces to produce small effects with economy, and small forces to produce great effects in time, and which are further capable of transferring forces from their natural point of action, to another point of application. They are the *Lever*, the *Wheel and Axle*, the *Pulley*, the *Inclined Plane*, the *Wedge*, and the *Screw*. To these sometimes are added the *Toothed Wheel*—None of these *creates* new power, though several of them store up the successive additions of power which successive impulses give, until the sum total comes to be

equal to the demand.—All of them can be reduced to the two simplest, the lever and the inclined plane; and these derive their chief efficacy from the equivalence which they produce between parts of the forces which prevent our results, and resistances that we can call into action in almost unlimited quantity. Thus the lever transfers the excess of the one weight over the other or the sum of the weights, to be borne by the fulcrum; and the inclined plane throws part of the gravitating forces, upon the board which bears the weight. —We shall merely enumerate the formulæ for what is called the mechanical advantage in each; that is, the value of $\frac{W}{P}$, where W is the weight or more properly resistance, and P the power applied to overcome it.

In the lever, $\frac{\text{the arm of the power}}{\text{the arm of the weight}}$, the arm being distance from fulcrum.

In the wheel and axle, $\frac{\text{the radius of the wheel}}{\text{the radius of the axle}}$.

In the pulley, this varies with the peculiar system of pulleys. See PULLEY.

In the inclined plane, $\frac{\text{the length of the plane}}{\text{the height of the plane}}$.

In the wedge, $\frac{\text{the side of the wedge}}{\text{half the back of the wedge}}$.

In the screw,

$$\frac{\text{the circumference described by the power}}{\text{the distance between two contiguous threads}}.$$

In the toothed wheel,

$$\frac{\text{the number of teeth in the wheel of W}}{\text{the number of teeth in the wheel of P}}.$$

Medium. Any liquid or gas by which a body is surrounded, and through which motion can be transmitted according to the laws of hydrodynamics, is called a medium. Thus the atmosphere is a medium, and water is a medium, through each of which there can be transmitted light, heat, sound, and other motions. So the planets are supposed to move in a thin medium; and all space is conceived by modern physicists to be filled with such. The retardation of Encke's comet is one cause of this belief; and the transmission of the motions of light and heat across the spaces, intervening between us and the heavenly orbits, appears also to require it. Yet there may be worlds that remain invisible, because they move within spaces where no such medium exists; and the retardation of a comet may be due to its passage through one part of its orbit, where alone such a medium is to be found. See especially UNDULATORY THEORY.

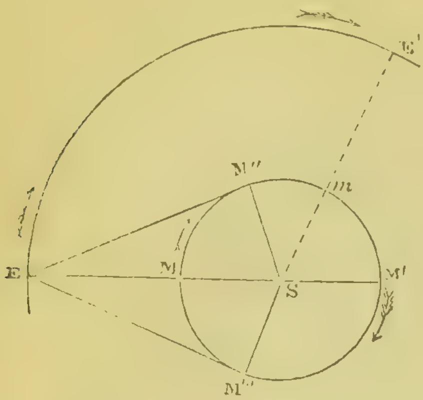
Melpomene. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Meniscus. Moon-shaped. A peculiar form of Lens. See LENS.

Mensuration, is that branch of mathematics which teaches us, from data of lines and angles, to deduce the superficial or solid content of a surface or a solid; or to find any dimension of a figure when others are given sufficient to fix it. According to this definition, mensuration would seem to include the whole of mathematical science. Special rules are deduced as consequences of special principles applicable to special cases. Thus, for triangles, trigonometry is sufficient, and generally so for all rectilineal figures. For curvilineal figures again, such as circles, ellipses, solids of revolution, &c., we require the considerations of the calculus, and the introduction of the idea of limits. For the more important and usual cases, as the case of a circle or ellipse, practical rules are deducible that are of easy application. Thus the area of a circle is equal to the square of its radius multiplied by the number π, that is 3·141592653, or as it is generally taken 3·1416. Its circumference is equal to the product of its diameter by this number.—The ellipse again is equal in area to the product of its semi-axes, multiplied by this same number.—Similarly, the spherical surface is + 4 πr^2 or the product of the square of the diameter by π, and the spherical content $= \frac{4 \pi r^3}{3}$. The content of a pyramid or cone is one-third of the area of the base multiplied by the perpendicular height from the vertex.—Rules such as these may be found in every elementary work on mensuration: our space will not permit us to exhibit them further.

Mercury. The planet Mercury is, of all the known planets, nearest to the sun, from which its mean distance is 36,890,600 miles. Its diameter is 3,099 miles, and it is denser than the earth in the proportion 1·12. The eccentricity of its orbit is much greater than that of the rest of the planetary orbits, being ·206. Its period of revolution is 87 days, 23 hours, 15 minutes, 46 seconds, and its time of rotation on its axis is 24 hours, 5 minutes. It is one of the *inferior* planets, moving, that is, in an orbit between us and the sun. It is generally so near the sun as to appear to us lost in his beams in our northern latitudes, but it is very frequently seen in the more southern. It is an immediate consequence of this position also that there should be *phases*, perfectly like those of the MOON, and *transits* like that of VENUS. The latter being of much the greater importance, from the too great proximity of Mercury to the Sun. The cycle of years after which they happen is nearly 13, 13, 13, 7, &c., and generally in November and May. One occurred in 1848, and the next will be in 1861.—It is easy to trace the actual path of Mercury in the sky. Let S be the sun, M M″ M′ be the path of Mercury round it, and E E′ part of the Earth's path round the Sun. The positions M M′ are the inferior and superior conjunctions. If the plane of the earth's and of the planet's orbit were the same, at these times, as

with the Moon, there would always be eclipses But the plane of Mercury's orbit is inclined at an angle of about 7° to the ecliptic, so that an eclipse can only happen when the planet is at



one of its nodes at the conjunction. Suppose the planet at M′ moving on in the direction of the arrow. It will seem to the spectator at E to move from M′ to M‴, apparently, it is clear, receding from the sun as the angle made by the line M′ E with that from E to the planet increases. At M‴, where E M‴ is a tangent to the orbit, it is farthest from the sun. But since the direction of the circle at M‴ is nearly the same as that of the tangent, near M‴, the angle spoken of—the angle of *elongation*, will be nearly stationary, and the planet will appear at rest. Passing from M‴ on to M, that angle continually diminishes — that is, the planet approaches the sun. At M it passes the sun, and recedes to M″, where the planet, for the same reasons, again seems stationary. From M″ to M′ it seems again to approach the sun. The angle E M M‴, or E M M″ is called the angle of greatest elongation. From M″ down to M‴ the motion is *direct*, from W. to E., and from M‴ to M″ *retrograde*, from E. to W.—Consider, however, how this motion must appear on the sky. Let the student conceive the shape of an elliptic—nearly circular orbit, a little inclined out of the plane in which its centre and the point of vision are projected on a distant surface. Evidently the figure will be a very elongated hoop or ring. If, however, the sun apparently moves in the ecliptic, it apparently twists this ring, just as it were broken at the beginning, and regularly twisted out so much in a uniform direction. Clearly, therefore, the path would be a sort of twisted line, in which we should have two *loops* which is the exact appearance of the planet's path.—To find the periodic time clearly if the planet moved as described for the figure, and E were stationary, it would be merely necessary to find the interval of any two conjunctions. But E moves on to E′, so that the next conjunction is at M. Let E and M be the periodic times of the two planets respectively, and T the observed interval from conjunction to conjunction, then the earth traverses $\frac{360°}{E}$ per day and Mercury $\frac{360°}{M}$. But their rate of separation is $\frac{360°}{T}$, therefore, $\frac{360°}{T} = \frac{360°}{M} - \frac{360°}{E}$. Now E is perfectly known, and T is observed. Hence M can be found directly. The interval, 115·88 days, from one conjunction to the next, is called the SYNODIC period of the planet.—The planet's distance from the sun can be found by means of the greatest elongation. There, E M″ S is a right angled triangle, and if E S and S E M″ be known, M″ S is known, and therefore M E. Or it may be found, as with other planets, by means of the *horizontal parallax.* Or, again, by comparison of the greatest and least angular diameters. These are in inverse proportions of the distances E M, E M′, that is, if we call M S, x.—$95\frac{1}{4}$ million miles + x: $95\frac{1}{4}$ million miles — x : : the greatest angular diameter : the least angular diameter. — The actual diameter may be found by comparing the greatest diameter of the planet with his horizontal parallax at conjunction; that is, in fact, by comparing the angles which the earth and the planet respectively subtend at the same distance, taken inversely.

Metacentre. See CENTRE OF PRESSURE.

Meteor: Meteors Luminous. The term Meteor, as generally used by Physicists, has a very wide and very vague application. It is employed to express any occurrence in the slightest degree extraordinary that takes place within the limits of our atmosphere. Thus we have the great classes of Aqueous Meteors or Hydro-Meteors, of Optical Meteors, Electric Meteors, &c., &c. These various subjects being fully discussed under more proper headings, we confine the word at present to two great classes of luminous Meteors,—of which, the latter class is by far the most important, and at present the least understood.

(1.) *Luminous Meteors depending on inflammable exhalations from the surface of the Earth.* —This class of meteors consists in the curious appearances to which the name *Ignis Fatuus* has been given. The appearance is that of a flame floating in the atmosphere a few feet above the ground, and shifting its position. It generally appears over morasses or graveyards, or any spot where there is much decayed vegetable or animal matter. From its tendency to lead the bewildered traveller towards disagreeable places, it has obtained its name. We have various and even conflicting accounts of the *Ignis Fatuus*, and the circumstances in which it has been seen; but much variation of this kind was to be expected from the flitting nature of the meteor, and the fact that it always must have appeared unexpectedly. The most reliable hypothesis at present is, that the flame is caused by the exhalation of a stream or streams of phosphorated or carbur-

etted hydrogen gas, taking fire on rising into the atmosphere. The chemical ground of this theory is, we believe, unimpeachable; and it seems to accord also with the general flitting aspect of the meteor. The brightness of the flame would, of course, change with the varying quantity and purity of the gas; and the flame would often disappear temporarily, when the quantity of the exhalation became very small. It cannot be doubted, that spontaneous ignition (as it is called) often occurs during the decomposition of vegetable and animal substances.

(2.) *Luminous Meteors not depending on exhalations, but occurring in the higher regions of the Atmosphere.*—This large and most interesting class of Meteors includes the whole subject of ÆROLITES, or *Falling Stones and Meteoric Iron*, and of *Falling Stars* properly so called, whether these are the ordinary appearances frequently seen in every clear evening in the sky, or show themselves in the recently famous so-called periodic showers of August and November. It is impossible to do otherwise than aver at the outset, that Science has here a vast deal to learn from prolonged and assiduous observation; nor must the opportunity be omitted of offering the tribute so justly merited by Professor Baden Powell, whose labours in annually collecting facts and publishing them under the auspices of the British Association, is beyond all praise. It is no slight addition to the merits of Mr. Powell, that, as his recent remarkable works so clearly show, his proper delight is in the higher walks of philosophical speculation. Nor should we omit to recognize the value of the long labours of M. Coulvier-Gravier, who, as it appears to the present writer, only required a slightly diminished inclination towards one special view of the origin or influencing causes of these Meteors, to have deserved as well as any independent observer ever did. But, as every one knows, it is a most difficult task to observe independently of some hypothesis—quite as difficult as to observe well in subservience to one.—There are only two views possible concerning this class of Meteors,—either they are wholly of atmospheric origin, or the cosmical fact to which they point becomes visible or sensible only when the objects—themselves external—penetrate within the bounds of our atmosphere. At present, no Atmospheric theory can be supported, nor do the attempted arrangements of the occurrence of such Meteors within certain months or hours, or at the times of certain winds, at all present the marked character, which —considering the necessary incertitude attending observations of phenomena of this description—is absolutely necessary to constitute even a first probability that correspondences of such a kind are connected with physical causes. We are driven, therefore, in the meantime, to refuge in the *Cosmical Theory* of these Falling Stars; not, however, definitely, but as our only resource in the present condition of observation and accurate knowledge. This theory, first proposed by Chladni, may be best put in the following general form:—*Through the interplanetary spaces, and it may be, through the interstellar spaces also, vast numbers of small masses of solid matter may be moving in irregular orbits; and these, as they approach any planet of powerful gravitation—such as the Earth—will be disturbed, and may fall towards its surface.* Chladni's hypothesis certainly explained much; but one essential part of the phenomenon it did not explain. For instance, it was quite consistent with the fact that some of these bodies fall to the Earth as ÆROLITES (*q. v.*), and that others escape as mere Falling Stars; but Chladni could, in his day, give no account whatever of the heat of the stones that do fall, and the apparent inflammation of those that only pass through our atmosphere and appear as *falling stars.* The desideratum, however, has been supplied by Modern Physics. No compression of the Atmosphere certainly, by any body moving through it, could evolve heat enough to evolve such results; but the recent and apparently established conception regarding Heat—viz., that it must be evolved as *an equivalent for any destroyed mechanical effect*, wholly removes the difficulty. The velocity of a sufficient number of these falling stars, for instance, has been ascertained within due limits by Brandes and others; and M. Joule has shown conclusively, that in regard of the greater number of these bodies—the heat, equivalent to the mechanical effect due to their original *vis viva*, and destroyed by the resistance of the Atmosphere—is such, as would melt the body and dissipate it into fragments. In case of smaller velocities, nothing beyond inflammation or white heat might ensue; but far oftener than we imagine, these falling stars are utterly dissipated by the agency now spoken of, and reach the Earth in the form of mere *Meteoric dust.* This special difficulty removed from Chladni's cosmical theory, other problems remained. *First*, Have these stones, or meteoric planets, a special or assignable origin? The Lunar hypothesis of Laplace, has frequently found favour. But it ought not to be judged by the form in which it was proposed by its founder. The idea that these meteors are directly shot towards us by Lunar Volcanoes in present action, is consistent neither with existing observation of the condition of the Moon, nor with the dynamical *essentialia* of the problem. But it does not follow that those vast Lunar Craters—such as Tycho—the result evidently of enormous cataclysms, have not contributed their part in driving among the interplanetary spaces, masses of broken rock, that may on occasion come within the range of the special attraction of our globe. But *secondly*, is it necessary to search for any confined or special origin? Is it not manifest, on the contrary, that masses of such bodies are most widely diffused and may form an essential part—not of our

Solar System merely—but of the Material Universe, in so far as Man can discern it? What, for instance, are these ASTEROIDS between Mars and Jupiter, unless—so to speak—*Meteoric Planets?* What also that strange annulus—the Zodiacal Light—environing our Sun? We shall discourse at much length on this very important subject in articles SOLAR SYSTEM, SUN, and SATURN; it is simply needful to intimate here, that there are large grounds in this direction for the acceptance, in the meantime at least, of the original hypothesis of Chladni. The curious alleged fact of these August and November periodic displays, cannot be passed without reference. Unless the testimony of Men, held unimpeachable in every other department of Observation, is to be rejected, we must accept, as an established fact, extraordinary recurrences of showers of such Meteors or Falling Stars in the months of August and November, for a considerable number of consecutive years. Nor although this very extraordinary shower has at present intermitted, does it appear allowed us to doubt, that, after intervals of rest, it frequently appeared before. If the periods of August and November are fixed periods then, very clearly, the phenomenon is an Astronomical or Cosmical one—being connected with the position of the Earth in its Orbit: and if the theory be correct that, at times we then come into contact with or pass through Nebulous Streams circulating around the Sun, it is far from unlikely that the *Nodal Motion* of these streams may furnish an explanation of the equally unquestionable fact of the apparent periodic intermission of the phenomenon. But this is a subject on which it is not admissible, at present, for any one to dogmatize. We trust vastly to future Observation, and to the course so admirably inaugurated by Professor Baden Powell.

Meteorology. That most extensive and important Science which takes cognizance of all Atmospheric Changes connected with the Earth,—with their causes and influences. It has been treated at great length under headings that apply to its various parts in other places in this Dictionary: so that it only requires that we here refer the Student to these different and appropriate articles. On the subject of *Meteorological Observation* generally—its aims, methods, and appliances—we have largely spoken under articles OBSERVATION, BAROMETER, THERMOMETER, HYGROMETER, and ANEMOMETER. The general Laws ascertained regarding the condition and fluctuations of our Terrestrial Atmosphere, are discussed under articles ATMOSPHERE, BAROMETER, HYGROMETER, HYDROMETEORS, CLOUDS, FOG, DEW, RAIN, TEMPERATURE, WINDS, and many smaller subsidiary ones. The chief *Optical* phenomena are treated under TWILIGHT, REFRACTION ATMOSPHERIC, POLARIZATION ATMOSPHERIC, MIRAGE, CORONÆ, HALOES, and RAINBOW. For others, connected with *Electric* and *Magnetic* influences, see LIGHTNING THUNDER, HAIL, WATERSPOUTS, and AURORAS. And the student is further referred to ISOTHERMALS, METEORS, STORMS, SNOW, &c., &c. A singular and very important discussion recently arose within the *French Academy of Sciences*, as to the right mode of conducting meteorological researches, so that results of a clear and definite nature be deducible. The side of the question hostile to present methods was taken by M. Regnault—a physicist whose opinions on such questions is entitled to every weight; while the propriety of existing methods was ably sustained by Le Verrier and many others. The questions mooted during this debate were so numerous, novel, and pertinent, that we shall re-open the subject under OBSERVATORY METEOROLOGICAL.—There are many great works on all parts of meteorology eminently worthy of being consulted by the student. The best general manual is undoubtedly that of Kaemtz, translated into English by Mr. Charles Walker. See also an instructive small volume by Mr. Drew. But the essays by the late Professor Daniel are especially worthy of attentive study; the writings of M. Dové, as well as the recent most able memoirs of Mr. Glaisher. As to monographs, there are multitudes;—to be distinguished above all others are the physical portions of the recent French *Voyage* to Scandinavia, Lapland, and Greenland, chiefly in this respect under the able direction of MM. Bravais and Martins; and the remarkably interesting memoirs by the late J. Fletcher Miller on the phenomena of rain in our Cumberland and Westmoreland districts. Special memoirs of highest value on the climate of various regions of the earth are, however, so abundant that we cannot undertake to specify them here. The student is referred to the instructive reports by Professor J. D. Forbes, read before the British Association. Nor must notice be omitted of the very beautiful *Physical Atlas*, edited and published by Mr. Keith Johnston, of Edinburgh, or of its great prototype by the excellent Berghaus.

Metonic Cycle. See CYCLE.

Metis. One of the Asteroids. For Elements. &c., see ASTEROIDS.

Micrography. Name given to the description of objects which are examined by the microscope chiefly.

Micrometer. The term generally applied to contrivances for measuring small spaces or angles with great accuracy or convenience. 1. *Wire Micrometer.*—When the rays, from any bright object, fall upon a convex line, an inverted image of the object is formed, which may be viewed by the eye-piece as if it were a material body. If a fine wire or spider's web be stretched across the telescope tube at the place where the image is formed, this, too, will be seen distinctly through the eye-piece. Instead of fixing the wire to the telescope tube, it is stretched across a sliding-

piece, which is moved by a screw perpendicularly to the length of the telescope, and can thus be made to measure the image in terms of the revolutions and parts of the screw. The head of the screw is divided, and there is an index by which the parts are read off. A little tongue passing over the notches of a plate notes the whole number of revolutions. A micrometer of this kind is now generally applied to circles, transits, and theodolites, in addition to the fixed wires, which of course are always necessary. There are two verifications;—first, the ascertaining the value of a revolution of the screw;—and secondly, determining the reading of the screw-head when the moveable wire coincides with the fixed wire. In a circle or theodolite the micrometer wire is placed upon a sharp distant object, and the divided limb read off. The screw is turned through several revolutions, and the object is again bisected by moving the whole instrument by its tangent screw, and the divided limb read off a second time. We have then the same angle measured in revolutions of the screw and in the divisions of the instrument, and by a simple proportion have the value of a revolution and of a part. With a transit, the passage of Polaris over the micrometer wire, is observed after several revolutions of the screw. The angular motion of Polaris for the intervals is computed from the polar distance; and thus the value in arc obtained for a revolution of the screw. To determine the zero position of the micrometer wire, the moveable wire is brought to touch the fixed wire,—*first* on one side, and then on the other,—and the screw-head read off each time. The mean of the two readings will be that—when the two wires are exactly superimposed. 2. The *position wire Micrometer* has lately come very much into use for observations of double stars, and is the wire micrometer proper for equatorials. In this construction there are two wires parallel to each other—each moveable by its own screw. The whole apparatus can also be turned round in the plane of the wires, so as to place the wires in any direction,—the angle, round which it is turned, being read off by two verniers upon a small circle, called the position circle. In measuring a double star, the wires are brought near each other, and the apparatus turned round until the two stars are either threaded on one of the wires, or, being placed between them, are judged to lie in the same direction. The division of the micrometer circle is then read off, and the observation in position is made. Now, by the divided circle of the micrometer, turn the apparatus round 90°, and the wires will be at right angles to the line joining the two stars. By moving the equatorial, place one wire A on one of the stars, and place the other wire B by its screw on the second star. Read off the screw-head of B, and then place A on the second star by moving the equatorial, and B on the first by moving its screw; and read off the revolutions and parts of B. The difference of the two readings of B will give—in revolutions and parts of the screw—twice the angle between the two stars. The process may be repeated—keeping B fixed and moving A. Before or after a series of observations, the zero or index wire of the position circle should be ascertained. Place the instrument nearly in the meridian, and make a star run along one of the wires from end to end. Read and note the position circle, which should mark 90° and 27°, and the difference from this is the correction to be applied to all the angles of position observed during the evening. The value of a revolution of the screw may be determined by separating the two wires a given number of revolutions; and observing a series of transits of known stars over them. The micrometer, or reaching microscope, for reading off the divisions of graduated circles, depends upon the same principle as the wire micrometer. An enlarged image of the divisions of the limit of the circle is formed, and this image is measured by the revolutions and parts of a screw. 3. The *divided object-glass Micrometer* and *Heliometer*. If an object-glass be cut across so as to form two semicircles, and the semilenses be separated by sliding one beyond the other, each portion will form its proper image, and these will retreat from each other as the semilenses are moved. The semilenses are mounted on slides, and the quantity of separation read off upon a scale. In Bessel's heliometer, the earliest and very perfect instrument of this class, the focal length of the object-glass is eight French feet, and the aperture nearly six French inches. Suppose a double star is to be measured with the heliometer: the whole of the object end is turned round, until four stars appear in a right line; and the semilenses are separated until the stars appear to be exactly the same distance from each other, when the scale is read off. The semilenses are then shifted in a contrary direction, sliding the two images over each other, until they again appear to be at equal distances, and the scale is again read off. The separation of the scale is four times the angular distance between the stars. There is a position-circle in which the direction of the stars is read off. Micrometers of this kind require no illumination. 4. *Reticules* and *circular micrometers*. The micrometers hitherto described are applied to the accurate measures of small angles; the present class, though very useful in certain cases, are of much lower pretensions. The reticule, or diaphragm, as it is sometimes called, is any fixed arrangement of wires or bars which can be applied to a telescope for the purpose of measurement. They are chiefly used when an object will not admit of illumination, or where the astronomer has no accurately-divided instrument at his disposal; or, as in the case, La Caille, at the Cape of Good Hope, when the object is to fix approximately a greater number of stars than could be

done in the same time with ordinary instruments. Suppose a cross like an X or V to be cut out of brass plates and inserted in the principal focus of a telescope whose optical axis is in the meridian. A star in passing through the field is occulted at its passage behind each of the bars, and the time noted. The *interval* will show, by an easy calculation, how far it passes from the vertex, and the *mean* of the times, the moment when it passes the axis of the diaphragm. If the true position of any one star so passing is known from any other source, all the other stars can thus be determined differentially with respect to it. The method is not very accurate, but may often be applied advantageously and with very small instrumental means. If a fine wire be drawn perpendicular to the axis, and a bright star, observed with illumination, made to run along the wire, the axis of the diaphragm can be set in the meridian, and that is the only verification necessary. The computation in declination will be least, if the angle between the stars is such that the base of the triangle is equal to its altitude. This reticule is very convenient for mapping, if placed in the meridian; or for cometary observation, if the telescope is mounted as an equatorial, however rudely. The circular micrometer was introduced, we believe, by Olbers, and perfected by Frauenhofer (*Astron Nachrichten*, 22), and is much less known and used in this country than it deserves. A metal ring is set in the centre of a perforated glass plate, and the outer and inner edge of the ring is turned true. The plate is fixed in the focus of a telescope, and the appearance is that of a ring suspended in the heavens. The telescope is pointed, and the observer notes the time when a star disappears at the outer ring, re-appears on the inner ring; disappears again, and finally re-appears. If two stars be thus observed, it is clear that when a mean is taken of the disappearances and re-appearances of each, the difference between the two means will be the difference of right ascension between the two stars; and therefore that if one be known, the other is determined. Again: if the diameter of the ring has been determined, and the declination of the stars nearly known, the time of describing the chord of the ring will give, by an easy computation, the distance of the chord from the centre, and that the more accurately the smaller the chord described. The sum or difference of these two distances, is the difference of the stars in declination. Frauenhofer afterwards proposed another, ring and reticule micrometer. He cut a series of rings or lines upon a piece of plane glass, which he placed in the principal focus of the object-glass, and then by a side lamp illuminated the rings, leaving the rest of the field dark. There are many other micrometers, but they are not in such general use as to demand any notice here. The reader will find them very fully described in Pearson's *Astronomy*, vol. xi., pp. 2126 to 72 inclusive.

Microphone. A general name given to instruments enabling us to augment sound, just as the microscope augments visual angles. Thus, the speaking trumpet, by confining sonorous vibrations and multiplying them by concentrations is a microphone.

Microscope. We cannot give anything like a history of microscopes, or attempt to describe the different constructions; but it seems necessary to offer a very brief account of the chief transformations they have undergone, from first to last. The ancients made use of glass bladders to magnify writing, and undoubtedly they often used them for the execution of their cameos. They had

Figs. 1, 2.

discovered that the convergence of luminous rays makes the object appear magnified. The four-

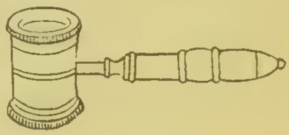


Fig. 3.

teenth century witnessed the discovery of wrought glasses; they alone were employed as microscopes

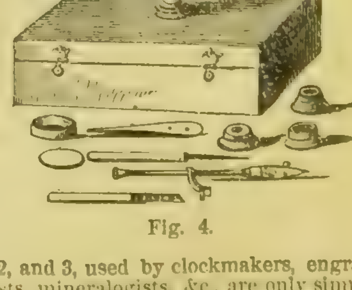


Fig. 4.

Figs. 1, 2, and 3, used by clockmakers, engravers, botanists, mineralogists, &c., are only simple lenses, sometimes called *simple microscopes*, though the name is usual when the simple lens is mounted on any foot.—The simple microscope, fig. 4, consists of a mirror *h*, reflecting the light on a carrying glass *e*, which is moveable up and down by a screw, and a glass *a*.

for nearly two hundred years. Originally, these glasses were pretty large, but the more perfect workmanship became, the easier it was to construct lenses having a very short radius of curvature, and which magnified very greatly. Of this kind are magnifying glasses of all sorts; consisting essentially in a convergent glass with short focus, fixed in a mounting varying according to the use to be made of the instrument. For most researches, the difficulty of holding little glasses in the hand, soon required the mounting of them between two plates of copper, which, by narrowing the opening, diminished the aberration: the necessity of *support* was at the same time felt, and the rudimentary form of our present simple microscope was the result.—It was with such aids as those figured above that all the celebrated researches of Leeuwenhoek, Swammerdam, and Lyonnet were carried through. Towards the middle of the seventeenth century, very small lenses of *melted glass* were substituted, their focal distance being much smaller than that of lenses made by hand: and their magnifying power being therefore more considerable, they occasioned a crowd of discoveries. The true inventor of these is scarcely known. Some say Father Della Torre, and others Dr. Hooke. Several years since M. Gaudin greatly improved these lenses by using in the manufacture of them rock crystal, free from double refraction. By an extremely ingenious process he found a means of working at once the smooth surface of more than 100 lenses; so that their plane correspond always to the most regularly spherical part of the glass. Each of these hemispheres, fitted in a very simple and convenient mounting, is an excellent microscope, and besides very cheap. It has but one defect, that of having a very narrow field. The researches of Sir David Brewster and Mr. Goring on lenses of precious stones in England, and of M. Raspail in France, are of great interest in the history of attempts to perfect the simple microscope. — The *compound microscope* dates from the seventeenth century; the first was doubtless formed by two separate glasses, the one acting as the object-glass and the other as the eye-glass. Ramsden greatly improved it by the application of his ocular system to two glasses. The microscope consists essentially of a lens of short focal distance, which is directed towards the object and which is therefore called the *object*-glass or *lens*. Placed at a distance from the object, a little above its focal distance, this glass forms an image of the object magnified within the tube supporting it. Starting from this real and reversed image, the rays which have originated it by their crossing, continue their path, so that the part of space occupied by the image radiates through a small space like a true object; this image may be then examined in space, looked at closely through the glass, as if it were a real object. This is why another lens or system of lenses is mounted at the end of the tube opposite the object-glass; a lens or system usually of pretty long focus, and called the *eye-glass*. Looking through it, we see the already magnified image of the object; thus the magnifying power of a compound microscope is equal to the magnifying power of the object-glass multiplied by that of the eye-glass.—It is impossible even to quote the chief improvements which the microscope underwent, until Euler, in 1769, indicated the construction of achromatic lenses;—that hint itself, will it be believed, not having been verified in practice till 1816, by Frauenhofer, the famous Munich optician. In France, the first achromatic microscope was presented by M. Selligue to the Institute in 1823. The opticians who have contributed to bring it to its present condition are principally—in Italy, Amici; in Germany, Ploessl, Schiek, and Pistor; in France, Chevalier, Oberhauser, Trécourt, and Lerebours; in England, Tulley, Pritchard, Smith and Beck, and Ross.—Micrographers have always been divided into two parties, the one advocating the exclusive use of the simple, the other of the compound microscope. The enormous improvements made on the latter have finally settled the question; simple microscopes being entirely incapable of competing in extent of field, or magnifying power, with compound microscopes with achromatic lenses. These can magnify as much as 1,500 diameters, and give everything with perfect clearness, up to 500 or 600. When noticing the various forms of compound microscopes, it is impossible to pass over the last improvements of Mr. Ross. The celebrity of this artist is now European. He has approached as near to perfection as any one in the construction of object-lenses; he has invented an adjusting object-piece of much practical value; and the firmness of his supports —the steadiness and facility of every motion required for adjustment, leave little to be desired. If we do not add here more special notice of the works of Smith and Beck, it is only because of the pressure of our limited space, and not because we undervalue the Instruments of these eminent artists. To Mr. Varley, also, improvements are due; and M. Nachet, of Paris, has recently produced microscopes with two, or three, or even four different eye tubes, so that a number of persons may view the same object at the same time.—The applications of the microscope are very numerous; it supplies important data in science, the arts, industry, medicine, &c.; it would be too long to exhibit them in detail; we shall notice only few. There is one condition without which the microscope cannot display its power, *e. g.*, transparency. Opaque bodies require to be illuminated from above, by the use of a glass or convex prism, or a mirror of very short focal distance screwed on to the glasses, which Lieberkühn (who in 1738 suggested the solar microscope also), first introduced; yet they can be but imperfectly studied in that way. Still as with them it is of most importance to get the reliefs, &c., the ordinary

glass or the Coddington microscope are sufficient —and besides, as these objects are often natural objects of considerable extent, they are divisible into thin plates, or steeped in certain mixtures so as to increase their transparency. In chemistry the microscope is used to detect the form of very small crystals which give a pearly appearance to liquids; in general, continuous and clear liquids escape the microscope, but the slightest appearance of milkiness or muddiness proves the existence of numberless light bodies held in suspension, spherical or angular, dead or living. The formation of crystals may be observed by placing a drop of the fluid on the plate for holding objects. This phenomenon, from the clearness of the lines and their completeness, the brilliancy of colours, and the activity of production, is one of the most beautiful that can be conceived: the isolated crystals which have been produced by slow evaporation are remarkable for the clearness of their facets and the beautiful transformations they undergo when submitted to polarized light. If this light be powerful as a help to determine the crystalline system of a body, it is still more so to effect the most brilliant phenomena of coloration which can be conceived. We quote especially the hydro-chlorate of ammonia, the chlorate of potash and the sulphate of copper. There are vital liquids also, the blood, milk, lymph, the urine, the sperm, the saliva, which are in the province of medicine. There, microscopic examinations are of the greatest value in cases of disease—perhaps too little practised. The most frequent and alluring application, however, is to the animal and vegetable kingdom; living nature especially, astonishes us with the delicacy of the organisms and the energy of movement shown in its smallest beings. With the microscope, we see that woods, barks, the coarsest as well as the most delicate vegetable and animal epidermis, hairs, filaments, algae, mosses, pollen, feculæ—are formed of concentric envelopes with canals and pores of a very delicate texture. The organs of insects are more curious still, their wings, their suckers, their antennæ, show a perfect aptitude and harmony to their various functions. There is another very interesting use of the microscope—to detect forgeries and falsifications. It appears almost impossible to obtain pure products in the case of valuable or useful articles sold in the form of powders, paste, or filaments: thus salts, meal, silk, linen, wax, chocolate, coffee, and many drugs are falsified. The microscope is indispensable for the detection of these; it is more powerful, because far easier of application, than chemical analysis; and the latter would fail in some cases, as in detecting infirmities in cloths. The microscopical analysis of the tissues shows the dealer whatever small quantity of cotton has been introduced into a stuff sold as not containing any; and it shows besides if the real material is so fine as it should be. In flour, fraud is very common; beans, pease, or oatmeal, may be added so easily; and the microscope alone can detect the adulteration. Almost all chocolate is adulterated with potato;—a fragment dissolved in a drop of water is enough to detect it. — Microscopical illusions have been much talked of; undoubtedly they are possible; they arise usually from too powerful illumination; care and attention easily however, get rid of them.—Mention should be made of the *Solar Microscope*. A concourse of the sun's rays is indispensable for it. A large bundle of rays is reflected horizontally by a plane mirror suitably inclined, and then concentrated, by a great concentrating glass, on the object. At a small distance, a series of glasses is placed, capable of forming a real and considerably magnified image on a white screen. If darkness has been previously procured in the chamber, the image, after the microscope is adjusted, is brilliant and well defined, and may be seen by a whole company. The oxyhydrogen light and the electric light have been both made use of as substitutes for the solar rays; the latter especially is capable of producing very brilliant results.

Microscope Dichroic; Dichroscopic Lens or Dichroscope,— Haidinger's. Some crystals viewed by transmitted light present different colours in different directions. This general property is termed *pleochroism*, or *dichroism* when the colours are different in two directions only. The *dichroscope* was contrived by Haidinger for examining this peculiarity. An oblong rhombohedron of Iceland spar has, cemented to each extremity, a glass prism of 18°. It is placed in a metallic circular case having a convex lens at one end, and a square hole at the other. On looking through it, the square hole seems double; and—when a pleochroic crystal is examined with it by transmitted light—on revolving it, the two angles, at intervals, in the revolution, have different colours—the colours being those which the transmitted light affords. *Andalusite, Tourmaline, Corundum, Topaz, Idocrase, Eucrase, Mica,* show well the property, and any coloured crystals not monometric that are sufficiently transparent. Dichroism is thus detected by looking in but one direction; the two colours are brought into direct contact, and made thereby obvious when not otherwise perceived. See PLEOCHROISM.

Microscope Reading. The general nature and importance of this valuable and indeed most essential instrument has already been described under MICROMETER. It only requires to add a word as to the mode of placing it. The essential requisite evidently is that its place be absolutely fixed in relation to the circle whose divisions it examines, or that its change be a definite and measurable one. In this country, generally, the former quality is sought to be preserved, and the reading microscope is fixed on stone piers:—there are two admirable examples of so fixing it,—one at Armagh, where the circle was erected under

the inspection of Dr. Robinson,—and the other in the case of the new Transit Circle at Greenwich. Perhaps no improvement could be suggested on either of these. In continental instruments again there is no effort to give absolute stability to the reading microscopes. They are mounted on the circumference or rim of a secondary brass circle or *alidada*, which, from its position, must be subject alike to motions of translation and rotation. The effect of motions or displacements of translation is equivalent to a mere *eccentricity*, and is of course compensated for, by the readings of the opposite microscopes. The effect of motion of rotation is detected by an exquisite *level*, and compensated for by a correction applied to the mean of the readings. Further reference has been made to this subject under CIRCLE.

Milky Way. The name long given to that milky band which passes through the midnight heavens nearly along a great circle of the sphere. It is sometimes applied to the entire cluster of stars to which we belong. The whole of this deeply interesting subject is discussed under NEBULÆ and STARS.

Mineralogy, Mathematical, Physical. With the great subject of Mineralogy in its largest sense we have of course nothing to do in this Cyclopædia. And there are only a few relations under which it is needful that we look at one part of it,—the part usually named *Crystallography*. Into the section of Mathematical Crystallography we shall not at all enter, further than by taking from it a few definitions. It would have been possible to have regarded it as belonging to the matters within our special range; but our space is so confined that this could not have been done without considerable impropriety.—*A crystal is an inorganic solid bounded by plane surfaces symmetrically arranged and resulting from the forces of the constituent molecules.* After this definition it is needless to add that crystallographic forms are our first and surest keys to the character of molecular forces, and that every physical action depending upon these forces or capable of being modified by them, will be found connected by closest ties with the form of the crystal. In article ELASTICITY, from the pen of Professor Rankine, certain great laws are laid down illustrative of this cardinal truth, and the same very able inquirer is further developing his views. Under ACOUSTICS we have given the striking results of Senarmont; under MAGNETISM we have treated of Magnecrystallic action in so far as that is yet understood; so that it seems to remain for us here to offer a few general remarks on the *optical* relations of crystals—a subject treated in due detail under REFRACTION and POLARIZATION. It is necessary, above all, that the student who would enter on this subject have a correct and well-defined notion of the meaning of the term—the *axis of a Crystal.* This term has simple reference to the form of the crystal; in other words, it is a pure mathematical term. An *axis* thus understood is a line connecting points diagonally opposite, such as the apices of opposite solid angles, the centres of opposite edges, or of opposite faces. Now if the three axes of a crystal (there must always be three in a regular solid) be equal, the crystal is termed *monometric;* if the horizontal diameters are equal the system is *dimetric* or *hexagonal;* if the three axes are unequal the system is *trimetric* or *oblique.* Reference to the forms of the regular solids will enable the student easily to realize these purely mathematical definitions. The main point of physical importance now to be referred to is this—(and we cannot avoid challenging its discovery as one of the greatest of this age, and one of the chief claims to high and lasting distinction of our countryman Sir David Brewster)—*the whole of the remarkable optical phenomena known as Refraction, Double Refraction, &c., are intimately and indissolubly connected with these mathematical characteristics of crystals.* It has already been stated as the law of ordinary refraction, or Snell's law, that when a ray passes from one medium to another its course changes, and that the *sine of the angle of incidence bears a constant ratio to the sine of the angle of refraction.* This new ray is the ordinary refracted ray. Now, Brewster first discerned that in monometric crystals there is no deviation whatever from this law; *one* refracted ray is produced with a direction determined as above. In all other forms of crystals, on the other hand, the incident ray is, unless in special directions, refracted or divided into two rays; and in this case also we have two classes of phenomena—viz., in one set or class of crystals *one* ray follows the law of Snell, and in another set neither ray does so. Now, Brewster showed that the former crystals all belong to the *dimetric* class, while the latter are *trimetric* or *oblique.* The details of these remarkable phenomena are discussed elsewhere (see REFRACTION DOUBLE), but we could not avoid a notice of them in this place, as the first incident that connected internal molecular arrangement, or internal elasticity, with these singular optical phenomena. Sir David Brewster was not arrested even by the brilliancy of these general results; nor is there anything finer in modern experimental inquiry than the zeal, acuteness, and success with which he followed out this range of inquiry. We shall frequently refer to his researches—especially under the articles just named and POLARIZATION.

Mirage. The name *mirage*, has been given to certain singular appearances due to special atmospheric conditions concerning the different densities of adjacent layers of air; and the phenomenon consists in an apparent displacement of the objects—an apparent depression or elevation of their images, which are sometimes erect, sometimes reversed, just as a distant shore is, when

seen by reflection in a sheet of water.—The mirage must have been observed in very ancient times. Perhaps the best known instance of it is in the Fata Morgana, at Reggio in Calabria, on the eastern coast of the strait between Sicily and Italy. From the shore of Reggio it is seen in the north of the town, that is, where Sicily approaches nearest Calabria, and as Messina is not far from the direction, it used to be commonly thought that the phenomenon was a representation of that city — distant from Reggio about three miles—seen in the air.—The elder physicists believed that *reflection* was the cause of the phenomenon—the more recent have seen that it is due to *refraction*. Wollaston, in 1800, showed how to imitate the chief effects by superposing in a vessel with plane faces, two liquids of different densities, and capable of chemical affinities to one another,—such, for instance, as water and alcohol, water and syrup,—pure water and gummed water, cold and warm water, &c. By gradual interpenetration a liquid would result varying in constitution, and therefore in density from point to point, so that the refractive indices would differ with the height. Thus

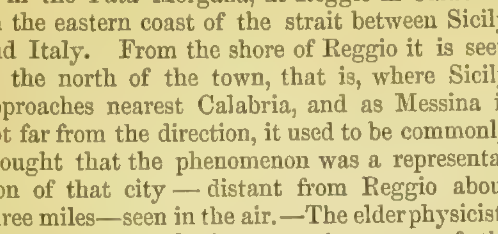


Fig. 1.

in the figure taken from his memoir, the paths or trajectories of the rays in a direction sensibly horizontal, give a direct image, little if at all erroneous, of the object.—If the curve $m\,m'\,n'\,n$ represent the law of decrease of density with height—the density being proportional at any point to the horizontal distance from a certain vertical A B (that is the refractive powers μ^2-1, where μ is the index of refraction), the trajectories, which pass from the object and traverse the system at the height $m'\,p$ where the densities are rapidly diminishing, are convex towards the sky, and the trajectory from the upper part of the object I, is more bent than that from the lower, so that the pencil of rays O $m'p$ which reaches the eye appears to come from an object R, in the direction of O $m'p$, but reversed. Finally, other trajectories may traverse the vessel, passing, for instance, across $q\,n'$, and seen at O as if coming from a second image D higher up than R and erect.—If, on the other hand, the superposition of the fluids produced an increase of the refractive powers with the height, the curve expressing their variation would be N M (as in the superposition of alcohol on water). Here, also, by placing the eye above the upper liquid we should have three images—the first, direct and straight, through the fluid, like I; the second, lower and reversed, like R; and the third, yet lower and direct, like D.—Wollaston instituted experiments also upon the vapour of ether and the vapour of water, the liquids being poured on a plate and allowed or forced to evaporate. The phenomena were exactly those of the mirage. An examination of these experiments, then, and of the conditions under which the results appeared, would evidently give us all that we could desire in respect to the actual physical cause of the mirage. In all of them there is the adjacence of layers of air of different temperatures, and, consequently, of different refractive powers, or if not of air, still of gases of different refractive powers. In the ordinary case of nature, then, where practically we have only air to consider, a mirage, since it does not always or frequently happen, will occur when the layers of air, near the level surface (of *e.g.*, water or sand), have different refractive powers to any unusual extent. A difference in temperature between the air and the soil, or water, will produce that result, if it be large enough, and then there will be a mirage. —The mirage shows itself chiefly on the sea and lakes, or on a large sandy plain. On the surface of water, it usually comes in the morning, and either in summer or autumn. At the latter season the water long keeps its own heat, and in the morning especially after a clear and calm night, its temperature may be several degrees higher than that of the air; at that time we are almost sure to see the mirage. But to do so, we must place the eyes very near the level of the water—a yard or even less from it, in order to look at the outlines of a shore or at objects, about three quarters of a mile distant. Objects which are eight or ten miles distant we may observe the mirage with respect to, from much higher elevations. Almost always at the sensible horizon of the water there will appear a little quivering motion, and if the mirage be distinct it will be bordered by a sort of serrated part, the jags of which shift and dance up and down continually. If we observe carefully the form of the objects which appear above the horizon—taking special note of those lines which seem to slope obliquely towards it, it will be seen that each of them, when it reaches a certain height above the horizon, changes its direction abruptly, nearly as if reflected at the point of change from a vertical mirror. The figure will exhibit some

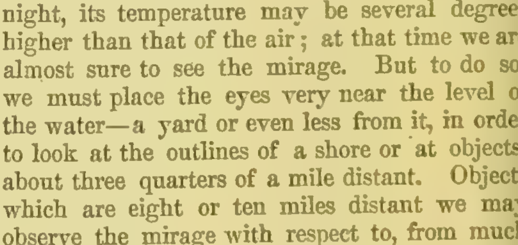


Fig. 2.

of these appearances.—The change of direction is made abruptly—but not nearly so much so as

for simple reflection at a mirror up to which the object reaches. Suppose, for instance, a horizontal mirror through the keel of the ship in the figure. The oblique line would in reflection from this mirror be given back quite straight and distinct—in the actual case of the mirage, it is confused at the point where it meets the mirror and the complete visible line—jointed sharply, in the one instance, and as it were rounded away in the other. Thus, for instance, a cape which stretches under a pretty acute angle into the sea, will appear in simple reflection quite as sharply given back beneath the water—in the mirage, on the other hand, it appears with its point smoothed and blunted.—The points of this inflection, or doubling back of the direction of oblique lines may be formed in idea by a line going all round the horizon, in the whole space where the phenomenon appears. This line we may call the line of separation. Above and below this line the appearance will be approximately as above that of an object reflected in a plane mirror passing through it and the eye. But this is not quite the case. In general it happens that the image below the plane is suddenly cut off—as in the figure—in fact, wherever it would occupy an angular space greater than that between this plane and the sensible horizon, it is so. Sometimes again a yet more remarkable effect results which we shall see farther on. By using a telescope we can often recognize this character where it could not be made out by the naked eye. The distinction between the optical appearance and the mirage can be very readily made too if a breeze happen to spring up. The reflection image wavers and disappears, while, according to Woltmann's observations, a little breeze makes the mirage more distinctly visible. The cause is probably that it preserves more perfectly the dissimilar states of the layers of air near the sea in respect to temperature—giving no time for their intermingling and permeation.—A remarkable appearance is produced where we see under mirage a low lying shore; a portion of the air which lies quite adjacent to it is repeated in the inferior image which will not itself reach quite to the sensible horizon, and the appearance is absolutely that of a portion of earth suspended in the air. Busch saw this phenomenon at the island Sproe, which was raised about 3′ above the sea level. It may be seen also very finely at the mouth of the Gironde, and it occurs even more frequently over sandy plains and steppes than over sheets of water. Sometimes in this way, a false horizon, upon which it seems to rest, may appear to the eye. Generally the telescope will make the distinction appear.—When an object is coming to the observer—for instance a ship at sea—it at one point, mathematically at the end of the caustic, which is the envelope of the trajectories of refracted rays—it appears to divide itself into two ships—an upper image—the ship as before, and a lower one, which keeps increasing in amplitude, as in fig. 2, until it quite reaches the sensible horizon. If the ship still approaches—this image seems to descend—part after part of the masts, sails, &c., fail, and at the other extremity of the caustic, it disappears. Similarly for an object which rises up from the level, but whose distance does not vary—for example the rising sun. At first there is seen a brilliant double segment—which grows until the lower segment touches the horizon—then gradually the upper segment, as the sun continues to rise, enlarges into a complete circle, and when the rim of the lower detaches itself from the caustic, the lower image disappears. At the setting of the sun, the phenomenon may be repeated, but in reverse order.—The depression of the apparent horizon in times favourable for mirage is always greater than it is under normal atmospheric conditions. It is important both as a practical problem to navigators, and as connected with the theory of the mirage, to determine this angle of depression. Woltmann has shown that when the depression passes 2′·6″ there is always mirage; and for 3′·36″ (the measurements are French), the mirage is very distinct. When we observe the angular interval between the line of separation and the horizon, it is noticed that the nearer the objects from which the observation is made, the smaller is the angular interval. Thus M. Bravais, in Lapland, observed at a distance of about 5 miles the angular interval to be 6′. At about 25 miles, it was 7′—a difference sufficiently evident to the telescope, though not to the eye. It is easy to see that if this decrease of angular intervals goes on, the mirage will somewhere stop. It is found in practice to do so at distances of about 300 yards—but this is nearly the smallest which has yet been noticed. To see it at shorter distances the eye must be lowered nearly to the level of the water. This last observation is very important—the higher the eye rises above the level the mirage becomes the less sensible. Thus, a distance of from 7 to 8 feet, is about as high as one can generally see the phenomena well under—while for lower elevations it becomes more distinct. Busch has seen it over water for about 8 yards' height. M. Bravais has made accurate measurements of this phenomenon also. He found the angular intervals for the heights 1·57, 3·03, and 13 metres (40 inches a metre nearly) to be 6′·1, 5′·8, and 2′·7 respectively for a mirage at the distance of about 5 miles. For about 25 miles' distance, the elevations being 1·9, 46·2 metres, the angular intervals were 7′·0, and 2·0. Probably, therefore, from observation there is a point of a maximum for ordinary distances and variations of temperature—a maximum which the simple geometrical considerations will show to vary with all the circumstances of the phenomenon, but which may be stated, generally, as that for heights from 1½ to 2 yards, the phenomenon will be best seen.—There is also to be

noted, the difference of magnitudes of the direct and reversed image. This, as we have already seen from the advancing ship, is a fact; for bodies at different distances and having different amplitudes, will necessarily vary. For distances of 5 miles, for instance, if the body seen by mirage occupy naturally an angular space of 12′, since the space between the horizon and the line of separation is only 6′, the lower image will represent only about one-half of the upper. This assumes what is true, that the lower image is very nearly the same as what would be produced by reflection with the under part cut off. —It is extremely difficult to give anything like an idea of the mathematical theory of the mirage. In a general way it will be evident that the curve which the different rays in fig. 1 form in their passage through the atmospheric layers must be determined. But there are an infinite number of them. If one limit curve which will just enclose them can be found, we may best confine our attention to it. Upon what then does its form depend? Clearly upon the character of the atmospheric disarrangements which induce deflection from the straight line. But these it is extremely difficult to approach by experiments. Besides, if we cannot express in mathematical form some law of variation clearly, we should have long tentative processes after all. Mathematicians have, therefore, sought for some formula, which being used as the law of variation, the trajectories or paths of rays would take such directions as in nature they do take. Two laws were stated at first—one that the densities varied in some arithmetical progression—the other according to some simple exponential form, a third law assuming a mixture of these two. Many phenomena of the mirage show the former to be untrue. It is easy to see how—for, given the law, if our processes of mathematical calculus be sufficiently powerful, we can determine what must be the path. M. Bravais considers the best expression for the density to be this:—

$$\delta = -\frac{1}{\cdot 000589}\left(\frac{\kappa}{z+h}\right)^{\frac{2}{\mu}}$$

Where κ, h, are constants, δ is the density, z the height above the level, and μ an indeterminate quantity. The differential equation for the curve is—

$$dx = \frac{dz}{\sqrt{m^2 + \cdot 000589\,(1+m^2)\,(\delta - \delta_0)}}$$

Which when m is small may be written—

$$dx = \frac{dz}{\sqrt{m^2 + \cdot 000589\,(\delta - \delta_0)}}$$

and which according to the law for δ, written above becomes

$$dx = \frac{dz}{\sqrt{m^2 + \left(\frac{\kappa}{z_0+h}\right)^{\frac{2}{\mu}} \left(\frac{\kappa}{z_0+h}\right)^{\frac{2}{\mu}}}}$$

It is useless to enter more minutely into a mathematical theory, which itself is confessedly tentative,—in which the fundamental law is hypothetical,—and which only aims as yet at binding together the actual phenomena. It is too probable, besides, that the results of practice must always considerably disagree with any, even true, mathematical theory of the subject. We may note that the value 1, for the indeterminate μ is found sufficient for the chief characteristic phenomena, but not for all; and that it has been hitherto found difficult to determine a value of μ, to which a clear preference could be given over that. It is only needful to say, that a quite similar mathematical theory directly applies to the images which appear to be the direct object — the upper image. There are certain additional and collateral phenomena of the Mirage which space requires us to give very briefly. Sometimes we see above the object a reversed image, and above that again a second erect image; sometimes, of these two upper images, only the inverse one is seen, sometimes only the direct one—the other, in either case, having disappeared. The first phenomenon is that observed by Vince, which has obtained greatest attention, and of which the completest explanation is possible. The explanation, in brief, is that the atmospheric condition is something in close analogy to the characteristic conditions of the beautiful experiments of Wollaston, to which fig. 1 refers. In fact, evidently there we have the first image of the object—the one immediately above, reversed; and the one yet higher, direct. Wollaston thinks that in the actual case of the atmosphere the logarithmic curve, as *n n′ m′ m*, will nearly represent the densities which the explanation requires. A full account of similar phenomena, with many instances, will be found in Scoresby's *Journal of a Greenland Voyage;* with illustrations, in plates 2, 3, 4. Vince noticed that the upper image disappeared when the ship, for example, approached within a certain distance. In general the explanation of the disappearance is due to the curvature of the earth. In fact, it does not disappear completely in general, but becomes smaller and smaller.—The Mirage may also be produced between two layers of air, separated by a vertical plain;—a large wall, for instance, with a southern exposure, when heated by the sun. It had been seen by Wollaston, in his experiments, that a Mirage could be produced with even more facility on a vertical than on a horizontal surface. The result he attributed to the disappearance of curvature, which rapidly, in the other case, equalized the temperature of the adjacent layers. But along the wall, the air—if in any

way of different temperature from the surrounding airs—slips up, and the effect rapidly fails. To see it, it is necessary as before to place the eye nearly in the plane of the wall, and look along it at objects approaching or going away. Wrede's account in Gilbert's *Annalen*, xi., 421, of this phenomenon, is the fullest. As here there is not the interrupting sensible horizon, there is not the cutting off of the image which holds in the ordinary case. Sometimes several images, all of them reverse, appear above the object. A description of such appearances will be found in Scoresby's *Journal*, p. 168; and their general theory, which is evidently a mere modification of the theory of Mirages above given, will be found in Biot's great work on the Mirage. One account of a combination of the two kinds of Mirages we transcribe from Bravais: —"It was in April, 1839, in latitude 69° 57′. During the observation, my eye was about twenty-eight inches above the sea level. In the evening, I saw all round me the ordinary Mirage. Gradually the sun became veiled, as we went on, then completely disappeared behind clouds; but the intensity of the appearance was little affected. The temperature was about 18° Fahr.; the wind slight S.E., and the clouds drifting from N.W. . . . . Before entering the bay of Kaafiord, we leave on the left a little peninsula, which almost forms the bay, and which juts out into the sea, with a uniform elevation of about a yard and a-half above the water. Beyond that, and within the bay, a schooner was lying at anchor—the hull of which rose out of the water about two yards. The land lay so that the deck of the schooner would naturally have been quite hidden from us; yet I saw from our distance, not merely the whole of the hull and its line of floatation, but, besides, a little of the reversed image, all of them above the little peninsula. Below the reversed image, I saw a brilliant horizontal line of white, which I do not doubt was due to the horizontal layer of snow on the peninsula. As we approached the peninsula, the schooner appeared to sink very rapidly behind it, and the hull became at length quite concealed from us, just as it would have been at any distance, had it not been for the remarkable refractions to which the phenomenon was due. The distance between the schooner and the peninsula would be about a mile, and about as much between that and our boat. The temperature of the sea was about 32° Fahr." Several facts, not noticed here, have been confounded with the Mirage. Some, like the famous Spectre of the Brocken, are merely shadows which are carried to great distances. Others are due to the reflection of the land on the clouds. We close, by recalling in brief, for the sake of observers, some of the *notanda*. 1. The Mirage is near the horizon, and especially visible over broad sheets of water, sandy plains, and great level lines of road. 2. That almost always there is an advantage in approaching the eye to the ground. 3. That the greatest attention ought to be paid to the images of the oblique lines. 4. That many phenomena of the Mirage which will escape or be indistinct to the naked eye, fully appear to the telescope. 5. That it is always of importance to know as well as possible the atmospheric conditions of temperature on which the Mirage depends. It is useful to rotate round a vertical axis, any little ball attached to our thermometer at the points for which we wish to tests before testing. 6. Wherever it is possible, *quantitative* measurement should be resorted to. Thus, the depression of the horizon—the angular interval between that and the line of separation, for a given height of the telescope and the eye—should be determined by the theodolite. A minute record of such experiences will supply the elements which the foregoing exposition shows to be as yet indispensable to all mere definite theory upon the subject of the Mirage. The whole subject is admirably treated in M. Bravais' article on the Mirage, in the *Annuaire Meteorologique*, for 1852, to which we here express our own obligations.

Mirrors. Mirrors are bodies which reflect light falling upon their surfaces. All bodies are, so far, possessed of this property. In some, however, the proportion of the reflected rays to the absorbed or transmitted rays is very small.—The chief optical use of mirrors is to enable us to change the apparent direction of such objects as a tree, a building, from us, &c.; and for such uses mirrors must be chosen possessed of the highest reflecting power, *i.e.*, throwing back a very large proportion of the incident light. Some substances are more and some less powerful in this property of reflection. Almost all the metals have it to some extent. Silver reflects more powerfully than any of them. Mercury and gold reflect also very powerfully. It is needless to say that the ordinary house mirrors are almost uniformly made of glass, with a layer of quicksilver at the back. The specula, or mirrors of optical instruments, again, are generally made of some metallic substance: a peculiar alloy, called speculum metal, is frequently employed for this purpose.—Supposing, however, that our mirrors perfectly reflect the rays of light which fall upon them, we now undertake to point out their effects, in altering the apparent directions of bodies, and also their effects, if any, in altering the apparent magnitude.—We shall take only a few cases, to avoid too much mathematical detail. The subject will be again discussed in considering various optical instruments; and the principles on which it rests are fully detailed in CATOPTRICS.—There are three kinds of mirrors commonly employed. These are the plane mirror, the spherical concave mirror, and the spherical convex mirror. The latter two are of the same form, but are turned so as to catch the light either on their hollow or their rounded

surfaces. This difference of position occasions so decided a difference in properties, that it is found needful to treat the two separately. First, then, of the plane mirror.—It has been already shown (CATOPTRICS), that rays emerging from any point, and falling upon a plane mirror, will be reflected to the eye as if they had come from a point on the other side of the mirror, situate in the perpendicular upon it from the given point, and at a distance from the plane equal to that of the point. Imagine any object placed before a mirror reflecting according to this law. Each point of the object will appear behind the mirror, at an apparent distance from it, exactly equal to its real distance from the mirror; and the various points will appear, as the continuations of lines parallel to one another. It is sufficiently clear that the image of the body seen in the mirror will be exactly like the body itself, and will appear at an equal distance from the mirror but behind it. This is the chief proposition of the theory of plane mirrors. It is an interesting deduction from this, that it is possible for an eye situate exactly at the surface of a plane parallel to a mirror, at a particular spot in that plane, to see an object whose surface is that plane, and whose linear dimensions are all twice those of the mirror itself. Suppose a straight line, A B,

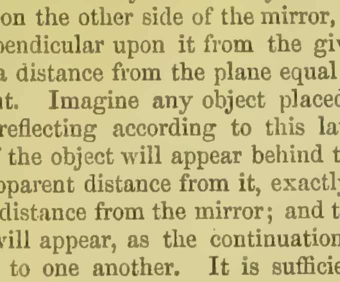


Fig. 1.

reflected in mirror C D, one-half of its size, and having a plane parallel to it. It will appear as E F equal to A B, and parallel to C D because A B is so. Let E D, F C be joined and produced to meet. Draw G I H perpendicular to C D and E F (G not being supposed to be in A B). Then since D C is half of E F, G I is also half of G H, and therefore equal to I H. Hence G must be a point in A B, seeing that I is a point in the image determined by the equality of I H to a line in its continuation terminating in the object A B. The rays coming from the apparent luminous body E F, which is the whole image of the real object, will therefore really come to G from the mirror, since E G and G F do not pass above or below it. An eye at G will therefore see the whole image.—The same proof will hold for any other line in a plane parallel to that of the mirror, because every such line is, what we have assumed the line B A to be, parallel to the plane of the mirror. Hence such a plane may be seen completely by an eye properly situate in G, in a mirror all whose linear dimensions are half those of the object, and whose superficial dimensions are therefore four times as large.—If an object be reflected at two plane mirrors, there will be generally, in each mirror, two distinct images of it, except in peculiar circumstances. In each there will be the same reflection which would have been had the other not been there; but there will also be another image which will be formed just as if the image of the object in the other mirror were reflected in this. The image, in fact, is a simple fiction, intended to represent results in the simplest possible way. The physical fact in each case is that incident rays have their directions changed, and are thrown back by the mirror. Now rays lose no ordinary properties of reflection by being once reflected, except, generally speaking, in regard to their intensity. Hence the rays emergent from an object, and falling on one of the mirrors, are reflected back to the eye directly, and give an impression of the object as usual. Other rays are not reflected back to the eye, but thrown back on the other mirror, whence they are again reflected, appearing as if the object were also behind the second mirror. But this second mirror has also given a direct image, and thrown back rays to the first, so as to form an indirect image on it, and there are therefore two images procured in each case. Fig. 2 will serve to illus-

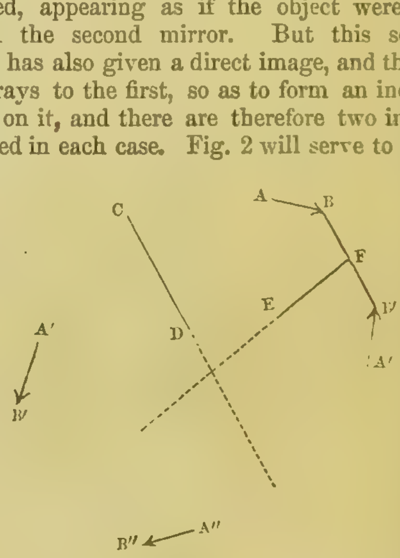


Fig. 2.

trate this, A B being the object whose images are represented, and the lines A′ B′, A″ B″, A′ B′, A B, represent the positions of the images successively formed.—If we have more mirrors, to all of which the body is exposed, we will have more numerous images, similarly formed. This, the most striking, perhaps, of optical phenomena at first sight—the indefinite multiplication of images—may be obtained still more simply by an arrangement of two parallel mirrors. An "endless gallery," as it is called, of images, is the immediate result, when an object is placed between them. The images, for a short apparent distance, are represented in fig. 3. The body Q, is shown at Q′ by reflection from B A. No rays, in fact, reach Q′, but the light from the body is thrown back upon the mirror D C. Now this light appears to come, and

diverges as if it did come from Q′, and it will therefore be reflected from D C as if coming

Fig. 3.

from Q″. The rays, then, which have not passed beyond D C are thrown back upon B A, and would give an image at a distance from it equal o Q″ G. The rays are again thrown back on D C, giving another image—that is, appearing to come from another place, and so on for ever. The same series of processes takes place with the rays which are first incident on the other mirror. —This arrangement is called the endless gallery. In fact there will, however, be a limit. At every reflection a portion of the light is not reflected but absorbed, besides a very small portion usually, which is very slowly transmitted. As the proportion of this is constantly the same, this will give the terms of a decreasing geometrical series (as $\frac{2}{3}$, $\frac{2}{3} \times \frac{2}{3}$, $\frac{2}{3} \times \frac{2}{3} \times \frac{2}{3}$, &c.), for the proportions of the light reflected in each case, to that originally incident. These proportions, though every time growing smaller and smaller, never disappear; and hence, mathematically, the images never disappear. It is clear, nevertheless, that after a few reflections, the images, being originated by rays less and less luminous, will become gradually dimmed, until they are finally imperceptible by the finest eye.—We shall just touch on the subject of concave and convex spherical mirrors. We shall simply state and illustrate one proposition. If an object before a spherical mirror be a spherical arc, having the same centre as the mirror itself, the image of the object in that mirror will be a similar arc, if the mirror be not very large in the angle which it represents at the centre. Let Q′ Q Q″ be the object (figs. 4, 5, 6), and A A′ be the arc of the mirror. Let F be the point of bisection of any radius of the mirror, and therefore (CATOPTRICS) the principal focus of the mirror for rays parallel to O F. Then the image of any point Q of the object will be obtained (CATOPTRICS) at a point along O F produced, determined by the expression F Q $\times$ F q = F O^2. Now, if we draw any radius, the point F in it would be equidistant from O, and therefore F O have the same value as for the line O Q q. The new line O Q also (the part of the radius intercepted between the centre and the object) would be of the same length as here, and therefore F Q, the difference of O F and O Q will be the same for this new radius. Hence, applying the equation F Q F q = F O^2 to both cases, we will have F q equal in each, and therefore O Q. It follows that the lines drawn from O to the various points which correspond in the image to the points of the object, will be equal, and the image will therefore be an arc having its centre at O, and so concentric with the object and the mirror. In fig. 4 the image is magnified considerably, although it is not necessarily much so with this portion of the object and of the mirror. The image is always, evidently, in the ratio of q O to Q O, to the object which it represents (since the lengths of arcs which subtend the same central angle, vary in proportion to the length of their radii. If Q′ Q Q″ be very small, and therefore very

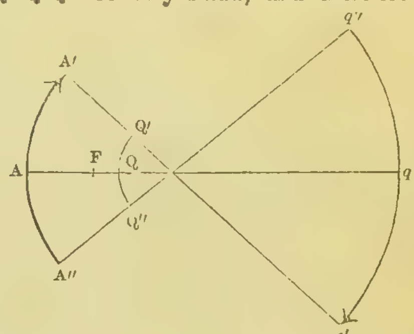


Fig. 4.

near the centre, F Q becomes nearly equal to F O. But F Q, F q, O = F O, F O, F q must therefore also be nearly equal to it; but a little greater, as F Q is a little less than F O, and as the lines F Q, F q are both measured off on the same side, Q will be nearly as much on the one side, as q on the other side of O. If O Q be more than half the length of O F, the image will be considerably magnified. Thus let O Q = $\frac{3}{4}$ O F, then F Q = $\frac{1}{4}$ O F, and F Q F q = O F^2, therefore F q = 4 O F, and O q = 3 O F = 4 O Q. The image would thus be magnified four times. It is evident, however, that the image in all cases like this would be inverted. Exactly the same proof goes to show, that if Q′ Q Q″ were an object, Q′ Q Q″ would be an inverted image of it; and a diminished one, in the same ratio as we have just found the image of Q′ Q Q″ increased. In fig. 5 the image is formed always

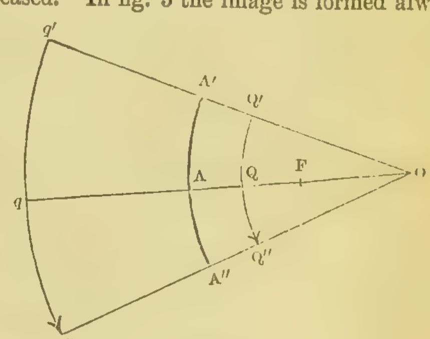


Fig. 2.

larger than the object (F Q being always less than F A or F O), and erect instead of inverted. The nearer the object approaches A, the nearer will the image do so also—(F q, F Q = F O^2), and the less will be the increase of magnitude. If Q′ Q Q″ be the object, the inverse results will obtain. This is exactly the case represented in

fig. 6, where the mirror is convex, and where

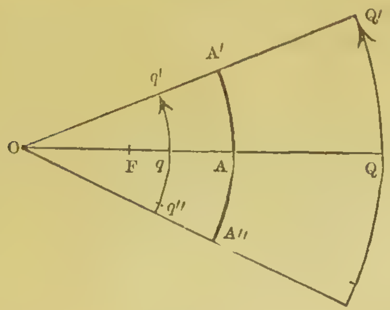


Fig. 6.

the rays from the object fall upon the convex side of the surface.—In this case, then, of concentric arcs reflected in spherical mirrors, we are not much at a loss to discover the optical effects. In the more usual case of straight or irregularly curved lines, or even lines circular, but not concentric with the mirror, we find somewhat more trouble, and the discussions of the optical effects by the old geometry would become tedious and difficult. A contortion of the image from the original form would be the evident result; but with a given small central angle, the proportion F O, F q = F O^2 might still be universally applied. See SPECULUM.

Mist. See FOG and DEW.

Mobility. That property of bodies by which they are capable of receiving movement from impulsion.

Molecule—Molecular Theories. An indefinitely small particle of matter. Where the matter is a simple body the molecules are called integrant or homogeneous molecules, and where they are not so, constituent or heterogeneous. It is conceived in chemistry that bodies can be divided into indivisible atoms, each having a definite uniform weight and general character. These ultimate particles are in this country almost always called *atoms*, while those are called molecules which are constituent or aggregated into a heterogeneous whole.—It is very evident that if we knew the molecular constitution of bodies—*i.e.*, the specific nature of these molecules, and the laws of the forces that retain them in connection, whether these be forces of attraction or repulsion, we should have the true key to tell the changes and sequences of the material universe. Various efforts have been made to construct theories on this ground, sufficiently general to enable the inquirer to avoid injuriously restrictive conditions, and at the same time special enough to afford a base for important although wide conclusions. How much may be accomplished as to specific subjects in this way, we have already illustrated on referring to the theory of Terrestrial Magnetism by the lamented Gauss. Undoubtedly the earliest satisfactory adventurer into the general field of molecular theories was the distinguished Boscovich (see appropriate article); and he has had worthy successors. Mr. Grove of London, in our own times, has most worthily undertaken the question of the correlation of the physical forces; but that estimable gentleman will join with us in especially signalizing the efforts of the recent Dr. Simon George Ohm. Ohm's grand achievements as to the theory of the voltaic circuit are now well known and fully appreciated in this country. It is much to be regretted that no translation or adequate English representation has hitherto appeared of his still more remarkable work, *Contributions to Molecular Physics*. In this most novel and singular volume he merely follows out those views which enabled him to lay down laws that are now unquestioned, as to certain large spheres of electric action. He has started the idea that ultimate molecules have both *simple* and *polar* powers; and on the ground of this hypothesis—one in consistency with every known phenomenon—he has attempted to educe a complete system from which the phenomena of *light*, *heat*, and *electricity* necessarily and harmoniously flow forth. The acceptance now so worthily given to the speculations of Mr. Grove, will, we should hope, induce some publisher to reproduce among us the researches of Ohm.

Moment. A technical term of much importance in Rational Mechanics; its definition being as follows:—*The moment of a force with respect to a plane is the product of the force into its distance from that plane.*—Let any number of forces, P, P′, P″, &c., act upon a solid body at various points of it; let x, x', x'', &c.; y, y', y'', &c.: z, z', z'', &c., be the co-ordinates of these forces: also let α, β, γ, be the angles made by the direction of the force P, with the rectangular axes, α, β, γ', the corresponding angles of the force P′, &c., &c.;—then it is known (see STATICS) that these forces will be in equilibrio, or that the solid body will be at rest, if the following six equations are satisfied; viz.,—

$$\Sigma P \cos \alpha = o$$
$$\Sigma P \cos \beta = o$$
$$\Sigma P \cos \gamma = o$$
$$\Sigma P (y \cos \alpha - x \cos \beta) = o$$
$$\Sigma P (x \cos \gamma - z \cos \alpha) = o$$
$$\Sigma P (y \cos \gamma - z \cos \beta) = o$$

The first three of these equations, intimate that the sums of the components of the forces along each of the three axes, must be o, or that these three sets of components must be in equilibrio. The second three equations intimate, on the other hand, that the sums of the moments of the forces, with regard to each of the rectangular planes, must also be o. The true theory of the subject of Moments can scarcely be said to have been understood previous to the writings of Poinsot, by whose happy introduction of the notion of *Couples* (*q. v.*) all doubtful speculation has been removed from this branch of Rational Mechanics. A body can move only in two ways; it may have

a motion of *translation*, or a motion of *rotation*. Now, according to Poinsot's simple interpretation, the first three of the foregoing equations, indicate that there is no motion of *translation;* while the second three indicate, as clearly and definitely, that there is no motion of *rotation*. The term *moment* is still preserved; but the old theory has quite given place to the doctrine of *Couples*.

Moment of Inertia. A term first employed by Euler. It signifies the sum of the products of each molecule of a rotating mass by the square of its distance from the axis of rotation, or

$$\int r^2\, dm.$$

This function is a very important one; it indicates the exact *energy of rotation* in the rotating body. See MACHINES. Euler proceeded to determine those axes of rotation, in rotating bodies relative to which, the *Moment of Inertia* must be a *maximum* or a *minimum;* and he hence deduced his remarkable theorems regarding the *principal axes*. See AXES PRINCIPAL.

Momentum. A term employed in various senses in natural Philosophy, though chiefly as equivalent to quantity of motion. It is found that the same effect is produced when a mass of, suppose one hundred pounds, strikes a body with a velocity of one foot per second, and when a mass of fifty pounds strikes with a velocity of two feet per second. Thus if we strike a ballistic pendulum with either of the two balls, we shall have equal amounts of oscillation resulting. The momentum then is the product of the mass by the velocity, or something which bears a constant relation to that product. It is usual simply to define momentum as the product of the mass by the velocity. Thus a body M moving with a velocity V has a momentum = M V. The peculiar use of the term, and of the notion which it contains, arises from the fact that in all mechanical effects produced by matter in motion, a loss of mass is compensated by an increase of velocity in the same proportion. See VIS VIVA.

Monoceros, or the Unicorn. One of Hevelius' constellations. It is surrounded by Hydra, Canis Major, Orion, and Canis Minor. Its largest star between the fourth and fifth magnitudes.

Monocular Telescope. One having a single eye-piece, and so serving only for one eye at once.

Monsoon. See WINDS.

Month. The solar month is the time that the sun takes to go over a space of 30°, or to pass through one of the signs of the zodiac. The lunar month—the month proper, is the time that the moon takes to traverse the whole of the zodiac. This is a *lunation*.

Moon, The. Every important detail regarding the motions of the earth's satellite being given under LUNAR THEORY, NODES, ORBIT, and ELLIPSES, it now falls to us to discuss that most interesting perhaps of all cosmical subjects—the *physical constitution* of our nocturnal luminary. The subject is as wide as it is interesting; and, after a few preliminary remarks, we shall divide it into several heads.—The general facts regarding the moon in relation to the earth and in comparison with it, are stated under ELEMENTS; nevertheless it may conduce to distinctness if we repeat the statements here. The mean distance of the moon from the earth is 59·96435 terrestrial radii, or about 240,000 miles—a long distance assuredly, but how small compared with the interval which separates us from any other globe! And this distance can be so largely diminished by the power of the telescope, that it ought not to appear wonderful that we know the entire selenography of the side of the moon turned towards us, better than we do the geography of any one hemisphere of our own globe. The diameter of our satellite is only 2,153 miles, that of the earth being 7,926, so that the hemisphere we see is equivalent in size only to *a fourteenth* part of one of our terrestrial hemispheres. The surface visible is not more than twice the size of Europe; nevertheless, upon that limited surface there are indications of the action of vast cosmical forces within a comparatively small globe, the study of which may, if rightly conducted, go far to guide and correct us in our inquiries respecting the progress of our own world. The *density* of the moon is little more than half that of the earth, so that, taking the mass of the earth as unity, that of the moon is only 0·011399: gravity at its surface is not more than one-sixth of our terrestrial gravity; therefore if internal explosion or upheaving forces exist there and are independent of the size of the globe, we should expect to find disruptions on its surface much exceeding in comparative magnitude anything that is manifested even in the most rugged of our own regions. Lastly, and to conclude these preliminary remarks, the moon turns towards us always the same face. This is equivalent to the fact of her rotation in space around an axis, in the same time that she performs her monthly revolution. Disputes have arisen whether the term rotation ought to be applied to a phenomenon like this. They are merely verbal ones. But as we shall speedily see, the phenomenon itself has most important physical consequences.—These facts premised, we proceed to a closer inspection of the character, habitudes, and probable history of our satellite.

I. THE MOON'S FIGURE AND ITS PHYSICAL CONSEQUENCES.—Like all the celestial bodies, the shape of the moon, speaking roughly, is spherical; but we cannot assume this to be absolutely correct without fuller inquiry. It follows theoretically from the consideration of *moments of inertia* applied to the peculiar circumstances distinguishing the moon's motion of rotation, that this orb must either be an ellipsoid whose greatest axis is nearly parallel to the radius vector, or that its

interior is heterogeneous, and that its centre of gravity does not coincide with its centre of figure. The first hypothesis is negatived by observation, so that we are forced upon the second; and the question becomes—Can we detect the relations of position of these two points, and the length of the interval that separates them?—a problem apparently hopeless, yet now fully resolved, through study of the *lunar inequalities.* Professor Hansen has recently established the following remarkable propositions:—1. *If the centre of gravity of the moon and the centre of its figure do not coincide, then must all the co-efficients of the inequalities in mean longitude be multiplied by a constant factor which is a function of the distance between these two centres projected upon the radius vector.* And 2. *If the centre of the moon be farther removed from us than the centre of gravity, then is this factor less than unity; but if, on the contrary, the former be nearer to us than the latter, the factor will be greater than unity.* Now, on comparing the theoretical evaluations of the lunar inequalities with these inequalities as observed, a slight discrepancy appears; and the Greenwich observations from 1750 to 1830, discussed by Mr. Airy, decisively show that theory and observation will not exactly coincide unless all the principal co-efficients of these perturbations be *increased*, or multiplied by a certain number *greater than unity.* Hence the immediate inference, that the *centre of the moon's figure is nearer us than its centre of gravity*—a result in itself of the last importance. But it is further clear that the *amount* by which these co-efficients require to be increased, or the magnitude of the constant multiplier which shall suffice to reconcile the observed with the theoretical perturbations, must also indicate the *exact distance between these two centres.* The constant factor deduced by Hansen is

1·0001544,

from which it follows that the interval between the two centres is

176909 feet,

or upwards of $33\frac{1}{2}$ English miles. Let us dwell for a moment on the momentous consequences of this singular result. Supposing the moon spherical, which that orb is very nearly, its peculiar condition will be understood by aid of the subjoined diagram. If the strongly marked circle

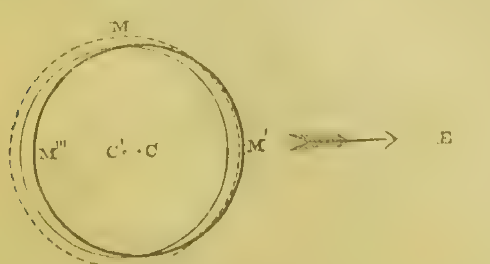


Fig. 1.

represent the lunar orb, C will be its centre of figure; but, at an interval from it, behind the line leading towards the earth, is another centre C', distant from the former upwards of $33\frac{1}{2}$ miles. Now this last centre C' is that one towards which heavy bodies on the lunar surface will fall; it is the point around which, therefore, all free gravitating masses, such as an atmosphere or ocean, provided there are any such, would endeavour to arrange themselves; it is the point, likewise, around which the moon must rotate, and which alone must be taken into account in problems regarding the motions of that body in its orbit. The physical meaning of the separation of the two centres is simply this—the hemisphere towards the earth is lighter than the hemisphere turned away from us. May this inequality have arisen from the action of great upheaving causes mainly within the former hemisphere,—and is this indicated by the uptorn nature of the surface of the moon that we see? Those disrupting forces acting partially and with the energy with which, as we shall soon find, they have acted, ar quite adequate to have *hollowed out* one hemisphere to an extent sufficient to account for th existing discrepancy. But—apart from speculation—observe the necessary consequences of tha discrepancy as to level, climate, and all physica relations connected with these. In the foregoin figure the regions at M and M'' alone, are at th *mean level* as that is determined by gravity; th regions at M', on the other hand, are far above tha level, while the opposite regions at M''' are as fa below it. Suppose then, that, on the surface o our satellite, matter is developed, as it is with u in the three forms of solid, liquid, and gaseous,— is it not plain that under such circumstances w have no chance of seeing either a lunar ocean a lunar atmosphere? The ocean, for instanc would tend to arrange itself along the line of th second circle, submerging the hemisphere opposi to us, but leaving the hemisphere nearest us (th only hemisphere we can ever see) utterly dry,— free from the presence of water as the loftie peaks of the Himalaya. It is certainly not nece sary to predicate of that antipodal hemisphere, complete submergence; but it may be confident asserted, that, whatever the aspects of the mo as we discern it, oceans with all their concom tants may exist on the opposite parts of its su face. Nay yet further, should the displaceme of the centre of figure from the centre of gravi have taken place in the manner indicated abo we should expect to find among the lunar plai —those portions of the visible hemisphere whi have been elevated only, not disrupted—distin marks of oceanic action, traces of the gradu retirement of the ocean shores. With resp again to the atmosphere, the dotted circle may supposed to represent its greatest palpable elev tion. Such an atmosphere will rest, therefo solely over the hemisphere concealed from us, a we need marvel no more at its apparent abse from the surface scanned by the telescope, th

Fig 2

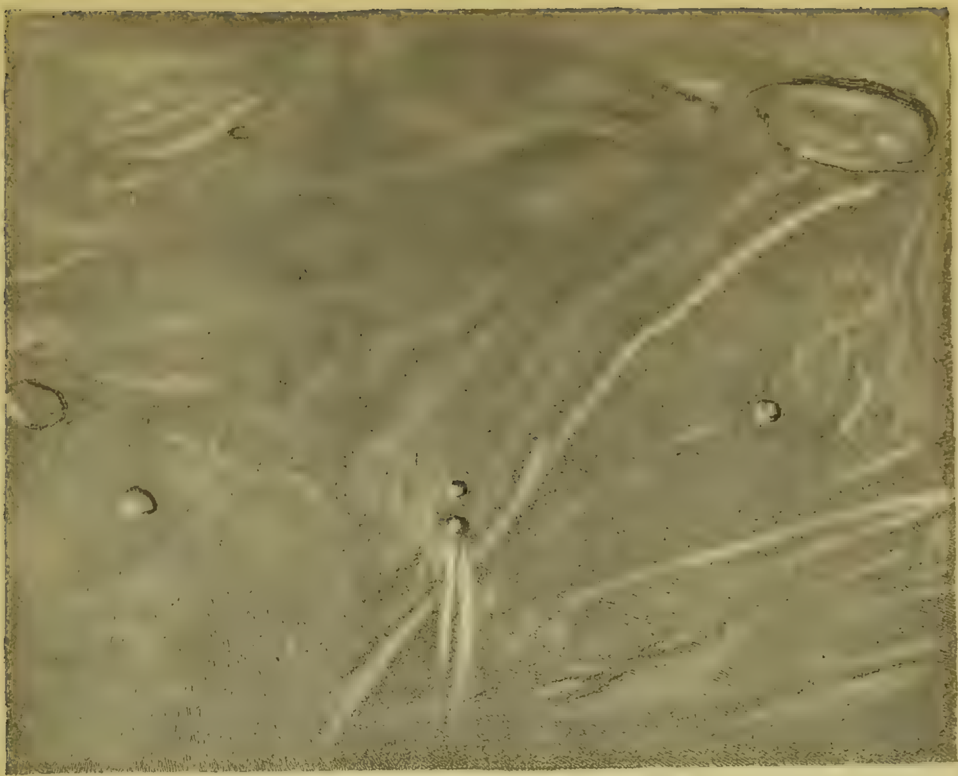
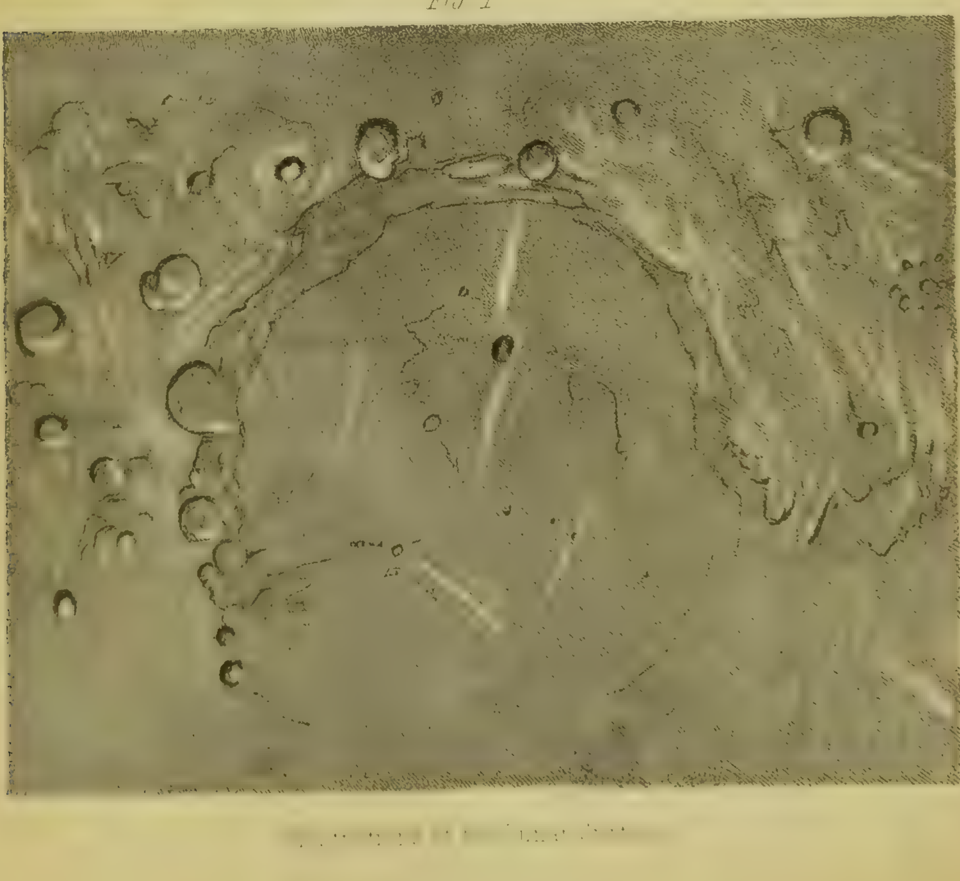


Fig. 1



at the fact that no trace of air could be found on the summit of a mountain raised 120 miles above the mean level of the surface of the earth. Thus are we constrained to conceive of our satellite, under two aspects. The face that is turned from the earth may be varied even like the regions around us. Oceans or seas may cover portions of its surface, refreshing breezes may fan it all, and it may be occupied by those very forms of animal and vegetable life which our own experience shows to befit the cosmical relations of the compartment of space common to us and to the moon. On that face of our luminary which alone we can see, it were folly to expect traces of anything so agreeable. The conditions under which it exists—as we learn these from inexorable science—suggest the picture of a surface, rocky, arid, and lifeless, three and a-half times more extensive than our own inhospitable Sahara.—Let us proceed to examine minutely this singular region.

II. Description of the Visible Hemisphere of our Satellite.—It is scarcely possible to conceive a more remarkable contrast than that between the appearance of the moon to the naked eye, and the forms she presents to the telescope, whether in quadrature or when she is full. Instead of a plain and bright surface sending from all its parts an illumination not far from equable, we discern a body of most strange character, broken by irregularities which in extent and contour present few analogies with the mountainous regions of our own globe. The reality of these, as well as the singularity of their shapes, the briefest glance at the crescent luminary, even with a small telescope, is sufficient to establish. The incomplete edge is always under the influence of a morning or evening light; and the phenomena of lighted peaks dark valleys and long shadows, which occur, in such circumstances, in any broken district of the earth, are there distinctly visible, but on a scale far more grand. Analyzing this strange vista into its separate parts by aid of large telescopes, and noting their distribution, we discern in one region vast mountains throwing their long shadows upon a plain, and elsewhere the aspect of various pits or caverns, some of which are as deep as Mont Blanc is high, often crowded together with the compactness of a honeycomb. Indeed, it can require no further detail to convince us that we are in presence of a scene of intensest interest,—one whereon that mighty cosmical force which is the common cause of disruptions in all the planets, has displayed its wonderful energies, not perhaps in every form and variety, but with a prolificness assuring valuable aid to the inquirer who may desire to reach its hidden seat. The surface of the moon has often been studied and depicted by good observers, and several men of note have constructed maps of our satellite. While referring to the older labourers, who wrought with imperfect telescopes, we must distinguish Shroeter, whose enthusiasm in this field was worthy of its importance. But it is to Mädler, now of Dorpat, that we owe the first accurate seleno-topography. This excellent and industrious observer has, in conjunction with M. Baer of Berlin, drawn a map of the moon of three feet in diameter, of surprising accuracy. Nor would it be pardonable to omit reference to the still unpublished landscapes of Mr. Nasmyth, the singular beauty of whose pencil so fittingly adorns the energy and perseverance he is addressing to the culture of this attractive theme. Waiting for the vast accession to our knowledge which cannot but accrue from the labours of the Committee of the British Association who have access to the gigantic reflector of Parsonstown, the writer of the present notice will make ample use of the researches of the indefatigable observers above named: and he will do so with the less scruple, because, through fortunate circumstances, the objects about to be described are altogether familiar to himself.

(1.) *The Lunar Plains.*—The diagram on the opposite page, although of slight dimensions, will serve as a sufficient index to the distribution of forms on the moon's surface. It shows that surface divided into two well contrasted portions. Of these, the compact unbroken and regular region occupies not more than one-third of the entire disc, and it consists of a number of low flats or plains, easily distinguished by the naked eye in consequence of their comparatively darker tints. These plains are, for the most part,—at least so frequently that it may be taken as a general rule—bounded by the lofty sides of mountain formations, although they likewise often communicate with each other by narrow open *necks*. Of their profile little need be said. They are not absolute flats, but low grounds through which low ridges frequently pass, and where isolated peaks sometimes arise, as also craters wide and narrow, but with a few exceptions, of no great depth. The plains constitute, at the present time, the undisrupted part of the moon's surface. In former times they were considered *seas*. Certainly there is no water or liquid in them now; but one cannot fail to be struck with the similarity between their outlines, and the general aspect of our terrestrial system of oceans. They have their multitudes of bays and creeks, their branching Mediterraneans, their straits joining sea to sea, their inland or isolated Caspians, their ridges of islands, as well as their isolated islands: in short, every conceivable feature characteristic of this portion of the surface of the earth. But there is another circumstance whose indications appear to point much farther. The regions we speak of are distinguished by great variety of *colouring*; and in very many cases, especially in bays, the contours of the different shades of colour run parallel to the contours of the coasts. This feature is exhibited in fig. 1, plate III.—a sketch which is merely illustrative What could indicate

more forcibly the gradual retirement of an Ocean and the formation of successive shores? Nor are concurrent phenomena of various descriptions wanting in the moon. Can these traces really point out the process of the retirement of the waters, during the progress of that immense movement of elevation to which the displacement of the moon's centre of gravity is owing? If

Fig. 2.

so, they must mark *epochs* in the history of the elevating force, and thus constitute a main fact in aid of our efforts to establish a *selenology*. We shall not dwell here on mere hypotheses: but the fact that such an hypothesis may already be started without absurdity or presumption, shows the importance that ought to be attached to more accurate and sedulous investigation of this portion of the phenomena of our satellite.

(2.) *The Moon's Mountain forms.*—These are of three distinct and easily discriminated kinds. In the course of our description, we shall compare them with whatever terrestrial forms may appear similar and cognate.

a. The *first* class of lunar mountains, taking them in the order of their simplicity, consists of a number of perfectly isolated peaks or sugar-loaf mountains *unconnected with any group or range whatever.* These mountains rise suddenly in the midst of unbroken flats, and at a distance from general disturbances. Perhaps the finest instance is Pico, a very brilliant rock, about half as high as the loftiest of our Alps, which towers almost precipitously north of the crater Plato. No system whatever is connected with that solitary peak; it stands out a single unaccompanied protrusion. Very frequently, also, we find such peaks in the centre of the plain that forms the base of a crater—in Tycho, for example. In our own globe such peaks are not uncommon, as in Cantal or Teneriffe or St. Helena, supposing the surrounding ocean removed. Generally speaking, these terrestrial cônes belong to some large sphere of disturbance; but frequently also they are quite isolated. How strange the energy that resulted in such phenomena! They would seem to have shot through the plain in obedience to some sharp limited internal force, as one would push a needle through a sheet of paper; and the plain has not been much more disturbed. Such rocks in our globe generally belong to the family of the Traps.

b. Mountain ranges or chains are by no means wanting in the moon, although they are very far from constituting a chief feature of the elevations of that body; their usual position is that of a curvilineal but broken skirt of the greater flats or plains. For instance, the Appenines the Caucasus and the Alps, form, in fragments of a ring, one edge of the *Mare Imbrium.* Some of these masses reach a great elevation, the Appenines rising from eighteen to twenty thousand feet; and there is another ridge on the very rim of the moon which appears to rival the gigantic Andes or Himalaya. Looking closely at these lunar ranges, and comparing them with the mountain chains of our own planet, we discover certain minute features, alike of resemblance and contrast, which it especially behoves us to signalize. *First*, a curious peculiarity in the form of our chief terrestrial mountains also distinguishes those of our satellite. All important ridges on either globe are comparatively steep on one side, descending to their base by abrupt precipices, or a succession of steep and narrow terraces; while they slope away on the other side through an extensive and gently declining highland,—a configuration of which the lunar Appenines and the Asiatic Himalaya or even the European Alps are illustrations of equal force. Attempts have been made to explain the phenomenon by the agency of torrents of water or floods or of the

ocean. Effect is here confounded with cause: the ocean washes the feet of these mountains on our globe because of their rapid descent on one side. Still further, the abrupt face of the mountain chains in the moon is uniformly towards one of its great plains. The plain, for instance, is situated in this fashion,—

Fig. 3.

never as below—

Fig. 4.

How like what we have on the earth! Suppose the vast Pacific cleared of its waters, or that through the gradual elevation or swelling out of one of our hemispheres the waters had retired towards the opposite face of the globe—both Indian and Southern oceans thus becoming dry land—*that* would be a mighty low land or comparative *flat*, broken only as the lunar plains are broken, here and there by a ridge or rock, and a group of mountains: but observe its *edge*. Skirted on the one side by the precipitous cliffs of the Andes and Rocky Mountains, on the other, after some breaks, by the still more precipitous Himalaya and the Paropamisan, and then by the fronts of the heights of Abyssinia and Laputa—in all which the *slope* is on the *opposite side*, forming, in the two chief instances, the continent of South America and the long inclination of Siberia. There is not mere analogy here, but an absolute identity of phenomena; nor can it be doubted that the comparative geographer will one day draw from the study of both, truths of high import concerning the orographic structure of either world. The *second* noticeable feature is one of differences, and although at first sight it may seem of trifling consequence, we shall find, in our next general section, that it is, on the contrary, of very great import. It is this: on our planet, important ranges of mountains have always systems with which they are connected. These systems consist of parallel ranges which, generally speaking, are found to have been elevated at the same time and by the same effort of the upheaving force. Now, in the moon, there is not a trace of this peculiar disposition. A characteristic like that prevailing on the earth, must, as will readily be suspected, be intimately connected with the mode in which the erupting force has, during recent epochs, usually acted here; and, indeed, it has been made the ground of some of the largest speculations of modern geology. The absence of that characteristic in the moon establishes a fundamental difference in the mode of upheaval peculiar to recent epochs in our satellite. The nature of that difference is indicated in the next general section of our article. *Thirdly*, The *direction* of the few lunar ranges known to us presents no indication whatever of the agency of a *central force*, but rather the contrary. The student must be aware that the origin and nature of the upheaving cause has recently been often referred to relations between the crusts of the various planets, and what has been imagined to be their *molten interiors*. Passing over other difficulties in the way of this theory—difficulties that seem at present insuperable (see PRECESSION and TEMPERATURE), it is easy to see that if a central force of any kind disrupted or cracked the crust of a planet, the fissures thence arising would—generally speaking—lie along *great circles of the sphere*. Now, the few lunar ranges that exist have no connection whatever, near or remote, with great circles of the sphere. On the contrary, they encircle one of the great plains; just as if, by virtue of some force, which assuredly could not be a central one, such a circular surface as our Arctic zone had been depressed, or the Arctic circle which surrounds it, elevated. In the case of the earth, the chains of mountains are so massive and multiplied, and of stretch so surpassing, that by aid of a degree of arrangement, and perhaps not over nicely scrupulous an adjustment, they have sometimes been constrained into unwilling conformity with a foregone conclusion: but the corresponding forms of our satellite absolutely resist assimilation, and thereby pronounce, in language not to be mistaken, their utter dynamical incompatibility with the idea of a CENTRAL FORCE.

c. Lunar Craters.—The objects so named may justly be termed the characteristic feature of the

Moon's disturbed region, as they certainly are its most wonderful and peculiar one. Not less than three-fifths of the surface of our Satellite are studded with vast caverns, or rather Circular Pits, penetrating into its mass, and usually engirt at the top with a wall of rock, which is sometimes serrated and crowned by peaks. These caverns, or craters as they are termed, vary in diameter from fifty or sixty miles to the smallest space visible—probably 500 feet; and the numbers greatly increase as the diameter diminishes. The ridge that environs the crater is always sloping on its external side, and steep or rather precipitous within, although it seldom descends at once to the cavern's base. Within it there are generally concentric ridges assuming the form of terraces. The bottom of the crater is sometimes convex; low ridges of mountains often running through it; while at its centre, conical peaks frequently rise, and smaller craters, whose height however seldom reaches the base of the exterior wall. These curious objects are so crowded in some parts of the Moon that they seem to have pressed on each other; and through their mutual interference the most odd-shaped caverns have arisen. It often happens, too, that similar craters are found *on* the wall; and in many instances one can discern that the wall has been severely shaken by the force that gave rise to the secondary object. The general character of the large portion of the lunar surface, occupied by these craters, is well represented in our frontispiece, drawn from the highly magnified image of a small region of our Satellite: but, to render the character of these craters more palpable, we shall describe one of them in detail —the crater Tycho. Tycho is that brilliant spot near the top of the lunar disc, in diagram, page 513, which, when the Moon is full, appears the centre of a system of shining streams or rays. The country around it is singularly disturbed. Now, if, passing across that rugged district, one were gradually approaching Tycho, its first aspect would seem like an immense ridge of rock in the horizon, with a stretch of nearly fifty miles, and reflecting the solar rays with a peculiar lustre. On approaching the ridge, its character would change: we should then discern it as part of an immense circle, but neither very lofty nor very steep. If one ascended that slope, how appalling the scene! Instead of finding on the other side a slope in most respects corresponding with that just ascended, we stand on the edge of a dizzy cliff, passing down by one unbroken leap for 13,000 feet! At the base of the cliff several low parallel terraces creep along: but a little onwards the full depth of the chasm appears; and it descends from the top of the ridge no less than 17,000 feet, or 2,000 feet more than the summit of Mont Blanc rises above the level of the sea. It is quickly perceived that this huge barrier encloses a vast circular area of the Moon's surface,—an area, fifty-five miles in diameter: so that if the spectator were at the chasm's centre, he would find around him on every side, at the distance of twenty-seven miles, a gigantic unbroken wall—unbroken by gap or ravine or pass of any description—rising into the air 17,000 feet, and forbidding his return to the external surface! How frightful such a seclusion,—a chasm utterly impassable, its walls bare, rugged, hopeless! Nor among these countless pits may Tycho be the most appalling. There appear to be some of nearly equal depth, whose diameter does not exceed 3,000 feet; nay, towards the polar regions of the moon, caverns probably exist whose depths have never yet been illumined by one beam of the solar light.—It is very evident, how wrong it were to confound formations so extraordinary with what are usually named craters on the Earth, or to fancy that there is any true similitude. The latter are openings on the tops of volcanic mountains of slight extent, and for the most part constituted of scoriæ and other loose erupted materials; a lunar crater, on the other hand, being a pit or circular cavern in the body of that orb. It is necessary when searching for similitudes, to look among displays of the erupting energy, of much greater extent and effect: and we find our best instances in what are termed *craters of elevation*. These exist in many regions of the globe—for instance, in Auvergne in France; and they consist of a circular mass of mountains, steep within, and generally with a central cone or

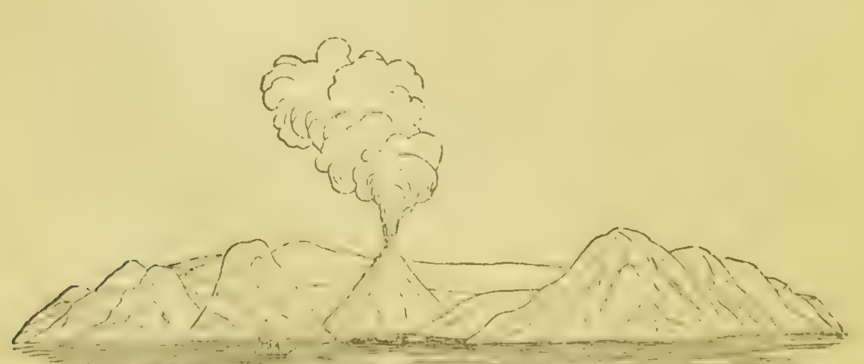


Fig. 5.

peak. One remarkable instance is *Barren Island*, sketched by Von Buch, on the foregoing page. Another is the curious island *Santorini*, as below.

Fig. 6.

On a larger scale, too, the valley of *Bohemia*, the *Montagnes d'Oisans*, &c., present the same class of forms. It were very unreasonable, however, to expect a perfect resemblance; for on the Earth, the persistence of winds, rain, and the agency of the ocean, has more and more destroyed and broken the primal rocks; while in this region of the Moon, meteorological action must have ceased for countless ages.——But there is a special characteristic of these lunar crater-forms of interest and value altogether paramount. We have alluded already to that system of bright rays of which Tycho is the centre. These curious rays are not confined to Tycho. Systems of the same description and of greater or less complicacy are connected with other craters, such as Copernicus, Kepler, and Aristarchus; and similar rays traverse the moon in many directions, although they cannot be traced to any crater now existing. But they may be studied, with chief reference to Tycho. The character of their connection with that crater is coarsely shown in the subjoined woodcut.

Fig. 7.

They consist of broad brilliant bands (seen in their proper splendour only when the moon is full) issuing from all sides of the crater, and stretching to various distances from their origin, —one of them can be traced along a reach of 1,700 miles. There are several *defining* characteristics of these bands. *First*, It is only when the Moon is full that we see them in their entire clearness. They may be traced, although very faintly, when the Moon is not full: their splendour at full Moon is very great. This cannot wholly be attributed to the effect of direct instead of oblique light, because at the edges of the Moon's apparent disc, on which the solar ray falls very obliquely at full Moon, their brilliancy is the same. No rational explanation whatever has been proposed, regarding this remarkable peculiarity. *Secondly*, The light thrown towards us by the rays from Tycho, is of the same kind as that reflected from the edge and centre of the crater itself: so that the matter of which they are composed had probably the same origin as those other portions of Tycho. *Thirdly*, These rays pass onward in thorough disregard of the general contour of the Moon's surface; nowhere being turned from their predetermined course by valley, crater, or mountain ridge. Now, this critical fact quite discredits the hypothesis that they are akin to lava, or that they are merely superficial. A stream of lava spreads out on meeting a valley or low-land, and forms a lake; nor can it ever overpass a mountain barrier. The question remains then, are these rays composed of matter that has been shot up from the *interior* of the Moon? It may seem incredible that we can solve this problem by virtually digging pits of vast depth down through those singular bands, and thus ascertaining practically that the matter composing them certainly descends towards the interior of our Satellite, and that in all probability it has been forced up from that interior. The telescope, which in this instance is our *labourer*, has discovered numerous small craters of varying depth in the midst of many

of the rays, and it reveals the fact, that these small craters, however deep, do not penetrate through the matter we are examining, inasmuch as there comes from their bases always the same kind of light that characterizes the ray. There is one remarkable case in point, rudely illustrated in the subjoined diagram. A large crater named SAUSSURE, and not far from Tycho, lies directly in the line of a ray, and of course appears to interrupt it; but at the bottom of Saussure, not-

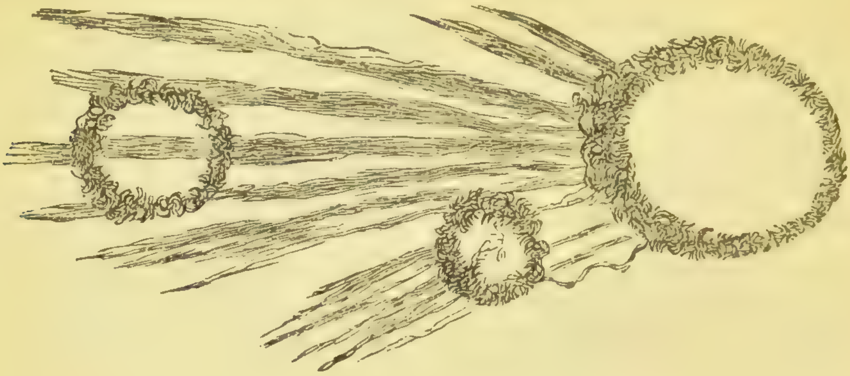


Fig. 8.

withstanding the great depth of that crater, the ray from Tycho may be traced. Nay, there is reason to believe that in favourable circumstances the same ray might be seen rising *up the sides* of Saussure, just as a vein of trap or of volcanic rock pierces the sedimentary strata upon Earth. What, then, can we make of such phenomena? Are not our terrestrial trap dykes or veins their fitting similitudes? Piercing the other rocks, as if shot up from below, these singular veins pass onward across valley and over mountain; their direction *their own*—independent for the most part of the rocks they have cut: they appear, too, in *systems*, some limited in magnitude, and evidently radiating from a known source; others of vast extent, and usually considered parallel, but probably owing their apparent parallelism to the fact, that we trace them only through a brief portion of their course. Accept this analogy,—and none other appears within reach,—and the rays or bright lines of the Moon assume an import quite unexpected,—they become *indices to those successive dislocations that constitute epochs in the progress of our Satellite.* In the next general section of the present article, we shall return to this subject.—In the meantime another indication furnished by the rays, demands notice. Reflect on the course, as to *continuous visibility*, of any stream of lava or any trap dyke upon the surface of the Earth. No lava current from Etna could be traced to any great distance by a spectator in the Moon, however powerful his telescope; and it would be the same with regard to those lines or dykes of trap, even supposing them endowed with an excessive power to reflect light. The reason is that they soon enter forest regions, and are concealed there, or become overspread by grass or other vegetable carpetings. But not even a lichen stains the brightness of the bands issuing from Tycho; they preserve, not their visibility merely, but one invariable brightness through their entire courses. The inference is but too clear; and we are glad to find a refuge from it, in the certainty that arrangements must be different on the other face of our Satellite. The existence of a rocky desert, devoid of life or living thing, of the extent of even one lunar hemisphere, is startling enough. But it is not from the examination of separate or isolated portions,—it is through their *co-ordination*—that the harmony of the Universe appears. The fabric surrounding us may be perfect as a vast whole; but until its entire and ultimate purposes are understood, we cannot expect to apprehend the essential significance of all its parts. Notwithstanding her clear destination towards perfection, Nature may embrace within her infinite variety, all possible forms, down even to extreme defects. And she comprehends accordingly the inhabited globe and the drear comet, the umbrageous hill and the frowning rock, the Sahara and the smiling plain—all Virtue and Vice.

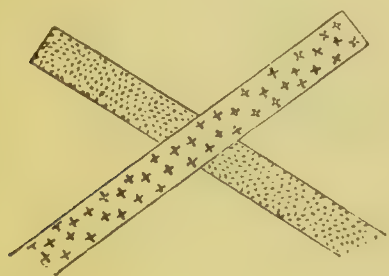
III. SPECULATIVE SELENOLOGY.—There are other features worthy of note among the results of the upheaving cause in the Moon—such especially as those long, narrow, and deep furrows or ditches (called by German selenographers *rillen*), which are found in different parts of its surface; but observation regarding their nature is not yet greatly advanced. And there is one special point of high import as well as difficulty, that urgently demands prolonged scrutiny—we mean the whole subject of lunar *levelling.* Although the heights of mountains and the depths of caverns are ascertained with singular nicety, in relation to some spot in their neighbourhood; no approximation has hitherto been made to the comparison of the heights of plains or plateaux: and we want accordingly one of the most essential of the bases of a satisfactory rational seleu-

ology. Nevertheless, some few conclusions may already be hazarded, if, under the express proviso, that the truth that is in them is only of the most general nature.—Two distinct questions seem to press for notice.

(1.) *The Seat and Mode of Action of the Lunar Disturbing Force.*—The results arrived at on this head are little other than negative. It is sufficiently clear, as already hinted, that the force which has disrupted the surface of our luminary is not a central force in the common acceptation of that word. No action or reaction of a solid crust and a central molten mass—in the light in which Humboldt contemplates that theory—could possibly have produced this crateriform surface. Neither can the force be such—supposing it local—as produced our terrestrial mountain ranges. The admirable researches of Mr. Hopkins of Cambridge, have connected the phenomena of parallel chains and their contemporaneity, with their most probable mode of upheaval. It seems that in our globe some large region—extensive both in length and breadth, like the plateau of central Asia—is gradually pressed upwards by forces from below, until it yields to the tension; when parallel and sometimes transverse refts occur, through which the mountain masses are protruded. Mr. Hopkins illustrates what he considers the facts of the case, by supposing molten lakes of limited but great extent, considerably below the surface of the earth, and that these—being further heated by internal causes—swell out and upheave the superincumbent crust. No such action, as we have said, can have occurred in the moon, or anything approaching to it; for although there are a few instances of mountain chains, they are wholly unaccompanied by one necessary consequence of such dynamical energy, viz., parallel chains.—There is no mode of action known on the surface of our planet at all analogous to that of the lunar force, unless we again refer to the case of *Craters of Elevation.* Now, whatever this force may be, it is not only essentially local, but confined within a very small circular area. The student ought especially to study on this point the classical memoirs of Elie de Beaumont and Dufrenoy, on the craters of Auvergne—memoirs elucidating the formation not only of these craters themselves, but also of the radial dislocations or rents with which they are necessarily accompanied. Other phenomena of our own globe unfalteringly attest the reality of very limited disturbances or upheavings. For instance, the local earthquake. Surely, these remarkable and well nigh incessant oscillations that heave up and down the small Scottish parish of Comrie, are no efflux of a central heat. Do but strengthen these oscillations; bestow on the imprisoned masses of elastic fluid, over which this northern district is uneasily reposing, a slight further augmentation of their natural temperature, and they will thunder and heave, and be repressed no more, but—converting Comrie into a *crater* or *Laacher See*—escape and be at rest. We conclude, then, that if the Earth, in the present phase of its development, presents in great profusion the results of an internal expansive energy diffused over a considerable area, the Moon stands forth in contrast, an eminent result of the workings of the same cosmical disrupting energy, but with its seat so limited in extent, that it may be said to act at mere points. How intense that action must have been! The rays issuing from the craters are merely a repetition on a mighty scale of the radial rents of Auvergne; but how mighty the scale! Assuredly, we have nothing on earth equivalent in degree to the explosive force that produced a Tycho, and at the same time shattered the lunar crust over all the space covered by these brilliant lines.

(2.) *The History or Development of the Lunar Formations.*—Considering the uncertainty that overhangs all speculation regarding the mode of action and ultimate nature of the upheaving cause on the Earth, it is no marvel that so little that is satisfactory has been ascertained concerning this remote and obscure question, when the seat of its force is in another planet. Fortunately we touch now on a more manageable inquiry. It cannot be concealed, that, up to very recent years, every problem connected with lunar cosmogony has been subjected to the same ill-considered and fantastic method of treatment which prevailed in the infancy of geological science. Nor is it marvellous that an early inquirer should discern in existing apparent confusion, nothing beyond evidence of terrible convulsion, or a relic of the condition of things that has been named *chaos.* But how admirably have the labours of science, by analyzing the objects before it, evoked from amid the seemingly lawless forms of the Earth, evidence of a long and solemn order. Those formless groups of mountains perplex us no longer—nor upturned strata, nor masses of contorted slate: nay, it has become an instinct with the sound geological Inquirer to reject at once whatever speculation presumes to go back to the origin of things, or is based on the assumption of universal cataclysms. But this grand achievement was virtually accomplished only when our terrestrial formations were arranged according to the category of *Time.* Is it possible to reach the same end, in relation to the mountain masses of the moon? Instead of settling down with some hypothesis regarding the escape of our satellite from this said chaos, may we not arrange its formations also according to their age; and thus begin, at least, to search amid its confused multitudes of craters, for traces of an order and progress that unquestionably exist? It may be asserted without presumption that this can be accomplished. The special method by which the relative age of a terrestrial mountain chain is ascertained, is evidently not applicable to the case of the moon;—no possible telescopic power shall ever ascertain the relations there, between different elevations and stratified masses.

But another principle, also of value and extensive use in our globe, is clearly applicable The geologist frequently requires to determine the relative ages of two or more contiguous masses of crystalline rock—say of granites; in which case the facts and reasons relied on are the following:—For the most part such rocks are accompanied by veins or arms emanating from their chief masses like the branches of a tree; and the *veins of contiguous masses often interfere with each other*—



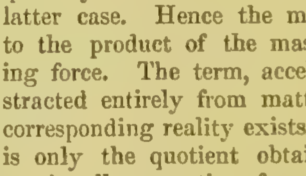
the one cutting the other as above. Now, it cannot be doubted that the vein cut through, must be older than the one which cuts it—the dotted vein than the crossed vein; and we pronounce, therefore, as to the *posteriority of the crossed granite.* Giving to this principle its widest significance, it may be termed the principle of the *intersection of dislocations.* But such dislocations are visible over all the lunar surface. Not the rays from craters merely, although these are abundant: other rents appear in multitudes,—for instance, that great one which passes nearly athwart the moon: and these dislocations interfere with or intersect each other. Of what sort of matter these rays are composed is not of consequence to this inquiry. That a fissure on the earth has been filled with trap, matters nothing as regards the age of the fissure; it might, in so far as that goes, have been filled by an upbursting metal. Neither ought the comparative smallness of terrestrial veins, and the impossibility of seeing them from the moon, affect the speculation now proposed;—if we see the dislocations, there can be no reason why we may not see their intersections. These intersections *are visible*, and in multitudes of cases it is easy to tell, through appreciation of the degree and colour of the light of the intersecting rays, *which set it is that uniformly cuts through the other.* The observations referred to demand large telescopes, and cannot be conducted with full effect unless under a clearer sky than ours;—one more reason for lamenting that through causes and influences not easily understood, the British government, so liberal generally in aid to science, rejected the recent proposal to establish at the Cape of Good Hope a reflector as large as Lord Rosse's. Here, too, a key may be found to the epochs of the retirement, to the opposite hemisphere, of the cis-terrestrial lunar oceans. The phenomenon spoken of is illustrated in fig. 2, plate III. As to positive results nothing as yet can be finally laid down. But it seems not improbable that Tycho is one of the oldest of the visible lunar craters; and that the mountain chains have been developed comparatively recently. It will, indeed, be strange should it turn out that the moon is merely a *younger* orb, growing, in so far as the upheaving cause is concerned, into a closer likeness to the earth. Long—unfathomable almost, our own geological history;—if the moon represents a period buried far deeper in the past than the epoch of the formation of azoic rocks, how stupendous the vista! At all events, let us search for results by this positive method, cease speculation about cataclysms, and learn that with the moon as with the earth, or with any physical history, it is delusive vain and presumptuous, to attempt to penetrate to a *beginning.*

Morgana Fata. See FATA MORGANA.

Motion, Laws of. See LAWS OF MOTION.

Moving Force may be defined to stand with reference to the momentum that it ultimately produces, just as accelerating force stands to the acceleration. Thus, two balls falling to the earth are subject to the same accelerating forces—though the one be twice as heavy: but the one will have twice the momentum of the other, and be acted on by twice the moving force. We must consider then, not only the amount of motion, but the quantity of matter in the body moved in this latter case. Hence the moving force is equal to the product of the mass by the accelerating force. The term, accelerating force, is abstracted entirely from matter, and, in fact, no corresponding reality exists. Accelerating force is only the quotient obtained by dividing the continually operating force (the moving force) by the mass. Frequently however, it is convenient to speak of the accelerating force generally, as entering among the data of any problem, independent of the mass of matter treated of. Thus $G = 32{\cdot}2$ is the accelerating force of gravity.

Multiplier—Galvanometer;—Galvanometer;—Rheometer. The object of all the instruments passing under the forgoing names is the same, viz., the measurement of the intensity of galvanic currents, strong or feeble. Their principle is also in common and as follows. If a conductor be placed *above* a magnetic needle, but only near it and parallel to its axis it makes the needle deviate to the east or to the west, according as the current is moving from north to south, or from south to north; and if the needle is *below*, the converse of the foregoing changes take place.



If then the conductor traversed by the current be bent into a rectangle as above, and the needle placed in the centre of the parallelogram, it is evident that this needle must be sub-

ject to *two*, not opposing, but concurring forces, and that its deviation, as due to the galvanic current, will be much greater and therefore much more easily discerned and detected than had it been influenced by one conductor placed either above or below it. Still further, if instead of bending the conductor only once, as above, it had been bent into a great number of continuous rectangles or convolutions, it is clear that the effect of currents on the needle might be made very great, or *multiplied* to a large extent. It is to M. Schweigger, a German physicist, that we owe the original idea of the *galvanometer-multiplier.* Many forms have been given to these multipliers —and various instruments have been proposed with different qualities. We can only indicate the chief of these, referring for special instructive details, to special treatises concerning the subject of *Galvanometry.*—(1.) *Nobili's Galvanometer Multiplier.*—In the case we have just imagined, there evidently remains to counteract the directive effect of the Galvanic current, the directive effect of Terrestrial Magnetism. Remove the latter, and the influence of the former will be felt singly and purely. M. Nobili proposed to suspend a second needle outside or over the convoluted coil, and of course in immediate proximity with the enclosed needle and parallel to it, —only with reversed poles, *i.e.*, the north pole of one needle being placed right over the south pole of the other. These two needles, connected in this way fixedly, and swinging on the same pivot or suspended by the same thread or wire, would necessarily form a system indifferent to the Earth's magnetic force, or as Nobili called it, an *astatic needle*; so that the entire amount of deviation would be a correct measure of the power of the circulating current. Much care is required to construct such an instrument; but the class to which this one belongs has been of essential service in advancing the study of the relations of Electric Currents. (2.) The *Sine Galvanometer*, suggested by De la Rive and perfected by Pouillet. See the standard works by De la Rive and Pouillet's *Physique.* (3.) The *Tangent Galvanometer.* This is the best of all, although somewhat expensive. See De la Rive and Becquerel. (4.) Becquerel's *Electro-dynamic Balance.* The name of this instrument indicates the principle on which it rests. It has been greatly perfected by Rhumkorff. (5.) Dubois Raymond has largely added to our knowledge of the habitudes of the *astatic needle*, and he has done much towards augmenting the sensibility of that instrument, so that it can detect and estimate very small currents. See, *inter alia*, the classical treatises by De la Rive and Becquerel.

Musca. The Fly. The name given by Lacaille to what Bayer calls the Apis. It is just below Crux; between it and the South Pole. Its largest stars are of the fourth magnitude.

N

Nadir. That point of the complete circle of the heavens which is vertically below us. If we suppose a man standing on the other side of the earth from us, his feet to our feet, our nadir will be his zenith—the highest or central point in his horizon. Like the zenith, the nadir is quite an imaginary point. There is none such physically. In truth, it is only the direction of the vertical line downwards, that we indicate when we speak of the nadir.

Napier's Analogies (Technical). Four formulæ for the resolution of spherical triangles, *i.e.*, for finding, from certain given elements, the ungiven elements of the triangle. The small letters, represent the sides, the circular arcs on the spherical surface; the large letters, the angles opposite to the sides marked by the same letter. The formulæ, which are very convenient for logarithmic calculations, and of great use in astronomy when we have to do with spherical triangles, are as follow:—

$$\text{Tang}\ \frac{1}{2}(b+c) = \cot\frac{1}{2}a \times \frac{\cos\frac{1}{2}(\text{B}-\text{C})}{\cos\frac{1}{2}(\text{B}+\text{C})}.$$

$$\text{Tang}\ \frac{1}{2}(b-c) = \cot\frac{1}{2}a \times \frac{\sin\frac{1}{2}(\text{B}-\text{C})}{\sin\frac{1}{2}(\text{B}+\text{C})}$$

$$\text{Tang}\ \frac{1}{2}(\text{B}+\text{C}) = \cot\frac{1}{2}\text{A} \times \frac{\cos\frac{1}{2}(b-c)}{\cos\frac{1}{2}(b+c)}$$

$$\text{Tang}\ \frac{1}{2}(\text{B}-\text{C}) = \cot\frac{1}{2}\text{A} \times \frac{\sin\frac{1}{2}(b-c)}{\sin\frac{1}{2}(b+c)}$$

Napier's Bones. The name given to certain pieces of ivory, &c., containing the products of any two single numbers, so contrived, that multiplication and division of large numbers may easily be performed by them: invented by the famous Lord Napier.

Natural Philosophy. A term, which in Germany is taken to signify the whole circle of the psychical and material sciences. In this country it is confined to the latter. The term as we use it is the simple English of the Greek word Phy-

sics, which is commonly employed to mean the same thing, *i.e.*, a summary of the laws which regulate the phenomena of material nature.

Nautical Astronomy. That portion of astronomical science which is applied to the determination at sea of LONGITUDE, LATITUDE, TIME, &c. See those articles.

Nautical Instruments. These are twofold. *First*, Those applicable to navigation strictly so called—viz., instruments for measuring the ship's way or the *log*, instruments for measuring the strength of currents, the vertical depth of the ocean, and the strength of winds. *Secondly*, Those applicable to astronomical observations at sea—consisting chiefly of reflecting instruments such as the SEXTANT, contrivances for determining horizon points, &c. To which may be added the COMPASS. Many improvements of the highest value have recently been suggested by Professor Piazzi Smyth, Astronomer Royal of Scotland. Reference is especially made to a very able and suggestive pamphlet recently published by this excellent observer.

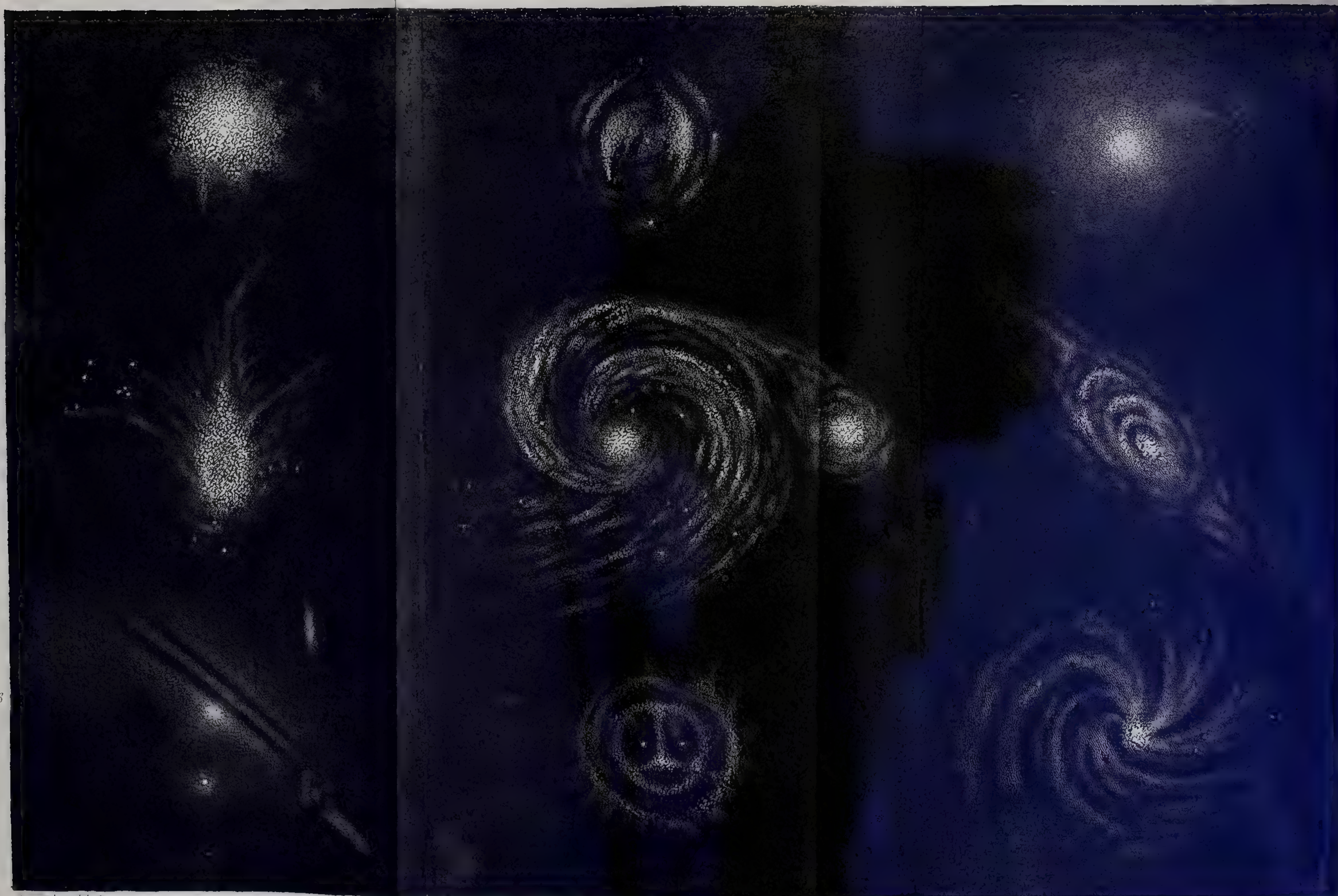
Navigation, as a mechanical *art*, consists of an account of the methods of handling a ship by means of its sails, &c., so that she pass through the waters along a certain definite course. An art difficult and of the highest importance. It virtually proposes to contend with winds and oceanic currents, by other mechanical contrivances. Into the details of this mechanical art, however,—so eminently possessed by the mariners of our own land and of one or two other countries,—we cannot enter here. But there are purely scientific requisitions besides. It is requisite that the navigator be able at all times to know accurately his position, or his latitude and longitude, even when long out at sea, in mid ocean. The true and only reliable way of effecting this accurately, is by the methods of Nautical Astronomy:—These are discussed under CHRONOMETER, LATITUDE, LONGITUDE, LUNAR DISTANCES, TIME, &c. But the sailor employs from day to day approximate methods; which alone, we intend very briefly to indicate in this place. The student is referred for details to the very excellent works on Navigation by Mr. Riddle and Lieutenant Raper. It is assumed as the foundation of all these approximate or tentative methods, that the Navigator knows the course on which he has run, and the distance he has run in a given time. For the knowledge of his course, he depends on his *Compass*, an instrument of the highest importance, but, whose variations—permanent as well as accidental—he ought to be able to correct for and compensate. The distance run is usually obtained by his *log line*—a contrivance showing the resistance encountered by the ship in passing through the waters, and thus furnishing a basis for calculation as to the distance passed over, or the speed of the ship. The log would be remarkably accurate if the ocean were quite at rest, or if, whatever its waves or oscillations—there were no *currents* in it. But so soon as the vessel falls within a current, the indication of the log becomes inaccurate, and requires to be corrected for the *set* of the current. Hence, the vast importance to the practical mariner of an acquaintance with the physical conditions of the ocean—a subject quite as important to him theoretically, as the subject of perturbations to the physical astronomer.—In what follows, however, we shall postulate that the mariner knows his course, and that he has corrected his log for the *set* of currents. The methods of deducing, from these corrected data, the actual positions of the ship, are mere approximations, and as follows:—

(1.) *Plane Sailing.*—This method supposes that the course of a ship has not altered; and further that the Earth is not a sphere, but a great *plane*. This latter supposition may be taken as nearly correct for a short space of time—say a day's sailing. Now, that day's sailing may be taken as the hypothenuse of a right-angled triangle, one side of which is the true difference of latitude made, and the other side what is named the *departure*, or the actual distance made east or west. This departure cannot of itself give the difference of longitude, because the lengths of degrees of longitude vary in different parts of the Earth. Still, by this easy method, a rough approximation can be had as to the ship's doings.

(2.) *Traverse Sailing.*—A ship seldom keeps the same course for any length of time,—that course often, through effect of winds, varies during a single day. This is technically called *traversing;* and the problem is to find the difference of latitude, and the true departure made during such a zig-zag course. The common method is easy enough. The various courses are computed separately upon the principle of plane sailing, and combined according to their signs, or as they are *plus* or *minus*. The difference of latitude made, may here also be pretty much relied on; but it is quite otherwise with regard to the difference of longitude. According to ordinary practice, all the *departures* are compounded according to their signs + or — ; but it needs no reflection to show that this method is utterly fallacious. Because of the spherical form of the globe, the departure at one latitude, signifies a totally different thing as to degrees of longitude, from the departure at another latitude; and yet, so strong is habit, so entirely does it dominate over knowledge—it continues the practice of the mass of our Navigators to deduce their true place as to longitude, from the simple average of these departures!

(3.) *Parallel Sailing.*—If a ship sails along a given parallel of Latitude, the difference of Longitude can of course be easily calculated, inasmuch as we know the length in miles of a degree of Longitude for any Latitude from Pole to Equator.

(4.) *Middle Latitude Sailing.*—It may be



Forms of Nebulæ

gathered from what has been said, that the practical difficulty is to deduce the Longitude arrived at, from the departures in traverse sailing. Middle Latitude Sailing is an ingenious, and upon the whole a very accurate device, by which the difficulty may be overcome. Suppose a ship traversing for many days from north to south, in a northern hemisphere:—her departure in miles, in the early part of her course, would give too great a difference of Longitude; while the departures in the latter part of the same course would give too small a difference. The average of the two might give very nearly the difference of longitude, for the ship's *Middle Latitude—i e.*, the latitude half-way between the point from which she started, and that at which she arrived. The ship's Longitude, when at that middle point, may thus be calculated; and so as she passes farther onwards. The method would be entirely correct if the meridians were truly *sides of a rectilineal triangle* whose apex is the pole. But as they are *great circles* instead, a farther correction is required,—a correction found in our best nautical tables.

(5.) *Mercator's Sailing.*—This is the most accurate of all existing modes of computing a ship's course. Its principle is naturally the same as that of Mercator's projection of the sphere upon a plane. The object of this projection is to exhibit the various parts of the Earth, upon a plane surface, under their actual relative dimensions and forms. This aim was not thoroughly realized;—hence, the value of the new *Homolographic Projection* of M. Babinet. Mercator's sailing aims at treating any set of ship's courses in any part of the Earth, on the principle of the Mercator projection; and the object is well accomplished by aid of corrections far from complex, supplied in appropriate tables. This mode of computation is certainly the nearest in accuracy, to astronomical methods; but, unfortunately, it is not extensively employed.—These are the chief methods of practical Navigation. But we cannot avoid noticing what is now termed *Great Circle sailing.* The object of a Navigator, is of course, to pass by the shortest route from one point on the Earth's surface to another. Now, that shortest route is a great circle of the sphere, passing through the two points. It is not possible for any ship to keep steadily on any line or circle; but if Navigators knew to aim at proximity to the great circle, much time and trouble would be saved. Tables have been published by the Admiralty under the title, "Tables to facilitate the practice of Great Circle Sailing," which instruct the practical sailor how to approximate as nearly as possible under all circumstances, to the great circle. These valuable tables were constructed by Mr. Towson, now of Liverpool.

Neap Tides. Those which happen when the moon is in the middle of the second or fourth quarters. They are low tides, the spring tides being the high tides. The lowest of all tides, technically called "deep neap," happens four days before the full or change of the moon. See TIDES.

Nebulæ. The name given to a class of objects which may certainly be termed the most extraordinary in the heavens. The nebulæ are invisible to the naked eye, the only exception in these northern climates being the great nebula in Orion and that in Andromeda—both sometimes presenting the aspect of a very faint and very limited haze. When first descried by the telescope these objects look like small milky spots. On the application of larger light-gathering and magnifying power, many of them are resolved, *i.e.*, they unfold themselves as clusters of stars. The additional power that resolves some nebulæ, discovers others having the aspect of milky lights; and to the great telescope of Parsonstown, which can penetrate so profoundly into space, there appear a great many more unresolved nebulæ, than can be seen by any other. Various points of importance are now under discussion regarding these memorable objects.

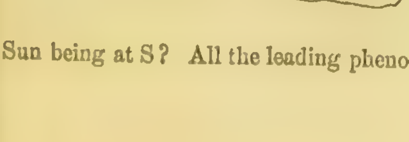
(1.) *The Forms of the Nebulæ.*—The extraordinary variety of forms assumed by the nebulæ will receive a partial although very inadequate representation from plate IV. Amid this singular variety two forms may be selected as more prevalent than any other. The *first* is the spherical form, or the approximately spherical. Multitudes of nebulæ exist of this shape, and, generally speaking, they manifest considerable concentration towards the centre. One brilliant instance of a resolved nebula of this sort, is the well known and most gorgeous cluster in the constellation Hercules. It is pourtrayed in fig. 1. Another form is that of fig. 2, where the central concentration seems to take place rather by starts, or breaches of continuity. Such a cluster would appear to a small telescope a mere nebulous star, *i.e.*, a star with a faint halo around it. It is scarcely possible to miss observing the very clear marks of the arranging influence of an attractive power within masses of such shape and characteristics as these. In all probability, also, vast orbital motions prevail, adequate to give them a comparative stability—to prevent or postpone that collapse which would inevitably issue from the unchecked agency of central forces. The *second* prevailing form is the *spiral*, illustrated in three figures in the plate, viz., 4, 6, and 9. The lustre of some of these forms, when seen through the great telescope of Parsonstown, is unparalleled; they strike astonishment into the most callous beholder. Impossible as it is to conceive what forces are engaged in sustaining shapes so strange, it is equally impossible to disregard clear intimations of a *gradation* of shape. Fig. 4 occupies a sort of middle place in the series. We have other shapes like that of fig. 9 where the spiral is much opener, and, on the other hand, there are brilliant instances in

which the spiral arms have quite closed in and formed an almost regular and continuous oval figure. In presence of such gradations of form, the idea of *progression* almost necessarily suggests itself. Are these gigantic masses in a condition of change or flux? A question as yet far from decision; but the foundation for ultimate decision is being laid by the labours being now performed at Parsonstown—viz., the *micrometrical measurement* of all the leading nebulæ. Fig. 6 is another spiral form. Attention has been arrested, too, by that very remarkable nebula in Andromeda, represented in fig. 8. The singular feature of it, consists in these two dark spaces or patches; is it not likely that Andromeda is also a spiral nebula, but seen from *above*—not from the side? If it were a spiral of the kind of a *helix*, these dark spaces might be at once understood.—Besides these leading forms, others, frequently of the most capricious nature, abound in the sky. Look for instance at fig. 5, or still more at fig. 7. How extraordinary an arrangement! This latter is a specimen of the real shape of what once were called *planetary* nebulæ—*i.e.*, nebulæ with discs of equable light, in which there is no trace of condensation. Many theories were proposed to account for such aspects: the true solution came at length from Lord Rosse,—*we had not seen these objects aright;* they are hollow nebulæ, with streaks partially through them—hence their apparently equable illumination. This is one among many instances of the danger of speculating on the apparent shapes of these shadowy appearances as if they were their real shapes. For instance, fig. 3 is the once famous dumb-bell nebula, as that is seen in the six-feet reflector.—*Ring* nebulæ are not uncommon—a fine illustration being in the constellation *Lyra*. But the greater number may certainly be styled *amorphous;* the most gigantic instance of which is unquestionably the grand *Nebula of Orion*, easily discerned with a comparatively small telescope. When, however, this object is regarded by an instrument of adequate power, it baffles all description—spots of light irregularly diffused—the mass of it passing away into utter faintness, probably through effect of the profundities into which it pierces—arms and branches of the most fantastic form and arrangement, stretching out one knows not whither! We have not as yet obtained a full figure of this nebula from Parsonstown; but M. Lassel sketched it at Malta, and a finely executed copy of the drawing made at Cambridge near Boston, U.S., is given in Professor Nichol's *Architecture of the Heavens*, ninth edition, *Bailliere*, London. It may be remarked that this volume contains the most effective engravings of the nebulæ, that are to be found in any accessible work.—The classical volume on the nebulæ of the Southern Hemisphere, is Sir John Herschel's *Results of Observations;* but the whole of this portion of the heavens ought to be re-surveyed with a much more powerful instrument.—See also Lord Rosse's various memoirs in the *Philosophical Transactions*.

(2.) *Constitution and Distances of the Nebulæ.*—This is the obscurest portion of the *Sidereal Astronomy*. It was a strong opinion of Sir Wm. Herschel's—the ever illustrious founder of the whole of this portion of astronomical inquiry—that there are two distinct classes of nebulæ; one class consisting of clusters of stars, so remote, that nothing but their milky light can be discerned; and a second class in which the matter is not stellar, but a confused gaseous substance, not unlike the matter of comets. The ground on which this eminent man thought that the distinction could be established, was—in general terms—the following:—A certain proportionality appears to hold between the power of the telescope that first descries a cluster as a milky spot, and the telescope that succeeds in resolving it. But there are objects with regard to which there is no such proportionality. The nebulæ of Orion and Andromeda, for instance, may be said to be visible to the naked eye; while no power that even Herschel had at his command, was adequate to resolve them. Now, their irresolvability—on the supposition that they consist of multitudes of ordinary stars—argues their enormous distances; while their visibility to very small telescopic power, would under such circumstances indicate an amount of compression of the separate orbs of which they are composed, that has no parallel in known regions of the heavens. That phenomenon—an apparent one only—of the *nebulous stars*, already alluded to and explained, first suggested this idea to Herschel; and, confirmed as above, he erected upon it his famous and captivating theory, that this nebulous matter is the rudiments of stars, and may actually be traced through its progress of condensation: (see his celebrated memoir of 1811). It is very clear that this separation of the clusters into two sets, or the acceptability of the doctrine that there is nebulous matter which is not stellar, does not require, in order that it be legitimately negatived, that all clusters, or all the milky light belonging to every cluster in the heavens, should be proved resolvable. Our telescopic power may always continue inadequate to such an achievement. It must wholly suffice, if the logical distinction just described, be broken down in any critical case. And this was the feat of the Reflector at Parsonstown:—it unravelled the mystery of the Nebulous Stars, and it resolved that nebula of Orion, on which Herschel rested *par excellence*. Notwithstanding this thoroughly satisfactory disposal of the question, a disposition still lingers with many observers to imagine the existence of such nebulous matter, simply because there appear masses of it connected with the resolved nebulæ. Certainly, so long as any portion of pure milky light remains in the heavens, an abstract possibility may be alleged on behalf of the old idea; but is it not much more natural as well as just,

in the present state of our knowledge, to refer it to the fact that portions of these galaxies stretch far behind the others? Our own milky way, for example, manifests *stars* to the naked eye, as well as the light of others that are far more remote; nor, perhaps, can our best telescopes resolve that zone in all places: nevertheless, we never doubt that it consists of stars?—But even conceding the fact that all nebulæ are resolvable, another question of deep import arises:—are the shining points composing them, veritable *stars*, as we understand that term, or not rather masses of meteoric bodies, somewhat akin to our asteroids? It is impossible to limit, even in thought, the infinite variety of the universe. Our own planetary system teems with such variety; and we know besides, that the fixed stars constituting our galaxy, are not all of one magnitude. That a certain average magnitude may be predicated of them will clearly appear when we treat the details of their distribution (see Stars), and certainly it seems at first sight the probable hypothesis, that the congerieses of orbs which we term the nebulæ, are composed in a way similar to the gigantic system to which we belong. There is but one way of finally resolving this doubt. If the nebulæ are akin to mere meteoric congerieses of comparative atoms,—they cannot be far removed from us, and careful observation will speedily determine their *parallax*. If, on the other hand, they are veritable firmaments, they must be seated so profoundly in space, that light with its almost magical velocity would not traverse the chasm that separates us from them in less than millions of years.—The whole subject however, must be referred to the future.

(3.) *Our own Galaxy, or Nebula, or Firmament.*—The idea that the grand stellar system to which our Sun belongs, is a finite cluster, with boundaries and even ascertainable shape, is one of those memorable advances for which Astronomy is indebted to Sir William Herschel. The cause of the Milky Way, or rather the question as to the true physical phenomenon indicated by it, very early arrested this remarkable man. Why, he asked, do the stars vanish away from us, and seem in such innumerable multitudes when we turn the eye or the telescope towards the Milky Way, while at the *sides* of our Firmament or cluster, they seem thinly scattered? Is it not, hence apparent, that if we made a section of that cluster, through the position of our sun and at right angles to the plane of the milky way, that section would have some such general shape as this,—



the Sun being at S? All the leading phenomena receive in this way an easy explanation. Looking backward or forward, the eye would be lost in the maze of stars, while the sides of our starry stratum would seem comparatively shallow:—the bifurcation of the stratum, corresponds with the fact that through about one-third of its course the milky way separates into two branches. Herschel did not rest, however, with this general speculation. He employed his great telescopes to gauge the heavens, to sound their depths, and ascertain their limits on every side; and on the ground of these elaborate measurements he adventured a somewhat minute sketch of shape of our galaxy. No signal advance has been made in this direction of inquiry since Herschel's time. Struve's *Etudes Stellaires*, are somewhat encumbered by questionable hypotheses; and he quite overvalues a casual remark by Herschel, that in a certain direction the Milky Way seems unfathomable.—See for admirable sketches of a large portion of this zone, Sir John Herschel's *Results of Observations.*—But Herschel thought of one section only, of our sidereal system. Suppose that we look at it sideways so that we could see the Milky Way as a ring? Is it not exceedingly probable that it would then appear as a spiral, of the kind of figure 4,—that zone itself being merely the branching curvilinear arms? If such conjecture should ever be verified, the question as to the character of these other imposing masses will be decided independently of the difficult research of parallax.—For details see Stars, Distribution of.

Nebular Hypothesis. It is our purpose to give in this article a general account of the famous Nebular Hypothesis of Laplace, in which he endeavoured to lay rational foundations for a cosmogony of the Solar System. It scarcely requires to be premised that on subjects of this nature, the highest evidence that can be attained is a degree, greater or less, of *probability.* On recurring to Gravitation, wherein the *foundations* of Newton's law are exposed, the reader will discern that the breadth of these is no greater than the three general principles in reference to the orbits and motions of the planets, whose discovery will ever distinguish the memory of the illustrious Kepler; and that not one of those other remarkable arrangements, noticed under Solar System, enter among the considerations to which that law owes its origin. For instance, Gravity, as there explained, takes no note of the fact, *that the various orbs, primary and secondary, move in ellipses approaching very nearly to the circular form;* nor of the fact, *that all these orbs revolve in the same direction around the Sun* (*excepting the satellites of Uranus*); nor of the fact, *that they all rotate on their axes in that same direction also;* nor, finally, of that equally singular ordinance, which has confined so many bodies *within a very brief distance of the plane of the equator of the Sun.* The comets are known to transgress every one of these laws which could

be applicable to them, and yet they are sustained through all their devious courses by the power of gravity as unerringly as the planets are: so that it appears a necessary conclusion, that the cause of the foregoing arrangements is something profounder even than Newton's principle—perhaps some remotest fact in the history of the universe. —It is possible, that on the threshold of the investigation before us, it may occur to a certain class of minds, that the laws or arrangements we are seeking to explain are really *primary*, and therefore incapable of explanation. It may be said, for instance, "These are merely expressive of the manner in which our planetary system was at first constructed; indications of the order stamped upon it by the creative fiat." The times indeed have long gone by, in which any sound or logical thinker would be disposed to fancy that an explanation of the mystery of the external world can be reached, merely by referring existing events and appearances to some prior physical condition; but it requires to be emphatically noticed, that the feeling which demands, as the necessary *substratum* of all that is comprehended within space and time, the existence of one Absolute, Unchangeable, Causative INTELLIGENCE, contains no element whatsoever entitling one to declare that at such or such a point any special stream of succession arose. Why, indeed, or by what power or faculty of the human intellect, can any one be authorized to assert, that the special condition in which it appears now, was the primary condition of the solar system? There is a creature named the Ephemeron, which is born and dies within an hour. Suppose that creature endowed with a momentary reason, and examining the exquisite arrangements of a blossom — the adaptations of its complex and innumerable vessels, and the wonderful evolution of that beautiful colouring,—what chance is there that a creature so evanescent, unless it had arisen to our loftiest views concerning unfathomable Time and the awful grandeur of Creation,—what chance of its conceiving that this blossom ever had a bud, far less that the tree which bore it sprung from an insignificant seed? And as Man gazes on that resplendent solar system, what is he save an Ephemeron? The element of difference is nowhere in the objects contemplated, but in the relative powers of the contemplators; and surely —notwithstanding the heights and depths of the boasted vision of our race—it is possible that the vastest durations it ever can explore are not, in relation to what *exists* or to the glance of the INFINITE MIND, more imposing than the solitary tick of a clock, which is heard and passes!—Is it indeed possible, then, that those remarkable arrangements, lying *beneath* the operations of gravity—the constituent or fundamental principles of a system, whose motions or present vitality gravity merely upholds—are indications of the mode of that system's origin, and flow necessarily out of dark and far-back processes which evolved it? Had this consummate fabric—this gorgeous planetary scheme—a bud, like the blossom? and, deeper yet,—can *we*, by a study of its existing structure, reach that mysterious germ within which, as in a chrysalis, rested the necessities of its present glorious unfolding? Questions before which the boldest imagination may well start back appalled, and to which our human reason, even in its highest strength, may never yield reply clothed in other forms than those of reverent conjecture!—The speculation about to be unfolded—the only one which has hitherto thrown the remotest light on what is manifestly not only a legitimate, but an urgent physical inquiry—constitutes the NEBULAR HYPOTHESIS of Laplace. This remarkable *hypothesis*—for that is its proper appellation—having been egregiously misunderstood and misrepresented alike in itself and its tendencies, and in directions the most opposite, we shall use every endeavour to present it as it really is, apart from all that is factitious.

I. The logical foundation of the speculation in question is, as it now stands, essentially *hypothetical*. But the supposition at its base is not an unwonted one, nor in the least strange to those schemes of knowledge which are concerned with ultimate facts. How often, for instance, has the geologist had recourse to the tenet of the original liquidity of the earth—drawing from that hypothesis the solution of much that at one time seemed otherwise inexplicable? It is not our present intention to offer any opinion on that tenet; we merely assert that it has been received, and been frequent subject of permitted discussion. But between this and other similar instances, and the fundamental assumption of Laplace, there exists a difference of *degree* only; viz., the distinction depending on the superior grandeur and remoteness of the latter. Rising to the thought of the probable origin, or rather of the probable proximate condition, not of solitary planets, but of our entire system, the illustrious French geometer (sustained also by the metamorphoses which recent chemistry had converted into realities out of dreams) brought within the grasp of his powerful conception the idea of a huge, chaotic, nebulous mass of matter, akin to the cometic, resolving itself during myriads of ages into an organized form; and, upborn on the wing of his most wonderful genius, he hovered aloft among the heights of space over the scene of this mighty germination, and at last beheld its issue in the Sun and his gorgeous train!—The assumption, however, of a Nebulous mass slowly condensing, does not quite exhaust the hypothetical portion of Laplace's speculation. That Nebulous mass must have had a certain prior condition—one for instance of rest or of internal agitation. Now, on looking at the system supposed to result from it, we discern as its special characteristic a grand *motion of revolution* in one direction; hence we are constrained to infer, that the original nebulosity must

have been distinguished by a sort of whirlpool or *vortical* motion—vague probably and indefinite, but still with a marked determination in one direction. This, however, is not wholly an assumption. If any internal agitation existed of whatever kind, it might be shown by the laws of mechanics, that when united with a process of *condensation* or of motion *toward a centre*, that internal agitation would, as its most probable issue, resolve into a motion of revolution. But in treating of processes so infinitely removed from vision and distinct knowledge as these, it is less fitting to confide in any dogmatism than to warn ourselves of their uncertainties.—These are the *assumptions* on which Laplace based his speculation: the remainder of his course is one of strictest deduction.

II. The conception before us, then, is the following:—Suppose a huge, dark, chaotic, void, and formless mass, determined towards a certain *vortical* or whirlpool motion, resting somewhere in space; and that within its atoms the tendency which we call attraction universally inheres. In consequence of that mutual attraction, the mass must condense; in other words, there must be a steadfast, however slow, stream of matter towards its central regions, proceeding through all time and issuing in some novel condition. It is our aim to determine what that condition most probably will be.

(1.) It is not difficult to perceive, that—the whirlpool motion once originated—there is an inherent power in matter, under such circumstances, to evolve finally a definite rotation *of considerable velocity*. Several well-known facts amply illustrate this. Every reader must be aware of the effect of shortening a pendulum, or of bringing the moving mass nearer the centre of motion. The motion is instantly *accelerated;* and it is the same with the little instrument the metronome. The moment at which the pirouette-dancer desires to increase the velocity of his whirling, he brings his arms, previously perhaps stretched out, close to his sides; and this enables him to perform the more rapid gyration without increased effort. But the truth, that the approach of the matter towards the centre, or the *process of the condensation of the nebula*, must gradually, and surely increase the velocity of its rotation, may be illustrated very plainly by a reference to simple mechanical principles. It is a general law or fact, that if a body is subjected to two moving influences acting in different directions, *it obeys both*, or moves in obedience to each, as if the other did not exist. For instance, let a person in a boat start from A, with the intention of pushing his boat right across a stream; and suppose that in the time he would occupy in rowing from A to B *if the waters were still*, the power of the stream could carry him from A to C *if he did not row at all;*—the question is, how his boat will actually move? Now, all experience tells us, that if he merely rows *right across*—pushing his boat from the bank A towards B, with its side invariably to the stream, he will, previous to reaching the opposite bank, have been

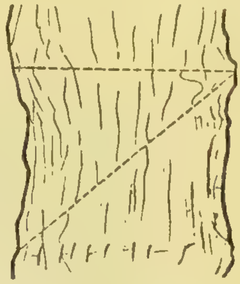


Fig. 1.

carried down by the current precisely as far as if his boat had merely floated; so that he will reach the bank at D, immediately opposite C, and his boat will have *partaken at once of the two motions or influences impressed upon it.* This law, as we have said, is general; it holds in every case where a body is under the influence of two moving powers; and the consequences of its action in a condensing nebula cannot be mistaken. Let the subjoined sketch represent a section of a circular nebula, revolving about the central region C, and in which condensation is permanently going on. It is evident that the particle at A moves, in consequence of the whirlpool, from A to B, while the particle at A′ only moves from A′ to B′; but as the attractive power, by drawing the first particle from A to A′, cannot, by the foregoing principle, *diminish its circular velocity*, the result of such condensation must be *the attaching to* A′ of another particle A, whose circular velocity is greater than its own. Now, the permanent consequence is manifest. If two balls, A and B for instance, are moving forward with

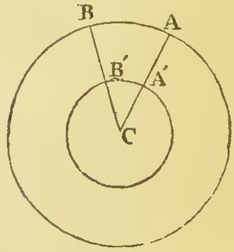


Fig. 2.

Fig. 3.

different velocities, A much faster than B, what will follow when A overtakes B? Certainly an acceleration of B's motion, and a retardation of A's; and the two together will, after contact, move on *much more rapidly than at* B's *former rate:* so that, by the very act of A (see previous diagram) being brought into union with A′, the rotatory velocity of A′ would be augmented; and

if the whole *outer* circle A B, &c., were attracted towards the inner circle of matter A′ B′, &c., that inner circle would accordingly rotate more rapidly than before, and the velocity of the rotation of the entire nebula must therefore be increased. Plausible objections, we are aware, may be taken to this explanation;—it is proposed merely as a *popular* one: but it indicates, nevertheless, the principle assuring us that the condensation of a diffused and comparatively slow whirlpool cannot take place without a great and growing increase in the velocity of its rotation, inasmuch as the momentum, or amount of the *rotatory force*, must in all its stages and conditions continue the same And thus may it clearly be seen, how, out of phenomena the rudest and most unpromising, and by the simplest laws of nature—those which guide the facts of everyday experience,—even that stupendous rotation might be generated—a rotation whose discovery was one of the first achievements of the telescope, and which, all who know Nature ought to be assured, does not stand by itself or as an independent fact, but is a cosmical phenomenon of wide significancy, and closely, however mysteriously, related with the whole scheme and progress of Things.—A conclusion here forces itself on our notice of the most unexpected kind. It may be anticipated from the previous speculation, that the individuals of the double stars also rotate on their axes; and in fact that they also have arisen, like the sun, from revolving nebulæ. Now, observe how the revolutionary or orbital motions of such systems would flow at once out of this hypothesis! The solution of the great and interesting fact now referred to, cannot indeed be termed less than picturesque,—it excites instantaneously our surprise and admiration. Has our reader walked in a mood of tranquil thought along the side of a quiet river, whose waving banks reflect a thousand currents, by the intermingling of which numerous dimples or whirlpools are produced—their easy course only marking the river's stillness? Has he followed these dimples as they pursue each other in gambol, and watched the phenomenon of the near approach of two or three? Then may he have witnessed the secret of the mystery of the double and triple stars! When one of these dimples approaches the vortex of another, the two begin *to revolve around each other;* and in fact they must, on approximation, act upon each other as TWO WHEELS; so that a revolution of each around the other *must* immediately supervene, and increase in rapidity, until by external pressure they are forced into one. If such single neighbouring nuclei were rotating, it would be precisely a case of two contiguous whirlpools; and *how could revolutionary motion be prevented?* Two such masses in approximate contact *must* originate such a motion: as the principle of gravity draws the nuclei nearer each other, the velocity of revolution must manifestly increase; and the two bodies would constitute themselves into a stable system when the rapidity of revolution sufficed to counterbalance their mutual attraction. The case would manifestly be the same in instances of three, four, or more nuclei, formed in the immediate neighbourhood of each other: so that *now* we may have a glimpse not only of the CAUSAL solution of Herschel's remarkable prophecy, but also an intimation that the *modes* of revolution of small clusters may be as varied and fantastic as the multifarious revolutions of associated dimples in a stream.—We are not sure that the portion of the nebular speculation, over which we have just gone, is not the most engrossing of the whole of it, for it points emphatically to a moral we are extremely anxious to impress. We are all too easily inclined to look on creation as made up of isolated parts—of independent or individual classes of beings—and to regard Nature as we do a case of botanical or mineralogical boxes; so that it requires a fact as striking as the identification of the Stellar motions of REVOLUTION with those of ROTATION, to startle us from the habitual error, and to bring us to right views of that stupendous ORDER within which we live, and of which our own beings constitute a part. The unity of things—their interdependence—their adjusted relationships—are proclaimed by every department of the Universe. We do not deny that different laws may exist; nay, they *must*,—for it is only by the commingling of Opposites that Variety and Progress can be produced; but all is not opposition which seems so, and most of what we divide and parcel out into isolated bundles, is nothing other than the parts of the same grand scheme. Philosophy has taught this for ages—it is, in fact, the secret of her life; for she aims to gather up all fragments, and to present the Universe united, compact, tending to one end—a type of its August CREATOR.

(2.) We now enter on the most difficult part of our speculation,—viz., the question as to the origin of *planets* connected with our central Luminary, and characterized in their arrangements by the existing peculiarities. We shall separate the investigation into several distinct steps.

a. The preservation and permanence of the place of a point on the surface of a rotating body, depend on the circumstance that the centrifugal force is not greater than the power of the central attraction. The inevitable consequences of an excess of the former are seen in simple operation in a common phenomenon. It is known to Mechanics, that a grindstone may be made to revolve with a rapidity sufficient to cause splinters fly from its rim, and even the whole rim to break in pieces—indicating that the centrifugal force of the rim with that velocity, more than counterbalances the mutual attraction or cohesion of the particles of the stone. Now if the rim, instead of being formed of brittle stone, had consisted of an elastic belt, say of caoutchouc, what would result in such a case? Clearly a separation of

the rim from the mass of the rotating body; it would expand somewhat, just as the orbit of a planet in a similar position; and, if other circumstances permitted, it would revolve around the stone as a separate ring at a distance where the balance or equilibrium of the forces would be restored. Let the attention now rest on the following diagram: —

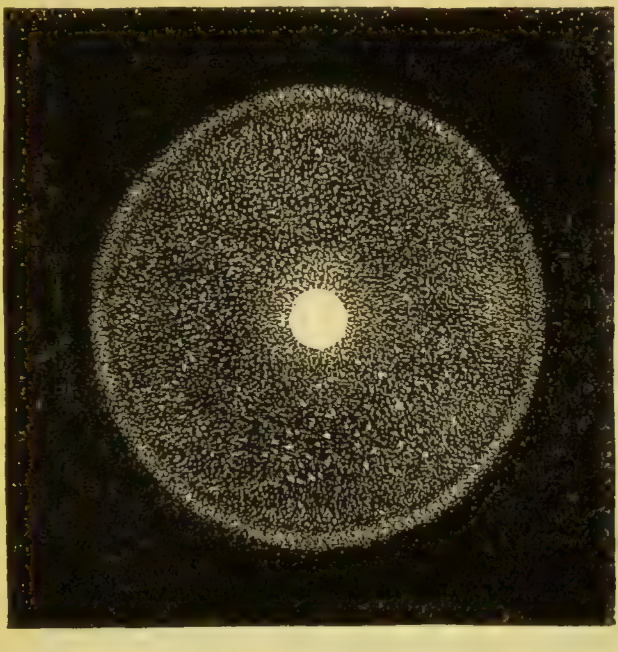


Fig. 4.

We have already seen that causes continually operate to increase the velocity of the Nebula's rotation; but when this velocity in any case became so great, that the centrifugal power of the exterior portion or ring just balanced the attraction exercised over it by the mass of the Nebula

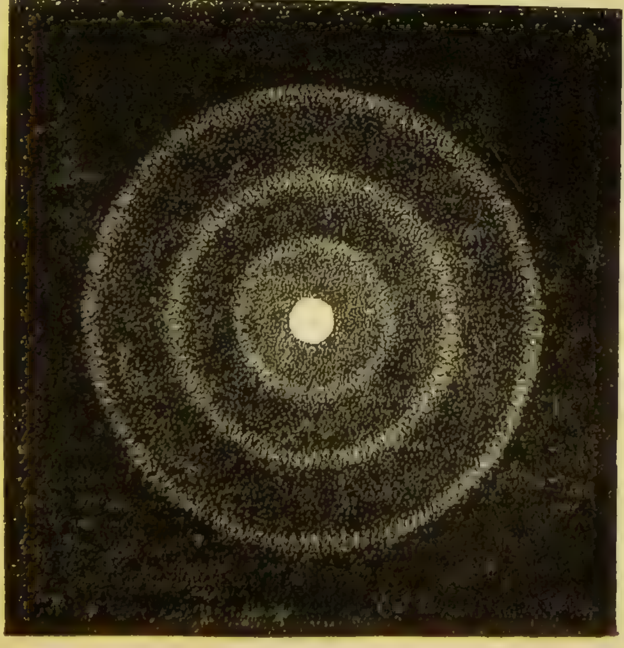


Fig. 5.

that ring would necessarily assume an independent character, and acquire, so to speak, a self-sustaining power; it would therefore be *abandoned* by the main or parent mass at the next stage of condensation, and left as a *distinct portion of matter revolving in* SOME FORM *around the central body.* There is no doubt whatever of the mechanical principles on which these inferences rest; and it is equally certain that there are almost infinite chances against the condensation of any large or original Nebula, without the occurrence of circumstances which would throw off numbers of such rings; so that, in a more advanced condition, every such mass might (*if the forms of the thrown-off rings had not altered*) present the appearance, as on the preceding page, of a large central nucleus, with subservient rotating annuli, composed of quantities of matter necessarily very small when compared with the main body.

b. The next step of the inquiry might naturally seem to be this—What is the probable ultimate condition or form of these rings? We clearly obtain from them our first conception of the origin of *planetary* or *parasitic* matter connected with a central globe; but we require still to determine the shapes which this matter will most probably assume. Before taking up this point, however, we must ask the attention of our readers to the light already cast on these fundamental or constituent arrangements of the solar system, whose apparently inexplicable character urged us on this investigation. It is not difficult to see that a large part of what seemed so puzzling, has already become quite intelligible.

First, As the separation of the rings resulted from the centrifugal tendency of the particles composing them, and as this centrifugal tendency must always be greatest at the equatorial region of the rotatory mass, the rings must all lie *nearly in the plane of that equator.* Therefore we are entitled to conclude, that into whatever forms or bodies these rings may ultimately be resolved, *these bodies must all lie nearly in one plane—the plane, viz., of the equator of the central globe.*

Secondly, The rings being *circular*, or, what is the same thing, the motion of each particle composing them being circular, the *orbits* or paths of whatever bodies are ultimately formed out of them, must *also be nearly circular.*

Thirdly, As the rings must continue to move as the nebula was moving when they were abandoned, *the planets* into which they may be resolved *must all move in the same direction—that, viz., of the rotation of the central orb or sun.*—Our subject is thus rapidly simplifying. We have already—even at this stage—deduced from this memorable hypothesis the necessity of the *principal three* of those fundamental arrangements which gravity could not explain. But let us proceed.

c. Resuming our direct investigation, we inquire now what forms would such rings most probably ultimately assume? There are three possible forms:—1. The mass, if tolerably equable in its original constitution, and undisturbed from without, might settle down into a *rotating* RING; but the chances against such a result are so numerous, that we would expect the phenomenon to be very rare in the Universe. 2. If the mass broke up or separated while condensing—as its own internal irregularities would, in all probability, constrain it to do—it might divide into a number of portions so equal in attractive energy, that none of them would have any tendency to coalesce with, or fall into the others; so that the ring would ultimately be transformed into a number of distinct small solid bodies, revolving around the central mass at nearly the same distance from it. 3. Even this second supposition, however, is not a very probable one, inasmuch as its essential condition—the separation of the mass of the ring into *equally balanced* nuclei—could, in the nature of things, occur but rarely. By far the likeliest result is the division of the ring into nuclei of unequal power—the larger of which would, by its superior attraction, assume the others into its mass,—the whole solidifying into one considerable globe. Observe the correspondence between these hypothetical results and the character of the bodies in our solar system! *First*, We have a central massive globe, with subservient globes engirdling it, at various distances, and of magnitudes very inferior. *Secondly*, The great proportion of the planets which compose our luminary's *cortège*, belongs, in accordance with theory, to the last of the three defined classes of forms into which a ring might break up. MERCURY, VENUS, the EARTH, MARS, JUPITER, SATURN, URANUS, NEPTUNE, are single globes, revolving in orbits of their own, and around some of them are dependent satellites. *Thirdly*, In one instance only would the ring seem to have divided into balanced parts—we allude to the group of small planets, those ASTEROIDS between Mars and Jupiter which have nearly a common orbit, or which revolve at almost the same distance from the sun: and *Fourthly*, We have also, in one solitary instance, a specimen of that most singular of cosmical appearances—A RING nearly in its pristine condition, and revolving around the planet SATURN.—Other facts regarding this remarkable appendage are given under SATURN.

d. The general laws characteristic of the planetary system, and the forms of the bodies composing it, being thus apparently direct consequences of the Nebular hypothesis, let us next inquire whether our speculation throws a corresponding light on the condition or attributes of the several planets? Now, we think that a slight consideration will make it evident that these globes must all rotate on axes, and also in the same direction in which they move in their orbits. Let us reflect on the true condition of a revolving ring, as illustrated by the diagram on next page.—Seeing that the ring *as a whole*

must go round in the same time, it seems very plain that the particles of the outer rim move with a greater *absolute velocity* than those of the interior rim; and, therefore, that the relations of the two rims can remain fixed only *so long* as the ring shall continue *entire*. Suppose the ring broken up, and observe the condition then of any section of it, A B D C, the course of revolu-

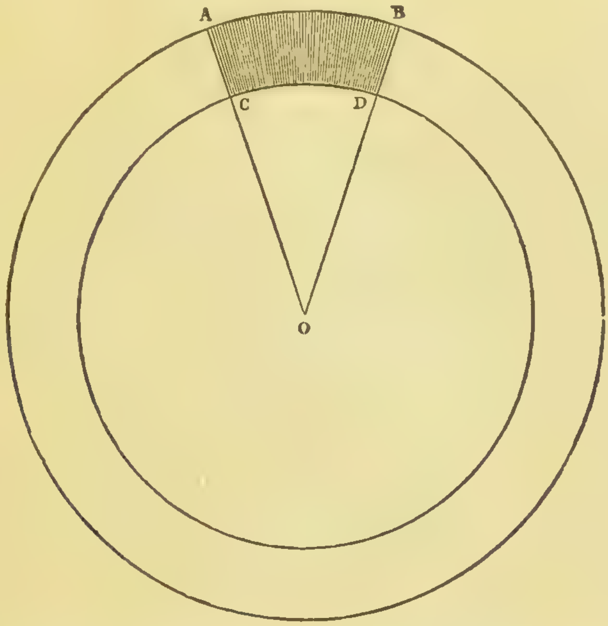


Fig. 6.

tion being supposed from B to A. It will be seen at once that the velocity of the outer part is such as to carry the particle B to A, in the same time that D is carried only to C. There is a momentum or force therefore, in the outer part, in the direction B A, much greater than the momentum of the inner part in that direction: the former must prevail therefore, and the segment, as it continues to revolve around O, will necessarily assume also a *whirling* or *rotating* motion in the direction B A C D; in other words, the broken ring, or any body evolved out of it, *must rotate on an axis in the direction of its revolution.* But still further:—Not only is this remarkable law of rotation a distinct result of the speculation of Laplace, but that most anomalous portion of our system, viz., *the varying periods of the rotation* of the planets, appears to receive a singular elucidation. The primary cause of rotation being manifestly the excess of the velocity of B A over that of D C, its degree of swiftness will depend on the *magnitude of that excess*; in other words, on the *breadth of the originating ring.* Now, the farther off any ring lies from the central mass, its substance must be the *rarer*, or of inferior density; *i.e.*, a larger amount of it would be required to compose a globe of given magnitude. But it would be easy to demonstrate, that, beyond the middle line of the Asteroids, the outer portion of the condensing Nebula must have been attenuated almost to an infinite degree; while within that line, especially at the orbit of Mercury, the same extremity would be comparatively dense. Look next at the *magnitudes* of the two classes of globes on either side of the line—the group of large orbs all lying *beyond;* and surely it cannot be questionable that the primal rings in that farther region, must have greatly exceeded in breadth the same class of forms which gave birth to MERCURY and VENUS, the EARTH and MARS. Here, then, appears the secret of the singular contrast in the rotations of the two systems of planets; and as *this* concordance, at all events, could never have been foreseen — in nowise entering among the fundamental conditions of our hypothesis—it assuredly yields no slender support to the views of the illustrious Frenchman.—See SOLAR SYSTEM.

e. It is abundantly plain that what has already been demonstrated contains the solution of the problem of the Satellites. Thrown off by the primary planets, as these had been thrown off from the Sun, their revolutions must correspond in direction with the rotations of their central globes; they must lie near the plane of the respective equators of the planets, and they must rotate on their axes as they revolve in their orbits. Our remarkable hypothesis, therefore, has left none unvisited of those difficulties which pressed on us; all the general arrangements of the system, unexplained by gravity, have emerged from its capacious womb. Strange indeed the thoughts with which, in presence of such speculations, we must gaze on these brilliant skies!—

Even that jewellery of midnight—a birth, a thing of yesterday, a step in the awful march of the visible and sensible picturings of the purposes of the Eternal Spirit! Realize for a moment the position of a tenant of a hut on the banks of the mighty Amazon at one of its great bendings; tell him that the waters, whose opposite bank his vision can scarce reach, are not an immense lake with appointed boundaries, but that, born of rills among mountains that are unseen, and ever increasing in depth and potency, they roll downwards until a whole continent is passed, and then mingle and lose themselves with an ocean engirdling the wide Earth with its everlasting waves. So, in the view of these high cosmogonies, seem to roll on the gorgeous stellar developments; whose limits no eye can now see; rising among the past depths of time in some hidden purpose of God; rolling onward as the ages flow, and augmenting as the mighty river, until the boundary of Time is reached, and their course ends among the quietudes of Eternity!

III. One further consideration remains, and then our loftiest views of our system are complete. As that grand mechanism had a beginning, a birth and progress, a growth towards its present glory; can we, as with the river, stretch onwards our gaze towards a time when its strength shall fail—towards the close of these stupendous arrangements? In article PERTURBATION, indeed, we shall speak of security against all disturbance, of the limits of every inequality, of a seeming fitness for Eternity: but when using language apparently so absolute, we refer only to the perfection of that mechanism *by itself*, and do not exclude its dependence on loftier ordinances. If for instance aught of the original Nebula should remain, filling with an ether, however thin, the interplanetary spaces, every orb moving through it would be *retarded* by an amount, infinitesimal probably, but definite withal; and as sure in its consequences as if that quantity had been large. Our readers cannot require to be reminded here, how—speaking generally—the permanence of the orbit of each planet depends on the perfect balance of two forces or tendencies; viz., the attractive power of the Sun, and that tendency to fly from the centre which follows from the motion of bodies being naturally in straight lines, and whose energy depends, in each case, upon the *rapidity* of the body's motion. If the power of either of those balanced forces be diminished, it is clear that the authority of the other will prevail. Relax Gravity, and the planet must recede from the Sun, and its orbit widen until a balance is restored. In the same manner, diminish the rapidity of the body's motion, and, as the centrifugal force must be diminished by that act, Gravity will prevail;—so that the body's orbit must be *contracted* or *drawn in*. Now, if a nebulous fluid is diffused among the planetary spaces, every body which moves through it must experience resistance, and be *retarded* as *we* are by the atmosphere when we ride at a rapid pace; and we should thus expect a notice of the ether's existence in the fact of the planetary orbits gradually drawing in, and the revolving bodies approaching the Sun. Unhappily, however, for this only mode of observation left, the planets are too dense, too large to be of service in so delicate an inquiry. However light and thin the ether, there is no doubt that it must and will influence even *their* motions; but perhaps by a quantity so small, that the accumulation of the perturbations arising from it during the entire existence of accurate astronomy could not render it perceptible. No trace of such influence, indeed, is yet found in our planetary tables; and astronomers would have been left in regard of the whole subject to conjecture, which, however plausible, had yet no actual or experimental *ground*, unless for a remarkable and certainly an unlooked-for occurrence. Until recently, astronomical science has not been able to present a complete and minutely accurate view of the orbit of any comet. The general character of the orbits of these bodies, and the important elements at least of *one* of them, have been known since the time of the celebrated Halley; but this philosopher knew nothing further than the general elements; and no orbit was laid down with exactness sufficient for the above purpose, until Encke of Berlin examined with so much accuracy the conditions of a *body*—if a thing so small and vaporous merits the appellation—which completes its eccentric course around the Sun in three and one-third years. Now it appears probable that this comet *is approaching the Sun:* on every successive appearance, its orbit appears somewhat contracted; and there is reason to believe that the contraction will go on until it is either absorbed in that luminary, or altogether dissipated by his beams. And after searching earnestly for some other cause, most inquirers are inclined to refer this extraordinary and hitherto unparalleled change to a RESISTING MEDIUM OR ETHER occupying the planetary spaces. "I cannot but express my belief," said Professor Airy, "that the principal part of the theory—viz., an effect exactly similar to that which a resisting medium would produce—is perfectly established by the reasoning in Encke's memoir;" and similar opinions have been offered by other great authorities. That the Sun, then, has a widely diffused nebulous atmosphere—extending far beyond the limits of the Zodiacal Light, and if not beyond, at least deep into the planetary spaces—an atmosphere of which that light may merely be the densest portion, appears resting on a high degree of probability; and how singular is it that we should have been guided to a truth so remote and difficult—one concerning which the grander phenomena of our system are silent—by the motions of a wandering object, in comparison with whose ethereal nature, even one of these light flocculi or flakes of cloud, which scarce stain the sky of a

summer evening is heavy and substantial! But though these greater orbs have not spoken hitherto—at least so that man's ruder senses can hear—of the change manifested by the comet; although no mark of age has yet been recognized in the planetary paths,—as sure as that filmy comet is drawing in its orbit, must they too approach the sun, and at the destined term of their separate existence be returned into his mass. The first indefinite germs of this great organization, provision for its long existence, and finally its shroud, are thus all involved in that master conception from which we have endeavoured to survey the mechanisms amid which we are! Not in confusion, however, shall this majestic scheme finally pass away—not with the jar and confused voice of ruin; but even in its own quiet and majestical order,—like the flower which, having adorned a speck of earth, lets drop its leaves when its work is done, and falls back obediently on its mother's bosom.

Neptune. The elements of this recently discovered planet are given in the article Elements. The history and method of discovery, however, so aptly illustrate the whole subject of the planetary perturbations, that we shall give it somewhat at length. The planets move approximately in *elliptic* orbits. If the sun were the only attracting body in the system, this form would be perfectly maintained. But all other matter in the universe apparently attracts according to the same law as the sun does; and what is of present importance—the planets are so near each other, that each attracts the other to an extent which produces upon the orbit an effect quite appreciable. It is easy to see that the result will be to make the absolute orbit of a planet assume, instead of the regular elliptic form in the diagram, some such dotted and irregular form as above; although the diagram is a very exaggerated representation of any actual case. We shall consider the ellipse, however, to be the normal or *mean* orbit. In such divergences from the normal orbit, as those figured, a key is to be found for a complete *mathematical deduction* of the whole arrangements of the solar system.—Any one who looks for a moment at a picture of the solar system, and remembers that the times of periodic revolution of its different component bodies are all different, so that the relative positions of all of them are incessantly deranged, will at once see how complicated is the problem which astronomy presents, viz.:—having given the positions at any one moment—and the masses of all the planetary bodies, to obtain expressions accurately describing their paths for all succeeding times. But the problem to be resolved first, in astronomy

Fig. 1.

is by no means so simple even as the foregoing. We have as data, in the first instance, only the apparent planetary movements. These are the results—following, it is assumed, according to the ordinary law of gravitation—of forces which vary with all actual masses and positions, in the system. It is at once evident, that on *every* member of it, *all* act and react; but that approximate results may be obtained by neglecting the less important forces. Thus Mercury may be considered to be influenced almost entirely by the sun and the next planet, Venus; and in such ways we may obtain results approximately true. It is clear, too, that nothing but approximations are possible, because the problem in its utmost rigour is, to find out from a finite number of observations, however great, an infinite number of unknown forces in magnitude, position, and direction. But by using the approximate values obtained in past results,—say the mass of Mercury or Venus, in endeavouring to obtain the mass of the Earth,—we can reach an approximate value for that. Suppose now the influence of the force due to *it*, upon Mercury and Venus be taken into account, we shall evidently find for them, more correct determinations. This system of continuous approximation can clearly be carried on, until—if we know all the planetary bodies—we obtain from the observed movements full information respecting the system, to every desirable accuracy. In whatever way we reach this, our approximations will enable us, if we know what number of planetary bodies there are, to obtain theoretical orbits approximating as closely as we choose, to the real orbits of the planets. But suppose one of the planets incapable of reflecting light. It is clear that upon the orbits next it, that planet will produce a powerful effect, quite incapable of being explained by any approximations we have obtained. That it will derange all our calculations respecting the whole system is manifest; but the less appreciably for the orbits at greatest distances from it. Laying this down, we have to explain by some hypothesis the phenomena of irregularity due to this dark planet. We can see what orbit the planet most disturbed ought to have if there were no other planets but those which we know; and we know the departures from that orbit by observation. From these departures then, it may perhaps be possible to discover the mass and character of this concealed planet. This, as we shall immediately see, was the special problem concerned in the discovery of Neptune.—It will be convenient to premise a few words respecting the technical division of the disturbances or *perturbations* from the normal ellipse. It must distinctly be understood that these divisions are founded on no differences in kind of the causes for different perturbations, but are merely used for the sake of clear subjective appreciation of the modes of their action. The principle of this separation

simply is, that for the sake of convenience, we count up, in the first place, the more obvious and considerable disturbances; then those which are somewhat more evanescent, and extend probably over a larger period; and lastly, those which enclose within their wide range the whole conditions of our system—even in its relations with the fixed stars,—stretching our view of the harmony of the various orbs, onwards through innumerable centuries. I. The first and most palpable description of variations, or perturbations, is what we term *periodical.* These depend on the directions in which the different bodies lie, in regard of each other; and they simply affect each body's place in its orbit. An irregularity of this kind, *does not affect the ellipse or the species of curve in which any body moves;* but it causes that body to be either *before or behind its natural place, in that curve.* The method of taking account of such, in our calculations of the place of a planet at some future time, is extremely simple;—we determine, by another description of inequalities, in what ellipse or curve the body must then be moving, and the laws of the periodical inequality readily determine in what place of the ellipse the body ought to appear. These perturbations, too, have generally very short periods; and although they may often be of much less absolute importance than deflections which stretch over a very wide range,—they are, nevertheless, all-important, when we are required to study the course of the planet only *through a short period of time*—say one or two revolutions. II. But, more complex in character, more difficult in determination, and more remarkable in their results, are those irregularities or inequalities of the second class, to which the term *secular*—because of the great periods they involve—has universally been attached. These inequalities *affect the orbits in which the planets move*—each orbit, through effect of the actions of the other orbs on the planet to which it belongs, slowly passing through modifications, which sometimes occupy centuries in their course. The relation of the two sets of perturbations to each other, has been represented by the following diagram:—

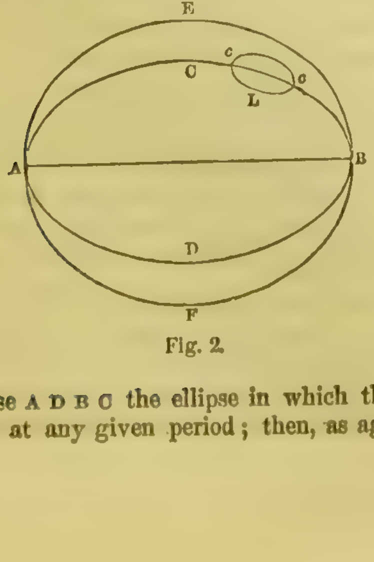


Fig. 2.

Suppose A D B C the ellipse in which the body moves at any given period; then, as ages pass onwards—by the slowest and most gradual evolution—that orbit will modify itself and change into A F B E;—this is the change indicated by the secular inequalities. All the while, however, the planet is not moving on the surface of the ellipse, but in a small orbit, A C B, whose centre performs the revolution in the main orbit; and the distance to which the planet is carried from that main orbit by its motion in A C B, is the periodical inequality. It will be readily discerned that the mechanism of the system, properly so called, consists in the relationship of these secular inequalities; for although perturbations of the periodic class may affect considerably a planet's place, it is those others which interfere with the arrangements of the system, which show how essentially each single part hangs on all the rest, and which alone could affect its stability. Indeed it is in treating the details of this portion of the subject that physical astronomy has manifested its greatest power, and where it has most discerned the perfection of our planetary scheme. Let us rest for a moment, in illustration, on the relations of Jupiter and Saturn. These great orbs act on each other variously—producing, as our readers will expect, perturbations of the periodic class; but there is one great secular inequality, than which none within our system indicates more distinctly the delicacy of its relations. It is an inequality, in the course of whose evolutions the orbits of the

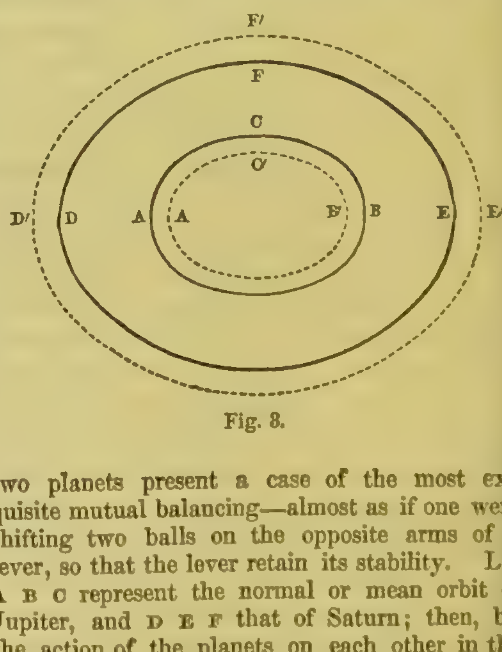


Fig. 3.

two planets present a case of the most exquisite mutual balancing—almost as if one were shifting two balls on the opposite arms of a lever, so that the lever retain its stability. Let A B C represent the normal or mean orbit of Jupiter, and D E F that of Saturn; then, by the action of the planets on each other in the course of centuries, the path of Jupiter contracts itself, slowly creeps in to an inferior limit, A B C'; and during that same time Saturn's orbit as slowly expands outwards, until its limit, D' E F, is also attained: at this point, the actions of the orbs seem to be reversed—the orbit which expanded before, contracting now, and the other likewise undergoing the inverse change; so that

this exquisite adjustment continues—for ever changing, yet for ever stable! The period of these changes is 929 years. It became known, by the comparison of ancient observations with modern ones, that the orbit of Jupiter must have been widening as his velocity was being retarded,—and Halley fixed the numerical value of the inequality: but it was by the penetration of science—by the genius of Laplace—that the nature of the change was discovered, and the period of its cycle fixed. The existence and nature of this inequality is certainly very remarkable; but it is chiefly important here, that we apprehend *its origin.* That lies in a singular *relationship existing between the mean motions of the two planets*—the fact, viz., that *five times the mean motion of Saturn is very nearly equal to twice the mean motion of Jupiter:* nor is the result peculiar to these planets: wherever any such relation—however different numerically—exists between the motions of any two planets affecting each other, it will obtain a definite expression in perturbations, in every respect manifest, and determinable by observation, like the one whose character we have now explained. How discriminating then, as well as powerful, the glance we now can cast among the complexities of our system! Resuming our previous hypothesis of an invisible planet, have we not here a distinct means, by which, from the information given by observation, we might reach the most important characteristics of its motions? As the balance of Saturn in those remarkable oscillations, we would require not a new planet merely, but one whose motions bear towards those of Saturn a *certain definite relation;* and that relation would establish all the important facts regarding Jupiter's orbit!—Let us refer, however, to the special data from whose existence Neptune was determined. When the planet Uranus was descried, it became necessary, as we have pointed out in the general case, to re-adjust the data of the Solar System and to fix the approximate orbit of that planet. Now the problem came up in the following form,—the question, viz., as to the precise path of Uranus—the *real curve* it describes in its revolution around the Sun: and there are two very essential points as to this matter, requiring peculiar notice. I. The problem itself is this: *From a number of observed places of Uranus, to determine its entire path.* It is not necessary to follow any planet through its *entire course,* in order that we lay down the path it regularly pursues. Observation so complete, would be needful only if we had no conception of the *kind* of path in which a planet must be moving; but, since we have a thorough knowledge of the grand LAW that regulates the movements of every orb belonging to our system, a few observations suffice to enable us to conclude concerning any orb's *entire* habitudes. At the very outset, then, we assume as the groundwork of our theory of any planet—and of course with regard to Uranus—*the integrity of the Law of Gravitation in the regions through which that planet moves.* The problem, on the ground of that assumption, is as follows: Reverting to the mode in which every planet must move, as formerly represented by the diagram (fig. 1), it will be seen that the phenomena of that motion are divisible into two parts—*first,* the normal ellipse, depending on the action of the Sun; and, *secondly,* the deviations from it, caused by the disturbing action of the planets. The two sets of phenomena, however, although distinct, cannot be determined *apart from each other:* it being clear that until we know something of the normal ellipse, we cannot compute the deviations, as these depend on the *distance* of the planet from *all the other bodies* affecting it: and again, so long as we do not know the amount of the disturbances, we cannot fix how much to take from or to add to, the observed place, in order to arrive at the normal or undisturbed ellipse. The problem is therefore by no means an easy one: but when our results do not agree with prolonged observation, it is always open to us to try *the effect of an alteration of the normal ellipse,*—an alteration, however, within limits—viz., the possibility of explaining the deviations from the curve on which we fix, by the action of known perturbing bodies.—The solution, it will be seen, depends, in every view of it, *on our knowledge of the mode of the action of these disturbing bodies.* This knowledge must be clear and definite; or we can reach no solution at all. If any unknown body, for instance, acts on Uranus, then the foregoing principles could never enable us, from a few observations of its place, to determine its true path: and therefore the work of astronomers must in every such case involve this other supposition—that *we are acquainted with all the bodies that act on the planet.* Any solution—so far as we have yet traced its progress—thus necessarily involves three uncertainties, viz:—(1.) *The assumption of the unmodified action of gravitation in regions so remote from the Sun as Uranus.* (2.) *The assumption that we have fixed on the true or normal ellipse.* (3.) *The assumption that all the disturbing bodies are known to us.* The first step towards a practical solution was taken by Laplace, who computed the relative influence of Saturn, Jupiter, and the Sun—neglecting the other planets at once from their distance and their comparative size—upon the motions of Uranus. It was subsequently found in 1820, by M. Bouvard, that the planet, discovered to be such by Herschel in 1781, had been before observed at least fifteen times, as a fixed star. The record of these positions clearly narrowed the risk of error in any theory of its motions. Bouvard immediately commenced to calculate the orbit from these observed phenomena. The result was that *the orbit he deduced was found unsatisfactory to either set of observations, and its deviation from*

the older ones was altogether remarkable; in other words, he found that the very circumstance which should have enabled him to crown his effort with complete success, was that which, from some unexpected cause, rendered success impossible. So long as he took into account both sets of observations, or, what is the same thing, so long as he took all available precautions to avoid error regarding the habitudes of Uranus —the results of his inquiries seemed entirely erroneous; in other words, they gave an orbit inconsistent with the facts on which the calculations were based. It further appeared, that if either set of observations was assumed by itself as the basis of the orbit required, a result came out sufficiently concordant with that set, but wholly discordant with the other set; so that Bouvard was obliged to conclude that these two classes of facts were *incompatible.* He concluded to reject for the present the older observations, and to leave to time their reconciliation with the new, calculating the orbit in accordance with these. But this orbit was found to be utterly at variance with the course which the planet proceeded to take *afterwards.* In the diagrams below, if A B in the first represent the observed orbit of Uranus, the broken line will represent the theoretical orbit from 1781 to 1840. If again in the third, A B represents the observed orbit from 1690 to 1771—the dotted irregular line represents the theoretical orbit of Bouvard. These discrepancies were startling enough. But to these was added immediately the still greater and more startling difficulty of the total departure of the planet from Bouvard's new orbit. No sooner had its wanderings been reduced to something within moderate limits, than it again showed a hopeless irregularity. Clearly then, Bouvard's supposition could no longer be entertained. At best, it was a temporary and venturesome expedient, whose simple object was to override a startling difficulty; that *it* at least had no correspondence with the truths of nature, these new results showed sufficiently. How then account for them? Of course, hypotheses were framed to cut the knot. Perhaps this irregularity was real and not apparent. Suppose a comet had struck Uranus and deranged its orbit. But now that this new irregularity had recurred, the explanation could noway be adopted. According to all our ideas such collision would have been simply what we should call an accident. That it should repeat itself in this way—being mere accident—was impossible. What other suppositions lurked at the basis of Bouvard's calculations? Perhaps *there* might lie the source of the difficulty. The law of gravitation had been assumed. Might it not be that in these far spaces that law ceased to operate—that some other, unknown, was substituted in that outer kingdom. A supposition feasible enough indeed, if the law were merely empirical. But such new law had yet nowhere been found; and so many phenomena, at first apparently discordant, had been, one after another, reduced to that great principle, that we might well hesitate. Evidently it was impossible to admit any hypothesis of this kind to explain one solitary series of phenomena, unless all attempts to reduce them to the old principle failed. Had these been exhausted? The reader will readily answer that they had not. There still remained the hypothesis, that disturbing forces, in positions yet unknown, might produce this irregularity. Here were phenomena adequate for calculation of such causes. The problem was novel, because, up to Uranus, the planets had all been readily visible, and we knew where to look for all the forces. Had Uranus not fortunately been discovered, we should have found its influence upon Saturn disturb that orbit, just as that of Neptune was now disturbing it. But the difficulty had been escaped, only to recur now. We must see, then, if some planet yet unknown be not the cause. Tentative processes—sweeping the whole of the zodiac with our telescopes—might give some hope, but how long time *they* might occupy, it was unpleasant to think. Leverrier and Adams, almost

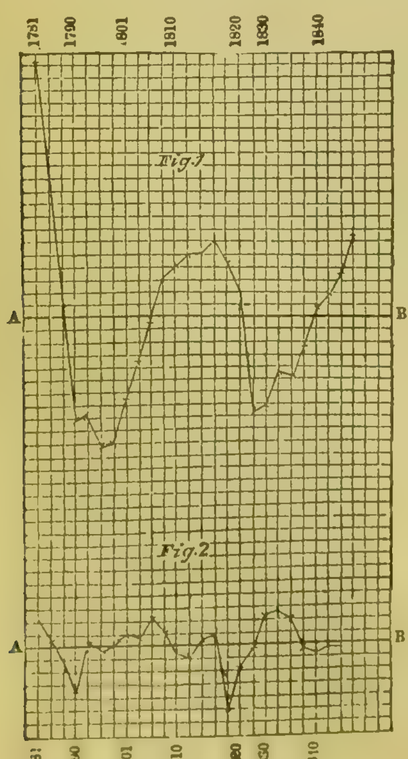


Fig. 4.

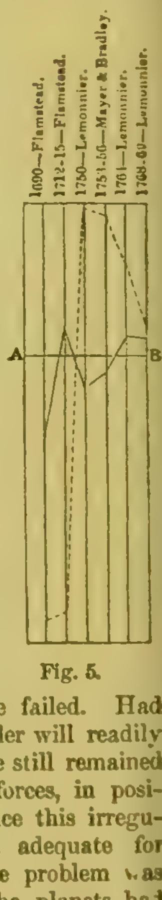


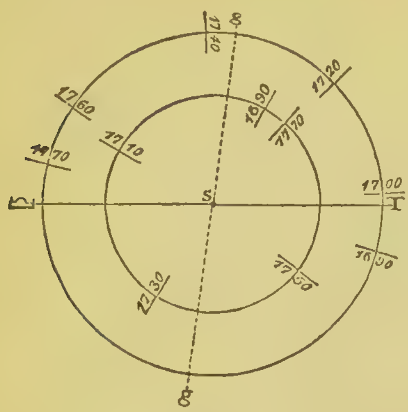
Fig. 5.

simultaneously, and without concert, resolved to attempt the mathematical problem,—to seek from these startling phenomena, what *was and must* be the foreign influence, if the law of gravitation held. Then they might point to where in the sky the disturbing planet was to be found, and search would discover it or prove its absence,—would establish or invalidate the fundamental hypothesis of the permanence of gravitation. The first process was a revision of past and ascertained results. Perhaps previous astronomers might have miscalculated the *normal ellipse* in which Uranus was supposed to move. All possible variations of that were tried, and the anomalies increased or diminished imperceptibly accordingly; but in mass they remained. There *must*, therefore, be an unknown force disturbing the motions of Uranus. Of what kind? A new planet for instance, or an undiscovered satellite? If the latter—to account for such results—it must have been at least so large as to be visible, and the disturbances must have been *oscillatory*. No mere oscillatory disturbances could explain the phenomena. Our ultimate hypothesis is a new planet. If other planets exist outside the orbit of Uranus, the chief perturbing effect is likely to be due to one, and that the nearest. If we can discover that one, the residual phenomena may afterwards lead us to find others, but *that* one must be first searched for. Let the student refer to Bode's law of the distances. That law had been strikingly verified by the discovery of the asteroids. Everything led us to infer that if a new planet existed, it would be found at the distance next in order, according to the law. Still further, all the planets move in the comparatively narrow belt of the zodiac, that is, in planes very nearly approaching the ecliptic. If this hold respecting the body sought for, we may take the ecliptic itself as giving the approximate course. Assuming these simplifications then, the problem has become,—"Is it possible that the inequalities of Uranus *are due to the action upon Uranus* of a planet situate in the ecliptic, at a mean distance determinable by Bode's law? If so, determine all the important elements of that planet." *With* all these simplifications no light task presented itself. Without them indeed, no analysis of ours could have answered these questions. As they stood before the solitary workers, how dread and awful must they have seemed. By toilsome thought only, to call from the Unknown a world which no man had seen, to pass out into those measureless spaces and tell us what things must be. No slowly passing ages great with new achievements, can dim the grandeur of the faith which approached the presence of such mysteries, and extorted their solution.—Thus stood the case. Observation gives us only the total or *gross* result of the combination of a number of unknown quantities—the elements of the yet unseen planet, and the real elements of this eccentric and irreducible Uranus; and from this total result are to be eliminated the fullest separate information regarding these commingled subjects.—It is easy to understand that it may frequently be possible, by neglecting quantities which, at different stages of the process, we may see *can* produce no great alteration of the final result, to reach approximations to it. Suppose we neglect such, on different sides as it were of what we aim at, and so obtain limits between which the truth must lie. One manifest approximation would be got by a rough separation of the unknown mass, and the unknown position of the planet. The former affects the *magnitude* of the force, the latter its *direction*. Such approximation made, it was announced that, on the first day of the century, the planet must have been at from 243° to 252° longitude, that is, within the determined space of one-twentieth of the whole visible arc of the heavens. Using that result, and going back on the calculations, it was not difficult to obtain a second approximation; and, returning to the old observations, it was found that the new planet ought to be at 326° 32′ on 1st January, 1847. The prediction was made, and the observatory at Berlin set to examine into the truth. In seeking for a planet, two methods present themselves. *First*, see whether the stellar body observed, have an appreciable disc. In that case, it can only be a planet. But the planet may be too small for that, or too distant. The *second* process is to ascertain whether the body changes its relative position among the adjacent stars. Fortunately for the inquiry, a new and most careful mapping of the stars of the zodaic had just been completed, by the Academy of Berlin; and it simply remained to find whether, about the place predicted, a star could now be found, of sufficient magnitude, to have been certainly not overlooked in constructing the chart. That was done, and the planet discovered, as of the eighth magnitude, at 327° 24′—an error of 52′. The planet was found, and the disturbances explained! It was found that the planet had been twice accurately seen before by Lalande—these observations serving the most important ends of verification. But we are not quite done with our history. Now that the new planet itself could be observed, it was evidently necessary to go back over all these tentative processes; and means were evidently within reach of obtaining by unchallengeable observation, all the facts which long and painful theorizing had approached to. Two most startling results were found; in the prediction an error of nearly a fourth of the whole distance, and of half a century—nearly a fourth of the whole periodic time—had been committed. Assuredly nothing but the rarest fortune could have prevented these mistakes from utterly misleading us. Concerning the distance, in the first place—Bode's law was certainly falsified; which fact immediately explains the discrepancy respecting the *mass* of the planet. It is easy to see that

a smaller body, which is nearer another, may produce an identical effect with a larger, which

2.

True position of Uranus and Neptune from 1690 to 1770.



1.

Supposed position of Uranus and Neptune from 1690 to 1770.

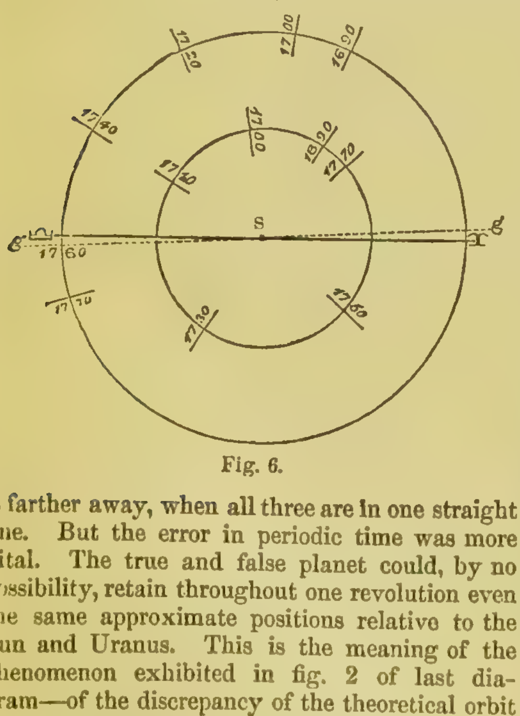


Fig. 6.

is farther away, when all three are in one straight line. But the error in periodic time was more vital. The true and false planet could, by no possibility, retain throughout one revolution even the same approximate positions relative to the Sun and Uranus. This is the meaning of the phenomenon exhibited in fig. 2 of last diagram—of the discrepancy of the theoretical orbit of the false planet, and the actual orbit of the true one. Consider in the last figure below, B to be the position of a body attracting a revolving planet—A, it is sufficiently clear, will be *accelerated* or drawn onwards in its course, during its motion *in the semi-orbit* A″ A′ A; — more accelerated, however, whilst in the near quadrant A′ A than in the quadrant A″ A′;—and, on the other hand, it must be *retarded when in the semi-orbit of* A A‴ A″; and more, during the quadrant A A‴ than when in the quadrant A‴ A″. Minute but withal perfectly simple consideration would bring out other facts of consequence, as to these accelerations and retardations; but what we have just stated will suffice. Let us now examine then, with some care, the figure which shows the relations of the *false planet* with Uranus from 1690 down to 1770. The epoch of 1690 may be termed the epoch of the conjunction of the two orbs, or when the energetic retarding action would begin: the action due to the first quadrant, would continue till 1730, after which the relation of bodies would be that of the second quadrant, and would so continue till 1760. These entire effects are as follows:—

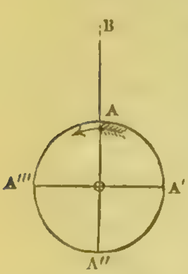


Fig. 7.

ACTION OF THE FALSE PLANET.

From 1690 to 1730.—The powerful retardation of the first quadrant,—conjunction having just passed.

From 1730 to 1760.—Action of the second quadrant—still a retarding, though less powerful action.

From 1760 to 1770.—Action after opposition—that of acceleration, but comparatively gentle, and as to quantity ineffective.

Let us now trace, by aid of the corresponding figure,

THE ACTION OF THE TRUE PLANET.

From 1690 to 1700.—The latter part of the first quadrant—the action retarding.

From 1700 to 1737.—Second quadrant to opposition—action retarding.

From 1737 to 1770.—Second semi-orbit—action accelerative, growing in potency; but only approaching the second quadrant.

A single glance will discern effective contrasts in these two schemes. Speaking generally, and without regard to minutiæ, they are these:—(1.) Immediately after 1690, and for several years, the strong retarding effects of the false planet have no equivalent in *quantity*, among the actions of the true planet. (2.) Between the years 1737 and 1760, the effects of the two orbs are opposed—that of the false planet retarding; and that of the other being accelerative. How happened it, then, that with these discrepancies of effect for the false and true planet, the place was correctly calculated within one degree—since the periodic times—and therefore the positions of the true and false planets are so different? The explanation is this. Though the two are not in general in the same position relative to Uranus, there will be intervals of accordance, among the great periods of disaccordance. Roughly, for instance, if the periodic times be 150 and 200 years respectively, they will be in accordance once every 600 years—for if, at any one point of time they are so, after 600 years, the one has completed four and the other three revolutions, and each has returned simultaneously to the point from which, at the beginning of that time, it had started. Now, when two planets are in the relative opsitions of A and B in the last diagram, the distance is unimportant, if we may change

ıe mass correspondingly. And it is clear, that ear the point A of the orbit, changes of disınce of the planet B from the planet A will have ıe least possible effect in altering the motions of . It happened that at the time when the lanet was discovered this was the position of ıatters, so that the very small error due to the roneous assumptions which had been made—in ıe strictest accordance with analogy—almost ɔmpletely disappeared. Scarcely at any other me could the problem have been attacked with ıch success. Certainly no less worthy had been ıe achievement and the effort—but the seal and ssurance of victory—the RESULT—might have, r half a century, escaped astronomers. Let us elieve, it can be no barren fact that such not remature, but most unlikely honour has crowned ıe Analytical Science of our time. About ıe question of priority of discovery, one word is ıfficient. Leverrier and Adams reached their sults independently. The priority of publicaon is due to Leverrier,—though no fault of Mr. dams,—that of discovery as unquestionably due our countryman. The question of precedence ɔpears absolutely insignificant. Their honours ıust be shared alike. Each did the same work; ıd will win from history the same award. See ATELLITES.

Night. Properly that portion of the twentyur hours during which the sun is below our ɔrizon, or rather our horizon above the sun. he twilight and the dawn are not, however, ınsidered as parts of the night.—Anciently, by ıe Eastern nations, Night was divided into three *atches*. The Romans, and the Jews after they came subject to Rome, divided it into four atches, commencing at sunset, and ending at ınrise. The ancient Gauls and Germans divided ıeir time not into days, but into so many nights; ıd the modern Icelanders and Arabs follow ıe same practice.—The length of night varies ıiefly with the position of the sun, according to hich he describes more or less than a semicircur arch in the visible heavens. See DAY.

Nodes. The points of a planetary or cometary bit that cut the ecliptic; and, by extension of eaning, the points where the orbit of a satellite ıts that of its primary. The node, is called the ɔde (*i.e.*, knot) of the first orbit upon the second. he node, when the planet is passing northerly, called the *ascending node* or *dragon's head*, and emphatically marked ☊. The other, when ıe planet is passing southwardly, is called the *escending node* or *dragon's tail*, and is marked ıus, ☋.—The positions of the nodes of every anet make, in a certain time, a revolution rough the signs of the ecliptic, and a real volution around an actual great circle in the avens. This peculiar change of node is the sult of a perturbation depending on the fact, at the bodies of our solar system are not ithin the same plane. It is the consequence the action of the perpendicular part of the disturbing forces; and so in other cases. See LUNAR THEORY and PRECESSION.

Norma. The rule. One of Lacaille's constellations between Scorpio and Lupus. It has no star above the fifth in magnitude.

Normal. A perpendicular to the tangent to a curve at the point of contact, is called a normal. For full technical expressions giving the equation of the normal, see Gregory's *Solid Geometry*.

Nucleus. A term given to the condensed portion of the mass of a comet. Hence it is applied to any part of a whole which is more opaque than the rest, as it is supposed that this contains the denser matter. The term is frequently employed in the cometary and nebular theories.

Numeration. The method of expressing numbers by definite signs. The history and forms of the special symbols used for different numbers by different nations will be found pretty copiously detailed in the treatise on Arithmetic in the *Encyclopædia Metropolitana.* The ancient Greek method of reckoning numbers had a special character for every number, and several systems retain this. The principle of numeration, however universal in civilized society, is that of giving different value to the same signs by *local* position. Thus the same sign serves for two entirely different things, and yet a clear distinction can be made between them. In the method of undeterminate co-efficients, where we have such a quantity as $a^5 + 4\,a^4 + 3\,a^3 + 2\,a^2 + 9\,a + 6$, we mark only the co-efficients $1 + 4 + 3 + 2 + 9 + 6$, and this serves us until the end of the question. In the methods of numeration which we adopt, we take 10 as a, and any number, such as $10^5 + 4 \times 10^4\ 3 \times 10^3 + 2 \times 10^2 + 9 \times 10 + 6$, can be expressed as $1 + 4 + 3 + 2 + 9 + 6$, and thus used the same as the other till the termination of a question; and, in fact, kept so, until we need to have a very clear idea of the actual number expressed. In the undeterminate co-efficients the signs are interposed, but in the system of numeration it is not necessary. In the former, in general expressions, sometimes negative signs are employed, but in this it is never needful, and therefore, for the sake of uniformity, never attempted. Hence the expression becomes 143296. It is evident that any ordinary whole number can be put under the form $x\,a^n + y\,a^{n-1} + 2\,a^{n-2}$, &c., the only condition being that x, y, z, be less than a. Thus to take 1296, which is to be put under the form of successive powers of 5, we have just to divide it by those terms thus—

$$\begin{array}{r|l}
5 & 1296 \\
5 & \ \ 259 - 1 \\
5 & \ \ \ 51 - 4 \\
5 & \ \ \ 10 - 1 \\
 & \ \ \ \ 2 - 0
\end{array}$$

and we will have

$$1296 = 5 \times 259 + 1 = 5 \times (5 \times 51 + 4)$$
$$+ 1 = 5 \left(5 (5 \times 10 + 1) + 4\right) + 1$$
$$= 5 \left\{5 \left(5 \times (5 \times 2 + 0) + 1\right) + 4\right\} + 1$$
$$= 2 \times 5^4 + 0 \times 5^3 + 1 \times 5^2 + 4 \times 5 + 1$$

Similarly, any number can be brought to such a form, and, therefore, one fixing upon the value of the constant a, which in our notation is 10, we can express any whole number by powers of 10, leaving these to be supplied from the relative positions of their co-efficients. The decimal method of expressing fractions in the same system is given in all arithmetical works.—The choice of the number is evidently caused by the number of the fingers. The numbers 5 and 20 have also been chosen. The latter gives too many separate characters—nineteen of them—for familiar use, and the former makes numbers which we have frequently to use, expressed by too many figures. The number 12 has also been suggested, and an attempt was made to introduce a uniform duodenary system in Sweden. The great number of the aliquot parts of 12 and its power compared with those of 10 is the inducement to this, and had we to choose over again, it would in all likelihood be adopted. The duodenary has been extensively introduced in the measurements of quantity—as inches, pence, &c.; and this, since the denary must be employed in abstract arithmetic, is a serious evil, already removed in France, and soon to be so here likewise.

Nutation. A small motion of the earth's axis like what the axis of a top frequently has when the top is moving very rapidly round. It must not be confounded with that much more important motion of precession—also affecting the earth's axis. Under nutation the complete revolution is accomplished in about nineteen years only, while the period of the complete motion of precession is about 25,868 years. In fact, they combine to produce a total motion something like that of a circle which, all round the edges, is indented outwards and inwards, like an India rubber ring, pressed inwards at various equal intervals round its rim, and therefore, bulging out between them. There are about $\frac{25868}{1}$ or 1362 of these little intervals round the large circular curve of precession. The movement of nutation is thus a gyratory motion in a small ellipse of 18″·5 for its major, and of 13″·74 for its minor diameter, which the axis accomplishes in nineteen years. The major axis of this little ellipse is directed towards the pole of the ecliptic, and the shorter, is at right angles to it. Since the pole is the point upon which the accurate position of the equinoctial circle depends, the point where it cuts the ecliptic will vary with this motion,—that is, there will be a definite effect upon the position of the equinox exercised by every such change. It is at its greatest, only 18″·5 in nineteen years, or about 1″ per annum, sometimes increasing precession, apparently to that amount, when it is acting in the same direction, and sometimes diminishing it when it is acting in the opposite direction. The physical cause of nutation is precisely that of precession. And if the moon's orbit had been co-incident with the ecliptic, we should simply have had a luni-solar precession. Nutation mainly arises from the changes of the moon in latitude, and passes through phases corresponding with the changing relations of our satellite towards the equatorial ring or protuberance. For a full discussion of this subject, see PRECESSION and STARS.

O

Oblate. See SPHEROID.

Obliquity. The angle which the various planetary orbits make with the ecliptic are respectively the obliquities of these orbits. The term is chiefly applied to the ecliptic, or orbit of the earth,—the angle which it makes with the equatorial circle is called the obliquity of the ecliptic. See ECLIPTIC.

Object-Glass. The object-glass of a telescope or microscope is the lens or system of lenses nearest the object contemplated. Its function is to form a correct image of that object within the tube. It ought, therefore, to be free from spherical aberration, and to be achromatic. Achromatism is produced by making the object-glass of *two* lenses having different dispersive powers—the one lens being convex, and the other concavo-convex. Such a compound lens will give a colourless image, when the foci of the two lenses are in the proportion of their dispersive powers. The substances usually employed are crown glass and flint glass—their dispersive powers being as ·6 : 1:—and an achromatic object-glass might therefore be obtained by employing a convex crown glass lens of 6, 60, 600, &c., inches in focal length, in connection with a concavo-convex flint glass lens of 10, 100, or 1,000 inches of focal length.—In regard to spherical aberration, Sir John Herschel has shown that a double object-glass will be nearly free from it, if—while the radii of the interior surfaces of the lenses be computed to produce achromatism—the radius of the exterior surface of the crown glass be 6·72, that of the flint 14·20, and the focal length of the combination 10·0. To correct this *aberration* farther, triple combinations have been resorted to, viz.; two convex lenses of crown glass, having between them a double concave of flint. But it

· so much more difficult to construct such lenses :curately, that artists do not now employ them. enerally speaking the two lenses of the achro-natic are in close contact; but in some tele-:opes, called *dialithic*, there is a considerable space :tween the two,—an arrangement that permits ' the flint glass being of inferior diameter.—:he object-glass being the great collector of light ı telescopes, it is of course of the utmost conse-uence that it be of as great a diameter as can be otained. But from the viscous nature of glass ı the molten state, it is most difficult to obtain ırge blocks or sheets of it, sufficiently uniform › make it applicable to astronomical purposes. he artist who first of all succeeded in obtaining omogeneous masses, was Guinand of Brenetz, in witzerland. He wrought subsequently in con-ection with Frauenhofer at Munich. Guinand otained success up to 13·2 inches in diameter,—ıis glass is now at Markree Castle in Ireland, aving been purchased by Mr. Cooper. The manu-:cture has continued at Munich with increasing ıccess,—M. Merz having produced admirable oject-glasses of sixteen inches in diameter. A aring attempt was recently made in England by ıe Messrs. Chance of Birmingham. They con-ructed a disc of twenty-four inches in diameter r the Rev. Mr. Craig, the enterprising vicar of eamington: and since then they have produced disc of twenty-nine inches, weighing two cwt. Vith what absolute success cannot yet be asserted, · the great telescope at Wandsworth, erected by Ir. Craig, having been found defective as to the orrection for spherical aberration; and the other isc not having been ground. But it is to be oped that the most acceptable and necessary lief from the vexatious pressure and interfer-ıce of excise duties—a boon we owe to Sir Robert eel—will enable our great glass manufacturers › overcome existing difficulties by aid of multi-lied and incessant efforts and experiments.

Observation and Experiment. The two ıbjects named in our title, and which we pur-osely consider together, are the two grand means y which the facts of Nature are ascertained and › examined that we may the more readily dis-ern the laws of the sequence of phenomena. *bs·rvation* signifies, as the word itself indicates, ıe direct scrutiny of any event or appearance resented in the ordinary course of Nature. *Ex-eriment*, on the other hand, signifies the arti-cial reproduction of a natural phenomenon, and ıe examination of it under circumstances which e may vary at pleasure. *Experiment* evidently ıcludes Observation, and offers besides vast faci-ties for the analysis of complex facts; but each ıethod has its legitimate sphere and its peculiar lvantages within that sphere. When, from *fects*, we desire to ascend to their unknown *uses*, it is plain that Observation must ever be ıe chief mode of inquiry, because, so long as the ıuse is unknown, we cannot—unless tentatively -produce the effect; and even tentative methods must rest on hypotheses suggested by previous Observation. When, on the other hand, a cause is producible, and we desire to observe it under all circumstances and to discover the pure laws of its action or the mode in which it intermingles with other causes in evolving the complex phe-nomena around us, our precious instrument is Experiment. Thus, while Observation is required for the successful pursuit of all sciences, the resources of Experiment are not everywhere avail-able. Astronomy, for instance, rests on Obser-vation alone: we cannot reproduce the celestial motions within our physical laboratories. In the field of physics, again, Experiment is our power-ful arm; in that field we have the freest and fullest choice of the circumstances within which we may reproduce and therefore analyze the *causes* or phenomena under scrutiny. Chemistry ranks *after* physics. Nothing can be accom-plished in chemistry unless by Experiment; but as the concurrence of certain conditions is generally indispensable to the success of chemical experi-ments, our choice of circumstances is not so free nor our power to vary them so complete, as in physics. Experiment is also applicable to the physiological sciences, but under restrictions much more serious. In all experimental inquiry in this wide and engaging field, it is imperative that the fundamental condition of LIFE be main-tained,—life, too, in its normal state; and this demands a concurrence of very complex condi-tions external and internal, which admit of being varied only within extremely narrow limits.—We shall remark briefly on the *essen-tialia* of the two methods.

(1.) *Observation.*—There are two conditions under which we observe phenomena;—either through aid of instruments, or without such aid. If we observe by means of instruments, it is manifest that the accuracy of the instrument must, in the first place, be most carefully tested, —a subject already considered under CORREC-TION and ERRORS. But whether the Observer employs the *medium* of an instrument or not, the accuracy of his recorded results will depend ultimately on the accuracy of his *senses*, and the *conscientiousness* he brings to the task engaging him. There are two classes of delusions against which every good observer must be ever on his guard. *First*, what may be termed *involuntary errors*,—errors depending on *peculiarities in his senses*, or on *temperament*. These are constant, and may be measured and allowed for. Errors arising from imperfections of the Senses are easily discovered, and guarded against; for instance, no man afflicted with *Daltonism* in any of its varied phases, could conscientiously pro-nounce on questions involving an appreciation of colour. The errors of Temperament are less easily caught, because generally they are not suspected. For illustration of them, see PERSONAL EQUA-TION. But there is a second source of error on the part of the Observer that cannot be termed

involuntary. It is his tendency to see things as he desires them, his lack of impartiality, the absence in his case of that grand humility which has annihilated self, and placed the soul as a child-like learner in face of God's Universe. No man worthy of the name of Inquirer, can now be found within the sphere of the physical sciences at least, with daring enough to say that he saw what he did not believe he saw. Nevertheless, there are multitudes who have not acquired this highest knowledge, viz., that the elaborate description of a thousand so-called facts as they appear through their coloured spectacles—*i.e.*, as modified by theoretical views, or probably by combativeness and bad temper—avails nothing, in comparison with the faithful description of one solitary phenomenon as it really is. Righteously however, may it be claimed for physical science, that within its domains an example of true conscientiousness has been set, which if followed in other regions of inquiry would go far to regenerate the world. It is not in physics that a Man's bias towards the Emission Theory of Light, or its opponent the Wave Theory, would be held satisfactorily or respectably accounted for, by the circumstance that he belonged to the Agricultural or to the Commercial class!—*verb. sap. sat.* Carefully however and conscientiously as the observer may act, he is still to some extent deceived; and the circumstances under which he observes are more or less favourable. Hence the consideration of what is technically termed the *Weight of an Observation.* On this important point information will be found under ERROR, CORRECTION, PROBABILITY, and SQUARES THE LEAST. It scarcely requires to be remarked that—all things else being equal—a man is a good Observer in proportion to his knowledge of his subject. It is that alone that can instruct him as to the specialties of the complex phenomena to which attention should be directed.

(2.) *Experiment.*—Experiment is not the mere art of reproducing or manifesting a special phenomenon. It is the art of *so* reproducing or manifesting it, that the laws to which it is subject may be the more readily discerned. The spheres of Observation and Experiment have been already defined; it remains that we enunciate the principles which govern every philosophical experimenter. Those laws are given as they are stated in one of the few very remarkable books of our time—Mr. Mill's *System of Logic,*—a book that has now its permanent and classic place beside the great *Organons* of former ages. The leading methods of experimental inquiry may be classed as *two*, viz.:—The *Method of Agreement* and the *Method of Difference.* Their *Canons* are as follows:—FIRST. *If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon.*—SECOND. *If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or cause, or a necessary part of the cause of the phenomenon.*—THIRD. *If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance; the circumstances in which alone the two sets of instances differ is the effect, or cause, or a necessary part of the cause of the phenomenon.*—FOURTH. *Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.*—FIFTH. *Whatever phenomenon varies in any manner whenever another phenomenon varies in a particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation.*—From the foregoing five Canons which include and circumscribe the entire subject, all practical rules for productive experimenting may at once be deduced.

Observatory. A building expressly set apart for the conduct of observations concerning any great class or series of natural phenomena. The Observatories now existing are of three kinds:—

(1.) *Observatory Astronomical.*—Observatories for detecting and measuring the motions of the heavenly bodies, and for scrutinizing their physical structure, are now wide-spread in all cultivated countries; nor is the triumph of the mechanical arts, as well as their vast importance as the handmaids of science, anywhere more emphatically manifested.—The prime essential of an astronomical observatory is *fixity.* Professing as its aim, to detect external changes of place so small that they may almost be termed evanescent, it is clear that the instruments with which it is furnished must themselves be stable. Hence the importance of the foundation on which an Astronomical Observatory rests. The best of all is a hard and stiff *till* or clay,—a material of no appreciable elasticity, and which no superincumbent weight will appreciably compress. Rock is not compressible, but it is more elastic than *till*, and therefore transmits tremors. It is almost needless to say, that the stone pillars on which the instruments are placed, must be wholly isolated from the floors or other parts of the building that covers them in. But after all precautions, these instrumental supports will seldom be found wholly reliable: in cases where absolute stability might have been expected, *periodic changes* have been traced,—or irregular expansions and contractions following the order of the seasons. It is for the observer to determine the nature and amount of these in each case.—The foundation provided for

ıd its exigencies, the builder of an Observatory ill next search for a good horizon, and attempt secure comparative freedom from atmospheric ›structions. The requisite horizon is not now ught, as formerly, by placing the instruments on fty towers, to the great and manifest detriment the first requisite of *stability:* it is obtained the selection, as a site, of a broad eminence of oderate elevation; but which is sufficiently gh to overtop surrounding buildings. Atmo-›heric obstructions are not easily avoided. enerally speaking, Observatories are connected ith large cities; and it is for the engineer to c on a site as free as possible from the fogs that evail in low swampy grounds, or the smoke suing from factories, &c. It is unfortunate that country like Great Britain has not made more tensive use in this respect of the unequalled ›portunities afforded by its colonies, and the her possessions which compose its Empire.—he *instruments* essential to a well furnished As-onomical Observatory are not numerous, but ey must be choice, and are very expensive. hey are of two classes, viz.; Meridional Instru-ents, and Extra-meridional Instruments. To e former class belong the *Transit* and the *ural circle*, or, combining both in one, the *ransit-circle* of continental Artists. These quisite tools, for determining what may be rmed the vertical and horizontal co-ordinates of a ar, are described under CIRCLE and TRANSIT. f Extra-meridional Instruments the *Equatorial* the chief, whether under the form of a single lescope or of a Heliometer. See EQUATORIAL d HELIOMETER. Subordinate, although in a ght degree, ranks the *Altitude* and *Azimuth strument* (*q. v.*) under its various forms—one which is the *Universal Instrument* (*q. v.*) of tel: and if to these be added a *Comet-seeker*, e furniture of an Astronomical Observatory may considered complete. Thanks to the recent rge advances in the mechanical arts—at once rfecting and cheapening such instruments—icient and admirable Observatories of this kind e now widely diffused. It is quite unnecessary speak of Greenwich or the other institutions this country. But in referring to the grandest European establishments—that of *Poulkowa*, present under the direction of M. Struve, it ay not be out of place to express the hope that ierever an Observatory may be built and fur-shed, provision will also be made for the *rmanent* use of all its instruments.—Great stitutions of this kind are now likewise in ergetic and prolific action across the Atlantic: especially let us signalize the Observatory of ımbridge near Boston, which, under the con-ict of Mr George Bond, has already done in-timable service.

(2.) *Observatory Magnetical.*—The object of stitutions so named, is to detect and record e phenomena of Terrestrial Magnetism. Already have given, under the appropriate article, an account of the Instruments recently so extensively employed, and of the results to which Observation has led. Subsequently to the labours of Gauss, many modifications were proposed in the details of his Instruments,—one being the substitution of small magnetic needles instead of large bars. The student is referred to the various memoirs by Professor Lloyd, and to a most interesting paper—reprinted in Taylor's collections—by Lamont of Munich. We do not enter here at any length on this subject, because of our belief that the constitution and conduct of Magnetical Observatories is still provisional. The inquiries instituted, mainly through the influence of the veteran Humboldt, were the first of their kind, and therefore merely laid the foundations of that knowledge which is requisite to establish *what ought to be observed*, and *how?* Many new vistas are already being opened; and there is no reason to doubt, that in course of a few years, governments and associations will find themselves at liberty to start afresh.—The standard Magnetical Observatories now in action in this country are those of Greenwich and Dublin. The labours of the Observatory at Makerstown are understood to be closed—not however without having done vast service to science.—The self-registering faculties of photography, have been made effective with regard to Magnetic as well as Meteorological changes.

(3.) *Observatory Meteorological.*—There are two distinct questions connected with Meteorological Observatories,—the question, *viz.*, as to the best mode of realizing the precise and defined intention of the Observatory; and, *secondly*, the question whether that intention is the one which ought to influence Observers. We shall discuss these two most important points separately.

(*a.*) *The Aim of Existing Meteorological Observatories and their Apparatus.*—The objects in view of the present Meteorological observer, may be stated as follows;—to ascertain the weight of the column of air above his Station and its variations; to determine the condition of the air as to temperature humidity electrical excitement and ozone, a few feet above the ground; to observe and value the agitations of the air or the winds; and to discern, if possible, relations among these various elements. The weight of the air is declared by the *Barometer*, which is examined at stated times; the temperature of the air by the *Thermometer;* its humidity by the *Hygrometer;* the amount of solar radiation by some form of *Actinometer;* the rain that falls by the rain gauge or *Pluvimeter;* the direction and force of the wind by the *Anemometer;* the electric state of the atmospheric stratum by the *Electrometer;* and the more essential relations of the air to vital functions by the *Ozonometer:* —all which instruments are described and discussed under appropriate articles in this Cyclopædia.—Until very recently, no observations, of such sort could be conducted continuously—

at least without an expenditure not often at one's command. The states of the air were noted, therefore, only at certain hours; and the grounds on which the *mean atmospheric state* as to any element, was inferred from few and sparse daily notices, is explained under MEAN. But the introduction of the principle of *self registering*, has inaugurated a new epoch in Meteorology. By the mechanical skill of Mr. Brooks and Mr. Ronalds of Kew Observatory, all these instruments have been made—through assistance of photography—to leave a permanent and continuous trace of their fluctuations; so that the diurnal mean can now be most accurately obtained by the summation of the states of the air at intervals as small as need be noticed. Professor Johnson of Oxford, has recently shown further, the great importance of photographic registration, in manifesting sudden and irregular changes—during *storms* for instance—which might altogether escape the notice of observation at stated times.—Let us venture to impress here —as in the case of Astronomy—and even more so, that facility of observation and of record, rather augments than diminishes the need for devoted calculators. Photographic pictures from all the stations in the world, will yield little unless they are compared; and, as a first step, reduced to definite numbers in accordance with a general and fixed scale.

(*b.*) *Is the Present Aim of Meteorological Observatories, the Right or the Best Aim?* A most instructive discussion on this subject recently took place in the French Academy of Sciences. Opinion was divided, but we cannot escape the feeling, that there is much truth in the views so forcibly put forward by MM. Regnault and Biot. The question is so important that we have thought fit to append nearly in full the note by M. Biot.

"The complex mass of physical knowledge, now called Meteorology, has not yet been constituted into a science. Were it so, we should find it comprehending, in the first place, the chemical and statical constitution of the atmosphere; the regular laws of the decrease of pressures, of densities, of temperatures, of electrical tension at different heights. Next, in the inferior strata, perpetually agitated by irregular movements, the infinitely varied causes would reach us, or at least the nature of the local accidents which produce these: the formation and the internal constitution of those definite groups of aqueous vapour which are called clouds; the physical circumstances which determine these vapours to concentrate themselves under the form of rain, snow, hail, with the ability, nevertheless, of sustaining themselves for a length of time, suspended and floating, even in these latter conditions, in opposition to the power of gravity. Respecting all these general phenomena, we are in almost absolute ignorance; and the little that we know of them, is due to the individual researches of a small number of sagacious observers, who have applied their intelligence specially to the study of certain peculiar phenomena. Thus Dr. Wells has admirably elucidated the phenomenon of the formation of dew, and the effects of nocturnal Radiation. MM. de Humboldt and Boussingault, by their bold ascent of the mountains of the Andes, and Gay-Lussac by his memorable aerostatic voyage, have furnished the only data of which use can be made, to determine mathematically, the true law of the superposition of atmospherical strata, as far up as these Observers ascended.—As to physical meteors, we know nothing. We scarcely know what a cloud is; in what state the aqueous particles are, which compose it, nor how they are held in aggregation. It may even be said with truth, that the instruments employed in the observation of the fundamental phenomena of meteorology, for example, pressure, temperature, the hygrometrical state of the air, have been reduced to accuracy only in later times; and even yet the determination of the proper temperature of the air, according to the indications of the thermometer, in the different conditions in which it is placed—a determination indispensable to practical astronomy—is surrounded with grave uncertainties.—We have, or believe we have, much more data concerning the general distribution over the surface of the globe, of what are called mean temperatures; a study which is certainly more simple and accessible than that of their distribution through the superior regions of the atmosphere. These data have been principally collected, as accessories to other researches, chiefly by astronomers in fixed observatories, where the observations of the barometer and thermometer are constantly necessary for the calculation of refractions; and, again, by intelligent navigators who have visited countries not explored, or but slightly explored previously; finally, by sailors belonging to the navy, who might render science the highest services by entering their daily observations in the ship's log book. To these incessant means of progress which require only intelligence and zeal—means which all enlightened governments might easily increase and render regular, without any augmentation of expense —we may add as a powerful auxiliary for the future. the efforts of free associations formed by persons especially instructed on subjects connected with meteorology. For, if—as will necessarily happen, men thus come into contact who are addicted to divers pursuits, such as physicists, geologists, botanists, agriculturists,—each of these classes will naturally look at meteorological researches from the point of view of their relations with the progress of the necessities of their favourite science. They will thus teach each other; and this concentration of divers ideas towards a common end, will doubtless, lead to the desire to see instruments perfected as much as possible, the interpretation of their indications, rendered more precise, and all great questions

·lative to the general constitution of our atmo- ·phere experimentally attacked or resolved.—It as for some time been believed that much advance iight be made in this direction by establishing ı a large number of localities, Observatories ·ecially called meteorological, in which shall be ·gularly collected, day and night, at fixed hours, ıe local indications of the barometer, thermo- ıeter, and hygrometer, placed in permanent ·tuations. This idea was first realized over ıe whole surface of Russia, in a multiplicity of ·tuations, proportionate to the extent of that vast ·mpire. There has been created there a body— n actual army—of Meteorographers, having its ·eneral, its officers, its soldiers; these having only ı fill, at the hours indicated, the sheets of ob- ·rvations sent to them, without any exercise of ·ıtelligence.—All these united lists are afterwards ·rinted, and constitute large quarto volumes, filled ·ith figures, the publication of which must un- ·oubtedly be very expensive. Analogous insti- ·ıtions have been pleaded for or established in ·ıany other parts of Europe with less gigantic ·roportions. France has not yet adopted them, ·, as their partizans express it, they have not ·et been endowed by government.—The experi- ·ıent which has been made by Russia, of these ·ecial meteorological institutions, is complete. ·heir general director is a distinguished savant; ·is principal assistants are very intelligent men; ·e and they are in possession of the most ap- ·roved and recent methods of observation.—The ·enerosity of the Czars has refused nothing which ·ould insure the success of the Institutions. ·otwithstanding, neither there nor elsewhere ·ave there been derived any real fruits from those ·ostly publications. They have produced nothing ·r the advance of meteorological science, as it ·as been above defined; and it may be added ·ot through the fault of the men, but through ·ıe want of a special aim, and from the nature ·' their organization, they can produce nothing, ·xcept masses of disjointed facts without any ·estination of foreseen utility, either for theory ·' its applications. The first part of this ·ssertion is simply the announcement of a fact: ·ıe second expresses a prophecy easily justified. ·ı the first place, in regard to the general laws ·hich regulate the statical condition of the at- ·ıosphere, one cannot reasonably expect that they ·ill ever be unfolded, or even indicated, by ob- ·rvations made in the lowest stratum of air, ·here all the causes of imaginable perturbations ·ave their particular seat, and produce at the ·ıme instant, in different localities, often near ·ıch other, sudden effects of which the difference · extreme—even from a calm to a hurricane. ·hat can be less philosophical, more opposed ·ı simple good sense, and to the experimental ·ıethod, than to attack so complex a study at its ·ıost irregular point? And can a single branch ·f physical science be quoted that has been pro- ·tably explored in such a way? Is it hoped that by means of noting these accidents, some connection will be discovered, some characteristic symptom which at least announces them? This is dearly to purchase a hope so vague; and as Sydenham said to physicians who wished to trace out the principles of disease by the description of the invalids, it is to seek the distinctive characters of a plant by the bites of the caterpillars that are there! But let us admit that these inquirers aim only at a simple description of meteorological phenomena which take place in the lower region of the atmosphere. Even then you will not obtain these by barometrical, thermometrical, and hygrometrical observations made at fixed hours. It is necessary that the intelligence of the observer should be applied to vary the intervals of observation according to the more or less rapid mutability of the phenomena; making, for example, those of the barometer more frequent at the times of the equinoxes, and repeating them nearly every minute during hurricanes, as all intelligent persons well know who have directed their attention to such meteorological accidents. The caprices of physical phenomena cannot be regulated by *ordonnances;* none of their laws have been established by rude observations—the plan of which was prescribed beforehand. It is necessary to take them by parts with much instinct and nicety, in order to perceive their laws, to follow them and disengage them from the mass, according as our subtlest judgment may enable us to disentangle them.—Failing of success in the discovery of general laws, we are next referred to the hope founded on practical applications. When, it has been said, you shall have accumulated during many years, in divers localities, masses of thermometrical and hygrometrical observations regularly made at all hours of the night and day, we shall deduce from them *mean values* which will be eminently serviceable in agriculture, vegetable physiology, the geography of plants, and consequently in the selection of the species of cultivation which may be successfully introduced into each locality. All this is yet found to be mere delusion, and, I shall add, that it will never prove otherwise.—I shall first prove the *fact:*—It is curious to see across what hesitations, with what respect for theoretical promises made to them, agriculturists and botanists have been finally led to acknowledge their almost entire inutility. They have exerted all their efforts to establish, according to the tables of mean temperatures, rules that should define the limits of the territorial zones within which the different classes of vegetables can live and be cultivated with advantage. They have found that, in fact, these rules almost always fail when applied. Those amongst them who like M. Gasparin, have fixed exactly the limits for certain vegetable species, have only succeeded by applying to local observations of temperature, an intelligent critique of the modifications rendered

necessary by a crowd of physical circumstances peculiar to each locality. The learned work recently published by M. Alphonse Decandolle, entitled, *Geographie Botanique Raisonnée*, is filled with such considerations; the epithet even which he adds to his title, sufficiently shows that the simple employment of mean temperatures as commonly observed, has not furnished him with data sufficiently approximate. But if we consider the conditions under which special meteorological observatories act, such as they are now conceived and organized at great expense, their inefficiency for such applications is an evident and necessary consequence, because the phenomenal indications which have been registered have only the most distant and incomplete relations with accidents affecting the life of vegetables. Only notice how humidity and heat are measured there—the two natural agents that most powerfully influence vegetation. At such places they observe at fixed hours the actual temperature of the surrounding air, as that is shown by a thermometer placed in a permanent exposure, sheltered from the solar rays and celestial radiation. The tension of the aqueous vapour is determined at the same moment by the hygrometer placed in like conditions; and the quantity of rain fallen is estimated by a rain-gauge fixed in the proximity of the observatory. But the impressions which the plants receive in the free air, are altogether different from what these instruments indicate; terrestrial vegetables have, so to speak, two modes of life; the one subterranean, by their roots; the other aerial, by their stalks; both of which meteorological phenomena affect very diversely. The action of the solar heat and that of nocturnal radiation are transmitted to the spongy terminals of the roots only progressively, with a slowness proportioned to the conductibility of the soil, and the depths to which these penetrate. The annual epochs of their summer and winter are quite other from those of the external air. The rain reaches them gradually by imbibition; and the quantity which they are able to absorb depends on the aptitude of the sub-soil to retain or let it pass. The aerial stalk, on the contrary, receives immediately and suddenly all meteorological impressions; the calorific and chemical radiations darted from the sun; those of nocturnal radiation; the rain which falls and covers their evaporating organs, of which they absorb a portion, and transmit it interiorly, until they have been freed from it by the heat of the sun and the agitation of the air.—What is there in all these varied phenomena that can be indicated by fixed instruments placed beyond the reach of the circumstances in which they take place, marking neither the progressive march of the one nor the suddenness of the other,—not even stating the existence of the physical actions by which the most important of them are produced? It is, nevertheless, under the influence of such causes that the alimentation of the plant takes place, —that it is retained in a condition to develop its leaves, its flowers, its fruits, and to accomplish all the vital functions which are proper to it."

[M. Biot here details at some length, and illustrates by examples, the mode in which the study of subterranean and aerial life of vegetables might be conducted, and what meteorological observations are fitted to aid in such studies. He then proceeds as follows:—]

"I have presented these details here from two motives. *First*, I have wished to show that permanent meteorological observations, such as have been established up to the present time, are not only ill adapted to throw light on the fundamental questions of scientific meteorology, but still more so to furnish the means of guiding vegetable physiology in its researches, or practical agriculture in its applications. *Secondly*, I have thought that an exact picture of the natural operations which take place in the course of the life of a vegetable, may be usefully consulted by persons desirous of establishing systems of meteorological observation, by whose application such studies might profit.—In all that M. Regnault has said of the barrenness of existing meteorological institutions, and of the causes which render this inevitable, I completely agree; and we can at least allege in favour of our opinion, that we have not reached it without having long, and from different points of view, contemplated the subject to which it refers. Nevertheless, it ought to be remarked that it applies solely to what is, and not to what might be.—The great error of existing systems of meteorological observation, is their inevitable want of a defined aim. How, indeed, could they fix one? How could they divine *à priori* the characteristic data of general laws which must from the first be presented amid a chaos of natural phenomena, and of which the determining causes, the variations, and the correspondences are almost entirely unknown? Can you pretend that useful applications to agriculture may be deduced, although the physical phenomena which must efficaciously influence vegetable life do not at all enter among your programmes? To arrive at such applications, it is necessary to study intelligently meteorological phenomena at the places themselves, and from the special point of view indicated by a clear knowledge of what you desire to discover.—Ask, for example, from the head of the practical department of the museum—M. Decaisne, five or six zealous young men already possessed of a substantial knowledge of botany and vegetable physiology; let him instruct them for some months in meteorological observation considered in its relation to vegetable life, and let him give them general instructions on this subject of study. Supply them afterwards with a small number of physical and chemical instruments, which are all that is necessary, and then distribute them over the

ountry you desire to examine. After a year ius employed, their register of observations iaced in the hands of M. Decaisne will give ore useful ideas concerning climatology and griculture, than can be supplied by twenty ermanent meteorological observatories, such as present organized; and, what is not to be over-oked, these young travellers will have gained struction as well as the State. Why must such scheme be fruitful? Simply because it would ive a special and determinate aim, and the sys-m of observations would be organized in a way accomplish that aim. Nevertheless, we by no eans pretend that to study any point whatsoever meteorological phenomena, it is necessary, or en useful to employ instruments which indicate mospherical pressure even to the hundredth rt of a millimetre, and temperatures to the ndredth part of a degree. We only ask that e indications thus registered, shall always have relation not distant and unknown, but imme-ite and certain, with the physical data that it desired to obtain. We ask, above all, that e end of the research shall be in every case inly defined, and that the system of observa-ns be suitably adapted to it. For example, as M. Le Verrier has proposed, the statical ndition of the lower atmosphere be simultane-sly obtained for many places surrounding a nmon centre at which the results shall be cussed, we do not at all think that such a dy would be useless, although not founded on al observations of the barometer, and thermo-ter accomplished with the utmost precision. e believe, on the contrary, that without this treme exactitude, perhaps even without being jected to an absolute continuity but merely the establishment of local phenomena of some portance, we would deduce therefrom con-ning the great accidental convulsions of the er strata of the atmosphere, conditions of cor-pondence which it would be extremely useful know, and which might lead to important ap-ations to the practical wants of society.—It because of their want of an aim—at least of ne precise aim—that we object to perma-it Observatories, such as have been hitherto anized."—Such is the substance of M. Biot's moir. Portions of it are very open to criti-n; but there is much truth at the bottom of rather unceremonious reclamations.

Occultation. The occulting or hiding of a by the moon is a phenomenon identical in ure with a solar eclipse.—It is well known t the moon describes an ellipse round the h, with even less approximation than most of heavenly bodies describe their orbits. Hence, he inclination of her plane be 5° · 8′ · 48″, will pass over a zone of just twice this ex-round the zodiac, and at some time will be n every part of it. Also, we must add to 5° 8′ 48″ the amount of horizontal par-x—that is the greatest angular distance which her radius can subtend at the earth's centre; because, evidently, any portion of the disc can cause the disappearance of a star behind it, and the 5° only refer to the extreme position possible for the *centre* of the disc. Then any star within a zone whose angular diameter is twice their total amount may be occulted by the moon.—We must refer to ECLIPSE for a detailed explanation of the usual phenomena. It is clear that an occultation is *possible* whenever the moon's course, seen from the earth's centre, carries her within a distance of the star equal to the sum of her semi-diameter and horizontal parallax; and *it will happen at any place*, upon her apparent path, as seen from that place, if her centre is at a less distance from the star's than the sum of her augmented semi-diameter and her actual parallax.—These are some interesting phenomena observable at occultations. They take place quite indifferently whether the moon be dark or bright, and either at her visible or invisible edge. Thus, a star may appear extinguished in mid air when no moon is shining, and, on leaving the bright limb of the moon, a luminous point appears actually to detach itself from that body. The sudden reappearance of a star on emerging from the dark edge of the moon, as well as its sudden disappearance or occultation, is a phenomenon sufficiently striking. A remarkable optical illusion—for in all likelihood it is so—is sometimes observed at occultations. The star seems to advance upon and to penetrate within the edge of the disc, sometimes to a considerable depth. It is worthy of remark, however, that this appearance has never been noticed by thoroughly practised Observers.—The main difference between solar eclipses and occultations, as will be at once seen, consists in the fact that the star, unlike the sun, has no parallax, and that its diameter subtends at the eye no perceptible angle. The reader, who wishes only general explanations of the method of calculation, may refer, therefore, to the article on ECLIPSES.—We think it right, however, to give here a more complete solution of the problem, in which the introduction of mathematical formulæ to a very considerable extent is inevitable. The first method used is that of projection. It will be clear from a moment's consideration that, to a spectator at the sun's centre, the axis of the earth appears at the solstices, inclined 23½° on different sides of the axis of the equator plane, and at the equinoxes as coincident with that line. Hence to such a spectator the extremities of this axis, the poles, will seem to describe circles of 23½° in radius projected on the sky.—In order to project an eclipse of the sun we thus see what processes are necessary,—first we must represent the earth as it would appear to the spectator at the time proposed. We must then draw the parallel of latitude for the place for which we are to determine the circumstances of the eclipse, and

mark upon it the position of the given place for different hours of the day. We must then draw the moon's apparent path across the earth's disc, and note points which it occupies at each hour of its transit. We must then find the point of the moon's path and that of the spectator, which, for the same moment, are nearest. At that time the eclipse will be deepest. Then mark the point where the distance is just equal to the sum of the semi-diameters of the sun and moon. The two points where this is *becoming* and *ceasing* are the beginning and end of the eclipse. We shall give an example from Loomis's *Practical Astronomy*. Suppose it required to obtain the times and phases of the solar eclipse, May 26, 1854, at Boston, latitude 42° 21′ 28″ N., longitude $4^h\ 44^m\ 14^s$ W. of Greenwich.—The time of new moon is found to be May 26, at $4^h\ 2{\cdot}9^m$ mean time at Boston. For this time the fundamental elements of the calculation are,—

Sun's longitude, ...	65° 12′ 32″.
Sun's declination, ...	21° 11′ 17″ N.
Moon's latitude, ...	21′ 30″ = 1290″ N.
Moon's hourly motion in longitude,	1807″.
Sun's hourly motion in longitude.	144″.
Moon's hourly motion in latitude,	167″.
Moon's equatorial horizontal parallax, ...	54′ 32″·6.
Sun's equatorial horizontal parallax, ...	8″·5.
Moon's true semi-diameter,........	14′ 53″·5.
Sun's true semi-diameter,.........	15′ 48″·9.
The geocentric latitude of Boston,	42° 10′.

The relative positions of the sun and moon will not be altered if we attribute to the moon the effect of the difference of their parallaxes, and suppose the sun to remain in its true position. This *relative* parallax for Boston is 54′ 19‴·1, or 3259‴·1, which will therefore represent the apparent semi-diameter of the earth's disc if seen at the distance of the moon from the earth. Hence the proportion of 54′ 19‴·1 to 14′ 53‴·5 will be that of the earth's to the moon's apparent

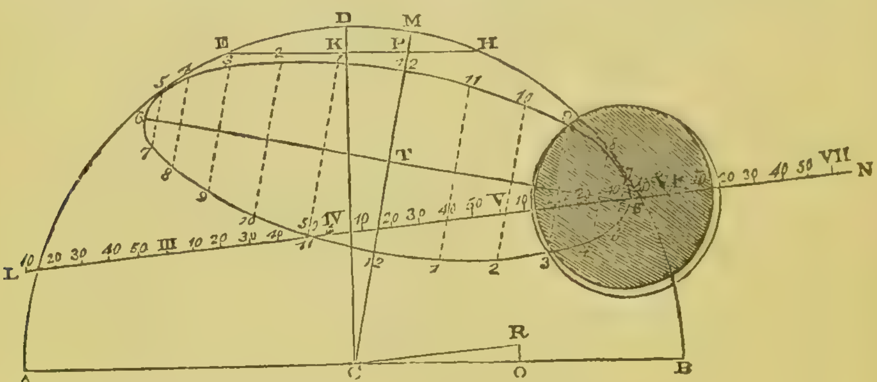


Fig. 1.

magnitude. Take A C from any convenient scale of parts, for 3259, and describe A D B to represent the northern half of the earth's disc seen from the sun. Mark off arcs D E, D H (D being the bisection of the arc), equal to 23° 28′, and draw E H. Then E H is the arc which the pole of the earth apparently describes. Hence if P be the actual position at the time given, P H will be proportional to the versed sine of the longitude of the sun—that is, to the versed sine of 65° 12′ 32″. Make it so, and draw C P M. This will represent the line of the axis. We now want to represent the parallel of latitude of Boston. If its latitude were just equal to the sun's declination, the sun at noon would be vertical, and Boston would be seen precisely at C. But since the latitude *exceeds* the declination by 20° 59′, Boston will be seen at noon at 12, if C 12 be the sine of 20° 59′ for radius A C. Similarly, if the earth were transparent, at midnight Boston would be seen at S, where C S is proportional to the sign of the *sum* of the angles of declination and latitude.—We can mark the position of any part of the day by marking off along the longest diameter 66, distances T 15, &c., which are for the radius T 6 the sines of the angles 15°, 30°, &c., and by them drawing ordinates perpendicular to the line 66. The elliptic path marked out meets the circle A D B in points corresponding to sunrise and sunset. At the time considered they are $4\frac{1}{2}$ A.M. and $7\frac{1}{2}$ P.M. We now wish to represent the moon's apparent path across the disc. From the same scale on which A C was measured, take a space 1290″ for the moon's latitude, and apply it on C D from C to G, but above A C B, because the latitude is N. Take C O equal to 1663″, the hourly motion in longitude and draw O R perpendicular to it, and equal to 167″ the motion in latitude, and draw C R. This represents on the relative orbit the hourly motion of the moon from the sun, and L G N parallel to it will be the relative orbit of the moon—then mark off positions before and after conjunction by means of the hourly motion C R—the moment of the new moon at Boston falling exactly upon the point G, when the new moon is at $4^h\ 6^m$. We can do this by the proposition $60^m : 6^m : :$ the line C R : the line G IV:—IV being the place of the moon at four o'clock.—Thus we have the circumstances graphically presented. Try by compasses to find two points, one on the moon's path and the other on that of the spectator, both of which are marked with the same times, and which are at the least distance from each other, that time—f

is case $5^h\ 44^m$—is the time for which the lipse is greatest. The appearance of the moon, projected on the earth's disc, may be seen y describing from the position of its centre at ny time a circle with the apparent semi-ameter of the moon at that time for radius. this instance the radius is 893″·5. If we scribe also from the position of Boston at e same instant, a circle with the sun's semi-ameter as radius—that is 948″·9—we shall ave the sun's disc at the figure for the moment greatest eclipse, and we see from it that the lipse will be annular.—The beginning and end the eclipse will be indicated by trying to find oints on the two paths corresponding to the me hour, the distance between which is just ginning to be and ceasing to be as great as the m of the semi-diameters—that is, as 1842″·4. e should thus have—

H.	M.			
4	30	apparent time as the beginning,		
5	44	—	—	middle,
6	51	—	—	end

the eclipse. The results are obtained in parent time because the points 1, 2, 3, &c., the parallel of Boston, correspond to that; and e phases of the moon on its apparent orbit do likewise. If the projection be carefully made e can determine the times of beginning and end ithin one or two minutes.—There are other ethods of graphical projection which depend quite similar principles. It is evident that is method labours under the grave defect of ding to the chances of error in measurements natural phenomena, the greater ones of incorrect ometrical representation. Just as we reject in igonometry methods for the description of a iangle on paper, and substitute methods for e numerical estimation of the elements to be und,—so here. We shall give, as the sub-t is of so much importance, Bessel's method of mputing solar eclipses, upon which we shall ound that of computing occultations. The dis-ssion must be entirely technical.—Let s be the

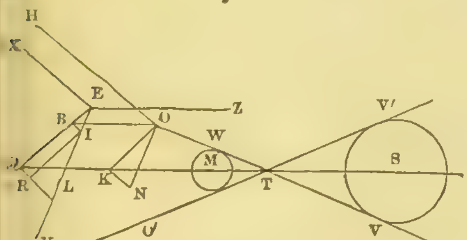


Fig. 2.

ntre of the sun, M of the moon, E of the earth, d O the observer's place. The limbs of the sun d moon will evidently appear in contact when is upon the surface of the cones circumscribing em. There are the cones V′ T V, and another ose side passes along V′ W. If O be on the rface of the first an observer at O will witness e external contact of the discs; and when on the other, the internal contact.—Let us seek the equation of the conical surfaces. Take a system of three rectangular axes—origin E. Let E Z be parallel to M S, and assume its direction positive. Suppose the positive end of this axis to have its right ascension A and its declination D. E Y is in the plane through E Z and the north pole of the equator.—The right ascension of the end Y will therefore be A and its declination $90° + D$. E X is perpendicular to the plane of the hour circle, which is clearly Z E Y, and lies in the equator, 90° from the intersection of the equator with Z E Y. The declination for the end X will be 0°, and $90 + A$ its right ascension. Let $x\,y\,z$, $x'\,y'\,z'$, $\xi\,\eta\,\zeta$, be the co-ordinates of M, S, O. Also let $MS = G$,

O T M, or O′ T M, the semi-angle of the cone $= f$

the distance of the vertex from Y E X $= s$

Then since M S is parallel to E Z,

$$x' = x,\ y' = y,\ z' = z + G \text{ ......(1).}$$

Draw T Q, O B, perpendicular to the plane X E Y, and in it Q L, B I perpendicular to E Y, and I R parallel to B Q.

$$OB = \zeta,\ EI = \eta,\ BI = \xi;\ TQ = s,\ EL = y,$$
$$QL = x$$
$$IL = y - \eta,\ RL = QL - QR = QL - BI$$
$$= x - \xi.$$

Draw the plane N O K parallel to Y E X, through O, and draw O N, O H parallel to E Y and E X. Draw K N perpendicular to O N.

Then

$$KN = RL = x - \xi,\ ON = IL = y - \eta,$$

and $\quad OK = \sqrt{(x - \xi)^2 + (y - \eta)^2}.$

Also $\quad \text{tang} f = \dfrac{OK}{TK} = \dfrac{OK}{s - \zeta}$

$$\therefore (s - \zeta)^2 \text{tang}^2 f = OK^2 =$$
$$(z - \xi)^2 + (y - \eta)^2 \text{ ....(2),}$$

which gives the conical surface for external contact. For internal contact we should have a similar result.—We may consider both sun and moon to be spheres, and let the radius be k for the moon and k' for the sun.

Then
$$ST : SV :: MT : MW$$
$$ST + TM = MS = G$$
$$ST \cdot \sin STV = SV$$
$$MT \cdot \sin MTW = MW$$
and
$$STV = MTW = f;$$
$$ST = z' - s;\ MT = s - z$$
$$\therefore z' - s : k' :: s - z : k;$$
and
$$G \sin f = k' + k.$$

Similarly for the occultation of internal contact we have

$$z' - s : k' :: z - s : k$$

and $$G \sin f = k' - k.$$

Therefore, for external contact,

$$s = \frac{z k' + z' k}{k' + k} \qquad \sin f = \frac{k' + k}{G};$$

for internal contact,

$$s = \frac{z k' - z' k}{k' - k} \qquad \sin f = \frac{k' - k}{G}.$$

It is easy to see that for external contact the angle f is always acute. For internal contact it is so also in annular eclipses, and in total eclipses it is obtuse. Eliminate s and f from the equation, and we have

$$\text{tang}^2 f = \frac{(k' \mp k)^2}{G^2 - (k' \mp k)^2}.$$

Hence,

$$(x - \xi)^2 + (y - \eta)^2 = \left(\frac{z k' \mp z' k}{k' \mp k} - \zeta\right)^2 \times \frac{(k' \mp k)^2}{G^2 - (k' \mp k)^2}$$

and reducing — $(x - \xi)^2 + (y - \eta)^2$

$$= \frac{[k' (z - \zeta) \mp k (z' - \zeta)]^2}{G^2 - (k' \mp k)^2} \ldots\ldots (3),$$

which includes, by using $+$ and $-$ where the double sign is, both external and internal contacts. Use the supplementary functions

$$= \frac{z (k' \mp k) \mp k G}{\mp \sqrt{G^2 - (k' \mp k)^2}} = z \,\text{tang} f \mp k \cdot \sec f$$

$$i = \frac{k' \mp k}{\mp \sqrt{G^2 - (k' \mp k)^2}} = \text{tang} f.$$

Substituting these, and introducing $z + G$ for z' in (3), we find

$$(x - \xi)^2 + (y - \eta^2) = (l - i\,\zeta)^2$$

and from (2) we see that $l = s \,\text{tang} f$,

which represents the radius of the circle formed by the intersection of the conical shadow with the plane through the centre of the earth, and perpendicular to E Z.—These are the fundamental processes, but the elements adopted are not directly given in ephemerides. Let us see practically how we may find x, ξ, y, η, l, i, and ζ. — Conceive a new system of rectangular axes through E. Let E Z be directed toward the north pole of the equator, E X be in the equator, and directed toward a point such that its right ascension a' is equal to that of the sun from the earth. Let E Y be directed to a point whose right ascension is $90° + a'$. Let a, δ, and r be the true right ascension, declination, and distance of the moon's centre from that of the earth; and let a', δ', and R be the same quantities for the sun's centre. — From M, the moon's centre draw on X E Y the perpendicular M N $= z$; and from N, N R $= y$, E R $= x$.—Then, from the triangle E M N,

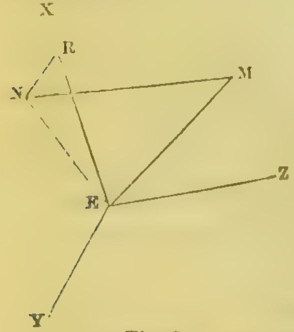


Fig. 3.

$$\text{E N} = r \cos \delta,\ z = r \sin \delta.$$

Also the angle N E R $= a - a$,

therefore $$\text{N R} = y = \text{E N} \sin (a - a')$$
$$= r \cos \delta \cdot \sin (a - a')$$
$$\text{E R} = x = r \cos \delta \cdot \cos (a - a'),$$

which give us the co-ordinates of the moon's centre, parallel to

E Z, $r \sin \cdot \delta$
E Y, $r \cos \cdot \delta \cdot \sin (a - a')$
E X, $r \cos \delta \cdot \cos (a - a')$.

Similarly, since E X has the same R · A · with the sun's centre, its co-ordinates are

R $\sin \delta'$
O
R $\cos \delta'$.

By transferring co-ordinates to M—so that the axis of z shall be directed towards the pole, that of x to a point whose R. A. is a', and that of y to one whose R. A. is $90° + a'$, we have new axes parallel to the old, and the co-ordinates of the sun's centre are

G sin D
G cos D · sin · (A $- a'$)
G cos D cos (A $- a'$).

Since A, D, G are the right ascension of the sun's centre seen from that of the moon, its declination and its distance. Hence

$$G \sin \cdot D = R \cdot \sin \delta' - r \sin \delta$$
$$G \cos D \cdot \sin (A - a') = - r \cos \delta \cdot \sin (a - a')$$
$$G \cos D \cos (A - a') = R \cos \delta' - r \cos \delta \cos (a - a').$$

Dividing each by R, and using g as an abbreviation for $\frac{G}{R}$, and e for $\frac{r}{R}$, we find

$$\left.\begin{aligned} g \sin D &= \sin \delta' - e \sin \delta \\ g \cos D \cdot \sin (A - a') &= - e \cos \cdot \delta \cdot \sin (a - a') \\ g \cos D \cdot \cos (A - a') &= \cos \delta' - e \cos \delta \cos (a - a') \end{aligned}\right\} (6).$$

From the second and third, dividing, we have

$$\text{tang} \cdot (A - a') = \frac{- e \cos \delta \cdot \sec \delta' \sin (a - a')}{1 - e \cos \delta \cdot \sec \delta' \cos (a - a')}$$

From the first and third,

$$\text{tang} \cdot \mathrm{D} = \frac{(\sin \delta' - e \cos \delta) \cos (a - a')}{\cos \delta' - e \cos \delta \cos (a - a')}.$$

And from the third,

$$g = \frac{\cos \delta' - e \cos \delta \cos (a - a')}{\cos \mathrm{D} \cos (\mathrm{A} - a')}.$$

In solar eclipses cos ($\mathrm{A} - a'$) is almost equal to 1, and therefore it may be suppressed.—By easy trigonometrical processes we may find from these formulæ,

$$\text{tang}\, (\mathrm{D} - \delta') = \frac{- \quad \sin (\delta - \delta')}{1 - e \cos (\delta - \delta')}.$$

But $\delta - \delta'$ is a very small arc. Hence nearly

$$\mathrm{D} - \delta' = \frac{- \quad \delta - \delta')}{1 - e}.$$

Similarly,

$$\mathrm{A} - a' = \frac{- e \cos \delta \cdot \sec \delta' (a - a')}{1 - e \cos \delta \cdot \sec \delta'},$$

$$\text{and } g = \frac{1 - e \cos \delta \sec \cdot \delta'}{\cos \mathrm{D} \sec \delta'},$$

$$= 1 - e \text{ very nearly.}$$

For the computation of x, y, we must return to our original axes of co-ordinates. Suppose about E any sphere to be described, upon which M represents the moon's place. Let P be the pole of the equator, and let X, Y, Z be on the sphere. Then P will be on the plane of the great circle Z Y, and the right ascensions and declinations of M, Z, Y, X are a and δ, A and D, A and 90° + D, 90 + A and 0°.

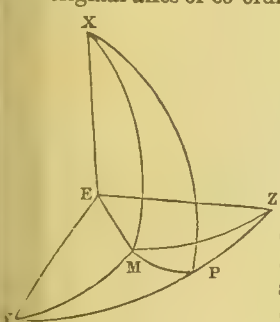


Fig. 4.

—The co-ordinates x y z of M are equal to the projections of E M $= r$, on the axes, or to r multiplied by the cosines of the axes Z M, Y M, X M, which may be obtained from the triangles Z P M, Y P M, X P M, in which Z P $= 90 - \mathrm{D}$, M P $= 90 - \delta$, Y P $= \mathrm{D}$, X P $= 90°$, Z P M $= \mathrm{A} - a$, Y P M $= 180 - (\mathrm{A} - a)$, X P M $= 90 + \mathrm{A} - a$.—Hence, by spherical trigonometry,

$$\left.\begin{aligned} z &= r \left\{\sin \mathrm{D} \cdot \sin \delta \cdot + \cos \mathrm{D} \cdot \cos \cdot \delta \cdot \cos (a - \mathrm{A})\right\} \\ y &= r \left\{\cos \mathrm{D} \cdot \sin \delta - \sin \mathrm{D} \cos \delta \cdot \cos (a - \mathrm{A})\right\} \\ x &= r \cos \delta \cdot \sin (a - \mathrm{A}). \end{aligned}\right\} (7).$$

We can readily change the first two expressions into

$$z = r \left[\cos (\delta - \mathrm{D}) \cos^2 \tfrac{1}{2} (a - \mathrm{A}) - \cos (\delta + \mathrm{D}) \sin^2 \tfrac{1}{2} (a - \mathrm{A})\right],$$

$$y = r \left[\sin (\delta - \mathrm{D}) \cos^2 \tfrac{1}{2} (a - \mathrm{A}) + \sin (\delta + \mathrm{D}) \sin^2 \tfrac{1}{2} (a - \mathrm{A})\right].$$

From these we find z' y' x' by means of the equations

$$z' = z + \mathrm{G},\ y' = y,\ x' = x$$

Again, suppose M to represent not the moon's centre but the geocentric zenith of the observer, then the declination of M will be equal to ϕ' the geocentric latitude of the place; and the R · A · to μ, the sidereal time of the observer, expressed in degrees. If the observer's distance from the earth's centre be ϱ, we find the values of ξ, η, ζ by substituting in (7), ϱ, μ, ϕ' for r, a, δ. Thus

$$\zeta = \varrho \left\{\sin \mathrm{D} \cdot \sin \phi' + \cos \mathrm{D} \cdot \cos \phi' \cos (\mu - \mathrm{A})\right\}$$

$$\eta = \varrho \left\{\cos \mathrm{D} \cdot \sin \phi' - \sin \mathrm{D} \cdot \cos \phi' \cos (\mu - \mathrm{A})\right\}$$

$$\xi = \varrho \cos \phi' \sin (\mu - \mathrm{A}).$$

We may take any unit of length for r, R, ϱ. Bessel chooses the equatorial radius of the earth. If the moon's equatorial horizontal parallax be represented by ϖ, the sun's mean horizontal parallax by ϖ', and the distance from the earth to the sun's centre by r', where the mean distance of the earth from the sun is taken as unity, we find

$$r = \frac{1}{\sin \varpi} \cdot \ \mathrm{R} = \frac{r'}{\sin \varpi'}.$$

If H represent the sun's mean radius for $r' = 1$, then the linear radius, or k is $k = \dfrac{\sin \mathrm{H}}{\sin \varpi'}$. Consequently, for all solar eclipses,

$$\left.\begin{aligned} e &= \frac{\sin \varpi'}{r' \sin \varpi} \cdot \ g = \frac{\mathrm{G} \sin \varpi'}{r'} \\ \sin f &= \frac{\sin \mathrm{H} \pm \mathrm{K} \sin \varpi}{r' g} \\ s \text{ tang} \cdot f &= l = z \text{ tang} f \pm \mathrm{K} \cdot \sec f \end{aligned}\right\} (10).$$

The numerator of the expression for $\sin f$ is constant for all solar eclipses. Generally we find from observation and tables,

$$\left.\begin{aligned} \log \sin \varpi' &= 5{\cdot}6189407 \\ \log (\sin \mathrm{H} + k \sin \varpi') &= 7{\cdot}6688050 \\ \log (\sin \mathrm{H} - k \sin \varpi') &= 7{\cdot}6666896 \end{aligned}\right\} (11).$$

There are additional formulæ for the hourly variations, times of commencement, and end, &c., which are investigated in quite similar ways.

We give merely their results in the following recapitulation of what Bessel's method for calculating eclipses requires us to do.—Let T represent a convenient assumed time near conjunction. Take from the *Nautical Almanack* for two or three full hours before and after T, these quantities

a = moon's R·A, a' = sun's R·A, δ = moon's declination.

δ' = sun's declination, ϖ = moon's equatorial horizontal parallax.

r' = earth's radius vector.

Then compute

$$e = \frac{\sin 8'' \cdot 5776}{r \sin \varpi}, \qquad r = \frac{1}{\sin \varpi}.$$

We find $\log \sin \cdot 8'' \cdot 5776 = 5{\cdot}6189407$.

Also $$\text{A} = a' - \frac{e \cos \delta \cdot \sec \delta' (a - a')}{1 - e \cos \delta \cdot \sec \delta'}$$

$$\text{D} = \delta' - \frac{e(\delta - \delta')}{1 - e}$$

$$g = \frac{1 - e \cos \delta \cdot \sec \delta'}{\cos \text{D} \cdot \sec \delta'}$$

$$x = r \cos \delta \cdot \sin (a - \text{A})$$

$$y = r \sin (\delta - \text{D}) \cos^2 \tfrac{1}{2} (a - \text{A}) + r \sin (\delta + \text{D}) \sin^2 \tfrac{1}{2} (a - \text{A})$$

$$z = r \cos (\delta - \text{D}) \cos^2 \tfrac{1}{2} (a - \text{A}) + r \cos (\delta + \text{D}) \sin^2 \tfrac{1}{2} (a - r)$$

$$\sin f = \frac{7{\cdot}6688050}{r' g} \text{ or } \frac{7{\cdot}6666896}{r' g}$$

$$i = \text{tang} \cdot f$$

$$k = {\cdot}2725$$

$$l = z \,\text{tang} f \mp k \sec f.$$

Also, for the special place of observation, look up

μ = the sidereal time

μ' = the same for Greenwich

ω = the longitude E. from Greenwich

ϕ' = the geocentric latitude

ρ = the earth's radius at the place.

Also compute from these

$$\xi = \rho \cos \phi' \sin (\mu - \text{A})$$

$$\eta = \rho \sin \phi' \cos \text{D} - \rho \cos \phi' \sin d \cos (\mu - \text{A})$$

$$\zeta = \rho \sin \phi' \sin \text{D} + \rho \cos \phi' \cos \text{D} \cdot \cos (\mu - \text{A})$$

$$d\xi = \rho \cos \phi' \cos (\mu - \text{A})\, d (\mu - \text{A})$$

$$d\eta = \xi \sin \text{D} \cdot d (\mu - \text{A}) - \zeta \, d\text{D}$$

$$m \sin \text{M} = x - \xi$$

$$m \cos \text{M} = y - \eta$$

OCC

$$n \sin \text{N} = x' - \delta \xi$$

$$n \cos \text{N} = y' - \delta \eta$$

x' = the hourly variation of x

y' = the same for y

m and n being taken always positive

$$l - i \zeta = \text{L}$$

$$\sin \Psi = \frac{m}{\text{L}} \sin (\text{M} - \text{N})$$

Ψ being in the first or fourth quadrant.

For beginning of eclipse,

$$t_1 = -\frac{m}{n} \cos (\text{M} - \text{N}) - \frac{\text{L}}{n} \cos \Psi.$$

For end of eclipse,

$$t_2 = -\frac{m}{n} \cos (\text{M} - \text{N}) + \frac{\text{L}}{n} \cos \Psi.$$

Time of beginning of eclipse,

$$= \text{T} + \omega + t_1.$$

Time of end of eclipse,

$$= \text{T} + \omega + t_2.$$

Angle from north point for beginning,

$$= 180 + \text{N} - \Psi = \text{Q}_1.$$

Angle from north point for end,

$$= \text{N} + \Psi = \text{Q}_2.$$

Angle from vertex,

$$= \text{Q} + \text{P} = \text{V}.$$

These computations need only a few modifications to be adapted to the occultation of a star. There is for this case no diurnal parallax, and so distant is the star, that we may consider the rays touching the moon's disc as in the shape of a cone infinite in length and finite in breadth, that is, a cylinder. Hence f, i, and the horizontal parallax of the star become each zero, a' = A, δ' = D, l = K. Also, we need not compute z or ζ. Since A besides is invariable, being the R·A of the star. $d(\mu - \text{A}) = d\mu$, which in one solar hour is $1^h\ 0^m\ 9^s \cdot 8565$;—easily reduced to degrees, and to the proportion of the arc to the radius.—Examples of the practical working of eclipses and occultations consist of the mere numerical realization of these results. The reader will find several examples fully wrought out in Loomis's *Practical Astronomy*, from which much of the preceding article has been taken. He is referred also to a most serviceable and ingenious memoir by the Rev. Temple Chevallier, in vol. xix. *Memoirs of the Astronomical Society*, for a separate method of finding the effect of differences of parallax in occultations. This enables us to dispense with the more laborious process, except for some standard positions for which the results are given in the almanack, to which results we merely require to apply corrections.

Ocean. The vast collection of waters on the globe. For the most part they occupy the lower or depressed flats,—which in our globe are found in greatest number and continuity in the Southern Hemisphere, just as the oceanic portion of the Moon must be in the Hemisphere turned away from us. By calculation the actual surface of the globe is about 197,000,000 square miles; and of this 145,000,000 are covered by the ocean. The depths of this mass of water greatly varies; a fact of great importance because of its influence on the tidal wave. Many phenomena of the ocean do not at present come under our cognizance—such as its saltness, &c. The regular movements that agitate it, are described under TIDES, and the article immediately subsequent to this one.

Oceanic Currents. Independently of the motions of the waters occasioned by the tides, by differences of level, and by saltness, the surface of the ocean is furrowed by currents, whose direction it is of great importance the navigator should know, that he may follow or avoid them as he requires. Although these phenomena—which concern navigation and terrestrial physics in the highest degree, have been an object of study by all navigators, we, nevertheless, do not yet know the whole causes of their production. Before entering into details on this subject, it is necessary to ascertain the direction, the position, and the extent of the different currents which manifest themselves in the mass of the ocean.—Currents may be classed under three great divisions, comprehending, 1st, Constant Currents; 2d, Periodical Currents; 3d, Variable and Accidental Currents.—The currents of the two latter divisions are less extended than those which constitute the first;—we shall pass them successively under review.

I. *Constant Currents.*—Captain Duperrey, in marking on a map, his own observations, and those obtained by the voyages of Cook, Baudin, Flinders, and other navigators, has succeeded in finding the direction of the great currents which circulate over the surface of the sea, and in connecting all the partial currents with each other.—We are about to endeavour to show what, according to him, is the distribution of those currents which act so important a part in the transport of all organized bodies from one country to another.—The South Pole is the starting point of three currents of *cold* water. The first, is directed towards the east, and strikes the western coast of South America, where it divides into two branches about the 40° of south latitude; the southern branch coasts along Patagonia, turns round Cape Horn and warms its shores, because it comes from lower latitudes and runs into colder regions; the northern branch, which is the most extended, on the contrary, coasts along Peru and Chili, and softens the climate of the countries near the equator, which differs, as is well known, considerably from that of Brazil. This northern current, after having coasted along Peru, takes a bend through the Pacific, in a direction from east to west, and constitutes that which formerly was called the great equatorial current. New Zealand is placed in the midst of that part of the ocean which is not affected by the principal current, or by that branch of it which has followed the equinoctial line.—To the equatorial current of the southern hemisphere another is joined in the same direction, coming from the northern hemisphere, and which is separated from it by a space of 7° in breadth, mainly situated to the north of the line. and where there is a sort of eddy, that takes a direction contrary to that of the currents. These two currents are warmed in approaching the Carolines, and their path is continually interfered with by the various islands. In the vicinity of the great Asiatic Archipelago, this great current divides itself into two branches. The one flows towards the north, the other towards the south. The first branch takes a direction primarily towards the north and north-east, passing between the Marianne and Japan islands, then coasting along the Aleutian islands, but without reaching Kamtschatka; before coming there it turns to the east, then to the south, falling back again into the great equinoctial current about the termination of the peninsula of California.—The second branch flows towards the eastern side of New Holland, turns round New Zealand, rejoins the southern central current, which it warms in its western portion; extends below Van Diemen's Land, and mixes with a branch of the *second* Austral polar current rising in the meridian of the Indies, and directing itself towards the east, like all currents of cold water.—This *second* great current of cold water, situated to the west of the central current, strikes on the western coast of New Holland, flowing northward towards the islands of Java and Sumatra, where it joins that part of the great equinoctial current, which has not been arrested in the Asiatic archipelago by the numerous obstacles it has met in its course. This current flows westward, takes a route towards the south, between Africa and Madagascar, and turns round the Cape of Good Hope, where it is a current of hot water. It then unites again with the *third* southern current, which passes along the western coast of Africa. It serves to feed the equinoctial current of the Atlantic, and forms the Gulf Stream, for an admirable description of which we are indebted to Humboldt. This current enters the Gulf of Mexico, passes round it, issues by the Straits of Bahama, runs from the south-west to the north-east at a certain distance from the coast of the United States, preserving a more or less considerable portion of the temperature which it had between the tropics. It then divides itself into two branches; one of which flows northward, modifying the climate of Iceland, the Orkney and the Shetland Islands, and Norway; the other branch makes a gradual sweep, and ends by

retracing its course, traversing the Atlantic from north to south, sometimes very nearly approaching the shores of Spain and Portugal. After a long circuit, the waters rejoin the equinoctial current from whence they issued.—This great equinoctial current of the Atlantic extends over a considerable space on each side of the line, varying according to the apparent situation of the sun. From the 16° to the 30° of latitude is the limits assigned to it. Beginning to be perceptible at the south-west of the Azores, it is very feeble at the 15° to the 25° of latitude. Near the line its direction is less steady than towards the 10° from 15°, and it directs itself towards the Bay of Honduras, traverses the Gulf of Mexico, and throws itself with impetuosity into the straits of Bahama, with a velocity of two *mêters* a second, notwithstanding the north winds which constantly prevail in this region: after this, it follows the direction indicated above.—About the 33° of latitude, a navigator may, according to Humboldt, pass in the same day from the eastern into the western current.—At Cape Blanco, this current, after flowing along the coast of Africa, takes a direction first towards the south-west, and re-unites itself to the Great Equatorial current, called the Gulf Stream. The two currents are separated by a zone of 140 leagues in breadth. It is estimated that the waters of this great current, according to the velocities observed, go through a circuit of 3,800 leagues in the space of three years; viz., thirteen months in going from the Canary Islands to the coast of the Caraccas; ten in making the circuit of the Gulf of Mexico; two in reaching the banks of Newfoundland, and from ten to eleven in returning to the coast of Africa. At about the 45° or 50° of latitude, the Gulf Stream directs one of its branches towards the north-east, upon the coast of Europe.—The temperatures of this immense current, which plays so important a part in navigation, are not without interest. Between the 40° and 41° of latitude, its temperature is 64°; under the parallel of Charleston it is 68°, and the waters outside it are 57°. Near the banks of Newfoundland it is only from 44° to 46°.—Such, then, according to Captain Duperrey, are the effects produced by the great Austral central current, whose course he has traced over the whole surface of the ocean with great sagacity. Let us take this current when it strikes the coast of Chili, where one portion of it ascends towards the equator, and the other descends towards Cape Horn. That which directs itself towards the south, appears to have cut up the western coasts of Patagonia, produced the islands which surround it, and separated Terra del Fuego from the continent. This current renders the passage round Cape Horn very difficult. To escape it, it is needful to sail to a considerable distance. That portion which goes northward appears to have hollowed out that great indentation on the western coast of America, between the 25° and the 15° of latitude, thus contributing to give it its present configuration. The current, which exercises so great an influence upon the southern coasts of South America, is intimately connected with the general direction of the winds, and, in consequence, with the progress of the sun. In reality, it has been observed that the current ascends towards the north when our luminary is in the northern hemisphere, whilst it descends towards the other side of the line when the sun is in the southern hemisphere. The measures of the temperature of the waters of this current, before it reaches the shores of America, leave no doubt on this point. In fact, between 105° and 90° of longitude, the thermometer indicates, in the month of January, 39°, whilst after having touched the coast, the branch which runs in the direction of Cape Horn, has a temperature of 48° in the very offings of the coast. This effect cannot be attributed to continental heat, by reason of its low temperature;—but to this—that, from the point of departure of this portion of the current, the temperature of the sea is higher than that of the air; on the coasts of Peru, the contrary takes place. Thus, the principal branch of the current flowing from the South pole, becomes so far warmed in approaching the 30° of latitude, as to raise it to a temperature higher than that of Chili, whose climate it ameliorates, whilst the portion which flows towards the north being inferior to that of Peru, necessarily lowers its climate. The influence which the heat of the current exercises upon the temperature of the coast, has explained certain facts which had not before been understood. In Peru, where, owing to the action of the current, the temperature is very mild, the inhabitants cultivate the ground without the help of slaves, and consequently, the Spanish colonies have been preserved intact.—It is otherwise in Brazil—under the same parallels of latitude, the excessive heat has induced the Portuguese to call in the aid of the African in the cultivation of the soil. The like reason explains how it is that vegetation presents the same characteristic features in Chili as in Terra del Fuego, and that the humming bird is found in Chili as well as at Cape Horn.—The direction of the currents from east to west, in the great ocean, also shows us how it is that the natives of the South Sea Islands have never reached America with their canoes.—Major Rennel thinks that the immense mass of fucus which covers the sea to the west of the Azores, to an extent comparable to that of France (which is called the grassy sea), is brought by the Gulf Stream from the Gulf of Mexico. The opinions, however, of navigators on this point, are by no means uniform. Some think that these fuci may have grown upon the reefs at the bottom of the ocean in its shallower parts, and that they vegetate up to the very surface of the waters. Besides, according to the observations of Humboldt and Major Rennel, this grassy sea, which was

first observed by Columbus, has not changed its place either in latitude or longitude.—Captain Duperrey has made a special study, in his navigation in the *Coquille*, of the current of hot water which passes between New Holland and New Zealand, to rejoin the central southern current.—We give here some thermometrical observations which he has made, the one to the east, the other to the west, of New Holland:—

December, 1823.

Latitude.	Longitude.	Temp. Air.	Tenp Sea.
33°28′ S.	102° 8′ E.	61°70′	62°78′
33 51	150 32	69 62	71 6
36 10	104 30	57 92	58 64
37 51	107 21	57 92	58 64
37 19	153 22	67 64	67 28
39 59	109 39	55 76	54 86
42 38	116 42	53 78	51 98
43 43	124 36	51 26	51 26
43 40	151 39	58 46	58 28
46 44	146 67	59 0	54 86

It is remarked that the observations on the east side give much higher numbers than those on the west coast. The presence of the current is indicated by a difference of more than 7° of temperature in passing from 43°·40′ to 43°. At the latitude of 46°44 the current has a velocity of 21 miles towards the south, and of 42 miles towards the east.—The following table presents the results of other thermometrical observations made by Captain Duperrey, relative to the current of hot water between the Japan and Marianne islands. Each of these temperatures is the mean of all the observations made every two hours in the four-and-twenty hours:—

Dates, 1819.	Latitude.	Longitude.	Temp. Air.	Temp. Sea.
24 June......	24°00′ N.	153°39′ E.	80°76′	81°32′
3 August...	23 44	205 50	73 4	73 76
1 July......	26 16	156 56	80 6	78 26
2 August....	26 1	205 5	71 6	72 50
4 July.......	30 29	160 22	76 46	80 6
31 July.......	30 29	203 45	71 78	71 96
6 July.......	32 59	160 47	76 46	76 46
30 July..	32 55	203 17	71 78	71 96
10 July.......	36 44	163 45	72 14	71 24
28 July.......	36 43	201 42	72 50	72 32
19 July........	51 12	185 5	65 48	63 32
20 July....,..	41 2	188 49	66 92	61 88

—The currents belonging to the north pole produce very remarkable effects. It is they which bring down upon the coast of Iceland that enormous quantity of ice filling the northern gulfs. Sometimes, instead of ice, they transport there immense masses of floating wood. These trees, according to all appearance, come from Siberia and Northern America. They are similar in kind with those found growing in Mexico and Brazil. They are generally the trunks of pines, firs, and some of resinous and mahogany trees. These last are invariably pierced by the teredo—a testaceous mollusc.—We shall mention a few partial currents to complete the sketch we have drawn of the constant currents which furrow the ocean. A current is found in Gascony, directed towards the north-east, which is probably only a portion of the Gulf Stream. In the Mediterranean there exists another which washes the northern shores of Africa. This current flows towards the north-east to the coasts of Syria, seems arrested at the island of Candia, returns upon Sicily, and thence to the coast of Spain. Is this current a part of the Gulf Stream, or does it not rather depend on the unequal evaporation of the waters of the Mediterranean and the ocean? The latter hypothesis is the more probable. Currents are likewise found in the straits of Constantinople—in the Dardanelles, and in the Greek Archipelago; all directed towards the Mediterranean.

II. *Periodic Currents.*—The equinoctial current, as we have said above, is modified in approaching the great Asiatic Archipelago, by the monsoons; this affords a proof of the influence of the displacement of masses of air on marine currents. Numerous examples of periodic currents are found along the coasts, especially in the Indian and Chinese Seas, where the sun is in his southern or northern declination. 1st. Thus, in the Gulf of Manar, between Ceylon and Cape Comorin, there is a current directed towards the north from the month of May to the month of October, and passes to the south-west or south-south-west during the other months; its ordinary velocity at the coast is a league an hour. Along the coast of Ceylon, from Point Pedro on the north of the island to the Point de Galle on the south, currents prevail directed towards the south and south-east, and along the line of the coast, and which have but little force in June and November. In the Bay of Bengal, the monsoons give rise to marine currents during their continuance. In the Chinese seas, the currents are generally directed towards the north-east, from the 15th of May to the 15th of August, and have a contrary direction from October to March or April. The velocity is in general greater during October, November, and December, than that of the opposite currents in May, June, and July. The strongest current in these seas is that which is near the coast of Cambodia, and which goes southward at the end of November with a velocity of a metre to a metre and a-half per second. These currents are felt in other regions near the coast of Africa, and in America. Examples might be multiplied of periodical currents, showing the influence of the seasons on their direction; but they have not an extent so great as the great general currents of which we have spoken. Periodical currents are observed, which have in part for their cause the unusual evaporation of the waters, combined with the effects of the prevailing winds. It is thus, that a current exists from the ocean into the Red Sea from October to May, and a current of an opposite direction during the rest of the year. The waters of the Persian Gulf present an inverse effect; from October to May, a marine current runs from the gulf into the ocean, and from May to October, from the ocean into the gulf.

III. *Variable and Accidental Currents.*—These currents are not dependent on any regular

law; they are produced either by the influence of variable winds which prevail along a coast during many days, or by tempests, or water-spouts, &c. We shall notice only slightly movements which are independent of winds and of tides, and which sometimes produce small currents in the surface of the sea; Humboldt has thus described them in his travels in America:—"When the sea is perfectly calm, there appear on its surface narrow belts, like small rivulets, and in which the water runs with a noise very perceptible to the ear of an experienced pilot. On the 15th of June, in about 34° 36′ of north latitude we found ourselves in the midst of a great number of these beds of currents; we were able to determine their direction by the compass. Some were flowing to the north-east, others to the east-north-east, although the general motion of the ocean indicated by a comparison of the log and of the chronometrical longitude continued to be towards the south-east. It is very common to see a mass of motionless water crossed by ridges of water, which run in different directions. This phenomenon may be observed every day in the surface of our lakes; but it is more rare to find partial movements, impressed by local causes on small portions of water in the midst of an oceanic river occupying an immense space and moving in a constant direction, although with an inconsiderable velocity. In this conflict of currents, as in the oscillation of waves, our imagination is struck with these movements which seem to penetrate each other, and by which the ocean is incessantly agitated."

IV. *Cause of Currents.*—Many causes have been assigned by Franklin, Bennet, &c. Bernouilli has admitted that as the motion of the rotation of the earth must leave behind it the waters and the atmosphere, there will result marine and atmospherical equatorial currents in a direction contrary to the movement of the rotation of the earth from east to west. D'Alembert and La Place have not found the reasons of Bernouilli sufficient to explain marine currents and trade winds. Some think that the trade winds—of which we shall speak under WINDS — which blow constantly in the equatorial seas from east to west, produce a liquid intumescence in the vicinity of the equator on the eastern coast of Africa, from which results an accumulated mass of water that pours without ceasing from north to south by the straits of Mozambique. Arrived at the Cape—being there no longer arrested by the African coast—it precipitates towards the west. Such is the cause which has been assigned for the current off the Cape of Good Hope. According to this mode of viewing it, the equinoctial current of the Atlantic would have a similar origin; in fact, the constant impulse of the trade wind upon the waters which adjoin the equator to the north and to the south, must also produce a great accumulation of water on the coast of America; from thence arises a general motion of the Caribbean Sea, towards the strait which separates the eastern point of Yucatan from the western point of Cuba; whence an elevation of the level of the sea in the Gulf of Mexico, and consequently, the production of that current which, in issuing by the straits of Bahama, forms the Gulf Stream. The current at the Straits of Gibraltar is attributed to the lower level of the Mediterranean, owing to an abundant evaporation, not nearly compensated for by the waters delivered by the rivers into this sea; an evaporation much more powerful than that usually taking place in consequence of the elevated temperature of the Mediterranean. It is impossible to give a complete theory of marine currents, ignorant as we are of the entire causes which combine in producing them. It is not enough indeed, to know merely the currents which furrow the surface of the seas, but we ought besides to have some idea of the currents produced by the differences of temperature and of saltness at different depths, and of these sub-marine currents, in contact with the bed of the sea, which convey the cold waters of the polar regions as far as the equator. On the other hand, the waters of the ocean, near the poles, are moved from west to east with a very feeble velocity in consequence of the diurnal motion; in making their way towards the temperate and hot regions their motion is accelerated, and there results relative currents directed from the east towards the west. The currents that we have described are thus only effects issuing from the final state in which the surface of the sea is found, under the combined influence of many very complex causes.

Octans. One of Lacaille's constellations round the south pole. It has no stars larger than the fifth magnitude.

Octant. Properly the eighth part of a circular arc or angle. Hence the instrument called Hadley's Quadrant—which from its peculiar construction is of the form of an octant—is sometimes called alternatively by this name. The word is also used in astrology, as equivalent to the *octile* aspect.

Octave. In music, is the sum of the notes of the scale. The ratio of the note at its one extremity to that at the other is 2 to 1. It embraces all possible primitive sounds; and although we wish to have any higher or lower, we cannot change the notes essentially in character as perceived by the ear. One may be louder than the other, but the same note is repeated. See ACOUSTICS.

Opacity. That property which matter has of withstanding the rays of light. All matter does to some extent absorb these rays; and some, instead of allowing it to pass or absorbing it, prefer rather to throw it back (REFLECTION). These effects are quite to be anticipated from the theory that light is caused by the rapid vibrations or undulations of a very thin ether. The metals are the most opaque of all existing bodies. The

full cause of opacity and the state of the interior particles dependent on it, is not yet understood.

Opposition. See CONJUNCTION for the Astronomical, and ASPECT for the Astrological connection of the term.

Optics. The science which treats of the phenomena of light. It is concerned with the causes of light, and the changes which it undergoes in given circumstances. Many of the empirical laws of these changes had been discovered before any settled theory of the causes of light had been arrived at, and are even still, better expounded, when all consideration of the intimate character of light, and of the physical facts which truly correspond to the laws which we observe and deduce, are dismissed from the mind. These phenomena, once inferred from the empirical laws, take their proper place in a complete physical theory, along with these laws. The branch of optics which treats of such laws and phenomena, dismissing in this manner all purely physical ideas, is appropriately called *geometrical optics.* Its theorems are, in fact, just like algebraical theorems, applicable to any real quantities, by the proper substitutions, and it is only in *making* these substitutions that the science at all differs from pure mathematics.—Optics treats of three points,—the reflection, the refraction, and the diffusion of light. The former two have been already discussed in the articles on CATOPTRICS and DIOPTRICS. We shall proceed very briefly to consider the latter. Its subject is the *intensity* of light, or the question as to the number of equally bright rays falling upon surfaces of equal area. This gives a measure of relative intensity, not an absolute unit of intensity. The latter might be readily fixed by taking the intensity produced by a light easily procurable of a fixed amount—as of a gas-light having a fixed size of jet, and burning gas obtained in a standard way, placed at a standard distance. For relative intensity, however, the definition above given is sufficient. — One law, upon which diffusion depends is this, that the rays of light proceed through uniform media in straight lines. Assuming this, it is easy to show that the intensity of light on a small illuminated surface, is to the intensity on another such small surface illuminated from the same point, inversely as the squares of the perpendicular distances of the surfaces from the point. The same rays, emerging from the point spread over spaces, at different distances proportional to the squares of the distances. —We have supposed that the light falls perpendicularly on the surfaces. It often does not so. Suppose it to fall obliquely on a little surface. Take any oblique line B D, and let A be the luminous point. Then the rays falling upon B D are exactly the same as those which fall upon the perpendicular line, B C — and if B C and B D be small, we may imagine all the rays falling on B C to be perpendicular to it. Then the rays falling upon B D, will give a less intensity at every point of it, than the same rays give upon B C, in the proportion of B C to B D (inverse proportion of surfaces on which equal amounts of light fall). But B C bears to B D the ratio of the cosine of the angle C B D (or A B C), which is the angle of incidence of the rays upon B D, to one. Hence the intensity of the light on any small space, B D will be proportional directly to the cosine of the angle at which the luminous rays fall upon the space, and inversely to the square of the distance of any point. This law is included in that already stated.—There is still one other proposition. It is this, that objects appear *equally luminous* at whatever distance from them we may stand, not increasing in luminosity as we approach, or diminishing as we recede.—As we approach, the light from a surface, caught by the eye increases, and as we recede, diminishes as the square of the distance undoubtedly—but the surface has appeared to increase and diminish in exactly the same ratio. It subtends at half any distance from the eye, twice as great a lineal angle, and four times as great a solid angle as before. Hence, though the light has increased to four times its original amount, yet the surface on which it is spread, which is always measured by the solid angle which it subtends at the eye, is increased by an amount exactly proportional. Therefore the two —the apparent surface, and the real amount of light—increase or diminish together at the same rate, and the body appears equally luminous, whether we advance or recede from it.—For the Physical theory of Optics, see COLOURS, DIFFRACTION, LIGHT, POLARIZATION, and other special articles.—We recommend especially Sir David Brewster's general treatise on Optics; and beyond everything else, the most remarkable set of diagrams by Engel and Schellbach.

Optics, Practical. A rather vague term for that branch of Optics yet open to experimental inquiry. Thus we know that the law of refraction gives a definite refractive index. Practical Optics seeks to determine for each body what this is. Like all other sciences called practical, it is connected with inquiries which no theory yet ventures almost to propose—such as the law which establishes for each body its especial refractive index. It is concerned also with the construction and use of optical instruments, such as the Camera, the Heliostat, Microscope, Telescope, &c. (*q.v.*); and all those branches of what we have called geometrical or physical optics, relating to the various contrivances for dividing and sub-dividing space in optical instruments, are generally also included within it.

Orb, is the old name for the supposititious crystal spheres of the ancient astronomy, in which the planets were supposed to move. It signified originally any round body, and hence now that that theory is exploded, the term is

generally applied to the mass of a planet or satellite, or of the sun—all nearly circular.

Orbit,—means the relative path in which a planet travels round the sun, or a satellite round its primary. In explaining the heavenly motions, and even sometimes in conceiving them, it is useful to take for the really complicated figures through which the various bodies pass, others much simpler but not unlike them in outline. Thus we may suppose that the earth moves in an uniform circular path (see ANOMALY), round the sun, and so with the other planets and with the satellites. In more delicate explanations we take a figure still more closely resembling the actual orbit—the ellipse in which each planet would move were it not for the perturbations, or attractive influences from other planets. When, again, we seek to be still more exact, we suppose that the planet does actually move in such an orbit, which, however, itself undergoes slight occasional changes. Thus the motion would be represented by a body whirling round an elliptic rod of iron at a definite rate, which rod sometimes expands or contracts from heat while the motion is proceeding; sometimes is tilted round a little way in one or another direction. This would represent with tolerable accuracy the state of the case. The circular path, or the elliptic path, is generally called the orbit. The true orbit is the curiously twisted spiral curve through which the body really passes. —To illustrate this subject, let us reflect on the orbit of our moon. If that body were influenced solely by the earth, her path would be a regular and stable ellipse; but since the great central orb is so near us, and acts over all the sphere within which the moon moves, with a power adequate to control the earth as his subservient, it must be evident that our satellite cannot escape being *disturbed*, or greatly affected, by the presence of that Luminary. Again, however, if the sun acted on the earth and moon *alike*, he could not disturb by such action the *mutual relations* of these bodies; no more, indeed, than any artificial system, placed on a board, would be disturbed by the board on which it lies being carried to a different place. But inasmuch as the moon's distance from the sun is not always the same as the earth's, and, moreover, varies regularly as our midnight luminary revolves in its orbit, the Sun cannot act on both globes with the *same energy*, and must therefore, by *disturbing* their relations, change the moon's path. Let us follow this action to one remarkable consequence. In the figure below, suppose E the earth, M′ M″ M‴ M⁗ the moon's orbit, and S the sun. Now, as the sun's power over any body increases as the distance of that body from him diminishes, the moon when at M′ must be attracted by that luminary more than the earth is, or pulled away from the earth; while, on the contrary, when the moon is at M‴, the earth will, for the same reason, be pulled away from her. At these two parts of the orbit, then, the sun must tend *to separate* the two bodies; or, what is virtually the same thing,

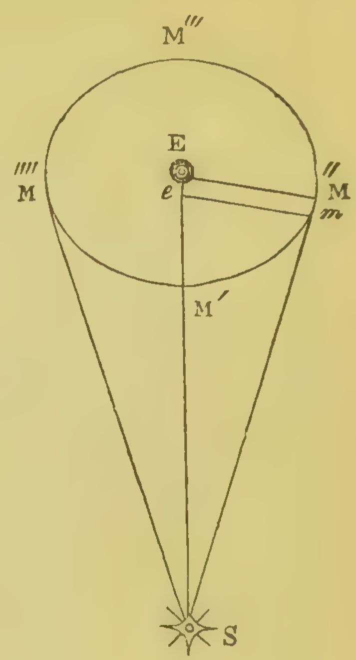


Fig. 1.

to *diminish the attractive power of the earth over its satellite* at new and full moon. Again, when the moon is at M″ and M⁗, or at the same distance as the earth from the sun, both bodies must be attracted with an equal *force*; but the *direction* of that force is not the same in the two cases. Suppose the sun to have the power of making the earth move through E *e* in a given time, he would cause the moon move through M″ *m* in that time; and the effect would manifestly be to bring the two bodies *nearer each other*, because they are being made to slide along within a wedge which grows narrower and narrower. The case would be the same at M⁗; so that we may say that the effect of the disturbing force of the sun on the moon at M″ and M⁗, or when she is *in quarter*, is to force her *nearer* the earth, or, what is the same thing, virtually to *increase* the *attractive power of the earth* over her. We thus obtain the following proposition:—About the region of *full and new moon*, our satellite is *less than normally* attracted by the earth, because of the disturbing effect of the sun; while about the region of *her quadrature* the earth's power over her *is increased* by the same cause.—Turn now to consideration of her *velocities* at these parts of her orbit: and suppose her travelling in the direction from M⁗ to M′. While passing from M⁗ to M′, she is plainly drawn *forward* by the sun, or her velocity accordingly is augmented by that disturbing force until she reaches M′. In the same way, while passing from M′ to M″, she is plainly being *pulled back*; and her velocity is therefore being diminished during that quarter

by the foregoing disturbing cause. Now, although not so evident or very easily made palpable, it is equally certain that, as she is passing from M″ to M‴ she is *accelerated* by the sun; and that during the quarter M‴ M⁗ she is again *retarded.* Summing up these facts, we reach the following result:—*About the region of full moon, when the earth's attractive power is diminished, the moon's velocity is increased by the disturbing power of the sun; and through the same energy her velocity is diminished at quadrature, or at those regions where at the same time the earth's attractive power is increased.* Observe now the necessary effect of these disturbances on the *shape* of the moon's orbit. The *curvature* or *bending* of any orbit evidently depends on the relation between the velocity of the moving body and the attraction of the central body. An increase of the relative force of the former, or, what is the same thing, an increase of the body's power to follow its natural rectilineal path through space, would necessarily *flatten* the orbit; while, on the contrary, a relative disturbance of the central energy would increase the curvature of the orbit. At M′ and M‴, then, the moon's orbit must be proportionally *flattened*, and while at M″ and M⁗ its curvature must be correspondingly increased; so that the orbit of our satellite must always be an oval, with its *flat side turned towards the sun.*—One step farther. Follow this effect, as the earth, along with the moon, passes through its annual course. At 1 in the diagram below, the flattened orbit of the moon must lie as

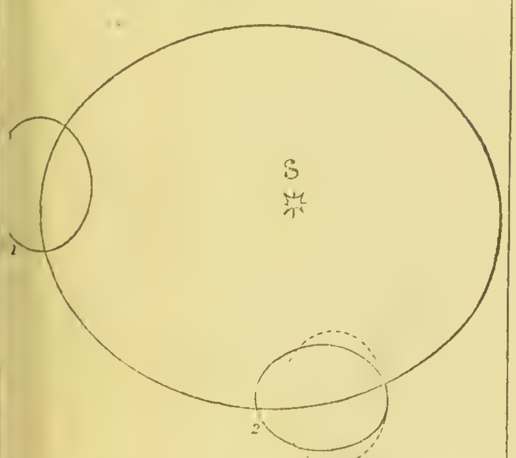


Fig. 2.

represented; and at 2, also as represented by the unbroken line; so that, as the conjoined bodies have moved from the position 1 to the position 2, the lunar orbit has changed from the *upright* position indicated by the dotted line, to the horizontal position of the continuous one. But it has not done this by *a start;* the change must have supervened gradually as the earth passed from 1 to 2: the orbit, in fact, must have been *twisting round* during that whole period, and the moon really describing, instead of a stable ellipse, the strange curve below!

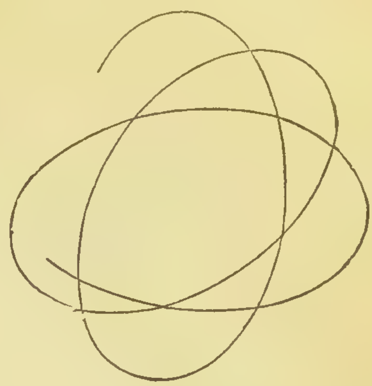


Fig. 3.

Not for one moment, however, even through these singular wanderings, is our luminary lost sight of by the eye of Science. Complex though her path may appear, it is as clear and determinate as her normal undisturbed ellipse. We can follow her through all those twistings for millions of centuries; and this was the power which revealed the imposture of the Brahmin, who, on the ground of a long *forged* list of eclipses, ventured to claim a fabulous antiquity for his race. —The modes of calculating the orbit of any celestial body from a given number of observations cannot be undertaken here. The classical work is the *Theoria Motus* by Gauss. The student is also referred to a most perspicuous treatise on the Orbits of Comets by Mr. Milne Holmes.

Organ. The invention of the organ is attributed to Archimedes, about 220 B.C.; but the fact does not rest on sufficient authority. Ammonius states that organs were used in the Western churches by Pope Vibatianus, in 658. It is affirmed that the organ was known in France in the time of Louis I., in 815, when one was constructed by an Italian priest. The organ at Haerlem is one of the largest in Europe; it has sixty stops, and 8,000 pipes. At Seville is one with 100 stops, and 5,300 pipes. The organ at Amsterdam has a set of pipes that imitate a chorus of human voices.

Orion. A constellation through which the equinoctial plane passes. It is one of the 48 old constellations. It has two stars of the first magnitude, *Rigel*, upon the middle of the left foot, and *Betelgueuse* upon the right shoulder of the figure. There are four of the second magnitude. One of them is on the left shoulder near *Rigel*, named *Bellatrix*, and the other three form what is called the Belt, lying nearly in a straight line, and at equal distances one from the other. The ancients regarded this constellation as one of terrible omen; his rising and setting being attended with severe tempests. It is in Orion—in his sword—that

we find the most remarkable and famous *Nebula* yet known.

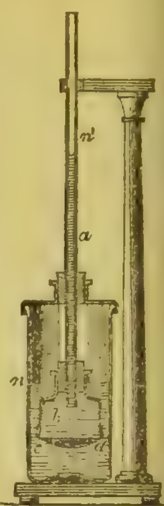
Orrery. An astronomical machine constructed to show conveniently the motions of the different planets. It is a model of the different planetary orbits; the arrangement being like what obtains in reality. The use of orreries for explaining popularly such phenomena as the seasons—the phases of the moon—the occultations of satellites and the eclipses of planets and satellites, will be very apparent. The best orreries are complicated and expensive.—The name was given to them instead of the original Planetarium, from the erroneous idea that the first of the kind had been constructed by Mr. Rowley, for the Earl of Orrery. An orrery was first constructed by Mr. George Graham, who had sent his model to Rowley, and Rowley copied it for his patron.

Orthogonal. See PROJECTION

Oscillations Small, Co-existence of.—Principle of Daniel Bernouilli. This remarkable and most important principle is as follows:—If any stable system be slightly forced out of its equilibrium, it returns to that state of rest through a number of small oscillations, and generally these are of many different sorts—the issue of various disturbing causes. Now, Bernouilli's principle is, that these small oscillations *co-exist*, and are simply superimposed—one oscillation in nowise interfering with, far less destroying another. To find the total or integral movement or oscillation of the body at any given time, we have therefore, merely to deduce each separate oscillation from its specific cause, and then to *compose* these various movements, or to find their resultant. The theorem was suggested to Bernouilli by the fact, that the differential equation which expresses such movements, is a *linear* one, and therefore belonging to that class of equations whose general integral is necessarily the *mere sum of special integrals.* But it would seem to have its roots deeper—to be a mere expression of the universal fact at the base of all rational mechanics,—that a motion impressed on a body in one direction never interferes with another motion impressed on the same body in another direction. See LAWS OF MOTION.—The property or theorem in question readily explains, or rather exhibits the simple root of a number of phenomena that have sometimes appeared puzzling. Such as the co-existence of waves on the surface of a liquid agitated at the same time at various points; the similarity of distinct sounds produced by various oscillations of the air; &c., &c.

Osmometer. See OSMOSE.

Osmose; Osmotic Force; Endosmose, Exosmose. The remarkable phenomena which form the subject of this article were discovered by Dutrochet. The *endosmometer* or *osmometer* renders these phenomena sensible and measurable, and is as follows:—A narrow tube *a* is fitted into a cylinder *b* of glass or metal—the lower end of which *c d* is covered by some animal membrane, or some porous solid: *n* is another large vessel of any substance into which the arrangement just described may be plunged. In illustration of the phenomena, suppose that the interior vessel *b* is filled with alcohol, and that the exterior *n* contains simple distilled water—the two liquids being separated by a membrane at *c d*:—further, let *n* be the level of water in the exterior reservoir, and *n′* the level of the alcohol in the tube *a*, as established mechanically or in virtue of the law of hydrostatic pressures. This state of things, however, does not continue permanent. In the course of a quarter of an hour after the arrangement shown in the woodcut has been completed, the level *n′* may have risen a full fifth of an inch; and if the tube *a* is only about a foot, or a foot and a-half in length, the liquid will in the course of a day reach its top and overflow. The intensity or amount of this change depends of course on the diameter of the tube *a*, the nature of the fluids completing the arrangement, and the nature of the membrane. There has, however, clearly been a flow of water from the exterior to the interior reservoir, through the membrane, in a way quite contrary to all hydrostatical laws—a flow which Dutrochet named an *Endosmose* of *water to the alcohol:* had the fluids been reversed, the opposite would have occurred—the water would have flowed from the interior reservoir, or, in Dutrochet's language, there would have been an *Exosmose* of *water towards the alcohol.* It is better, however, to dispense altogether with these terms, and to designate the phenomenon generally by the single name *osmose.*—What, then, is the cause of *osmose*—what this *Osmotic Force?* In the first place, it must be carefully distinguished from simple DIFFUSION (see that article in text and in Appendix). If a saline solution were in the interior reservoir, there would be a movement of *diffusion.* Liquid salt particles would ooze through the membrane, and a certain quantity of water would flow back and take their place. But in no case could the water flowing back be above five or six times the weight of the salt escaping were the phenomenon one of *diffusion;* whereas the water entering the osmometer often exceeds the salt leaving it, at least one hundred times. Besides, diffusion is always a double movement—osmose by itself is a single movement, one flowing inwards or outwards, never both ways. Neither can osmose be attributed to capillarity. The great inequality of ascension assumed among aqueous fluids is found not to exist when their capillarity is correctly observed: and many of the saline solutions which give rise to the greatest osmose



are undistinguishable in capillary ascension from pure water itself. The subject, although still obscure, has recently received much and valuable elucidation from Dr. Thomas Graham. This distinguished inquirer devoted the Bakerian Lecture of 1854, expressly to investigations respecting the Osmotic Force. He had performed a vast number of experiments, using all descriptions of solutions, and employing in the case of each, first, porous mineral septa, and then animal membranes. His general conclusion is that the *vis motrix* is chemical action. Solutions having no chemical action on the mineral septa, gave no osmose; and the intensity of the osmose in other cases bore a distinct relation to the chemical action evolved. It was very much the same when the internal reservoir was closed by membrane. The membrane is constantly undergoing decomposition, and its osmotic action is exhaustible. Dr. Graham's main conclusion seems irresistible; so the phenomena of osmose are simply the direct substitution, for one of the great forces of nature, of its equivalent in another form—the conversion of chemical affinity into mechanical effect. Nor does this theory interfere with the prevalent idea that the osmotic force is the hidden cause of the ascent of sap in trees, and many analogous physiological actions. All parts of living structures are in a state of incessant change, of decomposition and renewal. Now what more is wanted in the theory of animal functions than a mechanism for obtaining motive power from the decomposition of the tissues? It is thus even probable that the osmotic injection of fluids is the link intervening between muscular movement and chemical decomposition.

Ozone. A very puzzling aerial substance recently discovered. Into the question of its chemical relations we do not at present inquire. In the opinion of Faraday, oxygen is an allotropic substance—that is, a substance capable, like carbon, of appearing in various physical states; and he considers ozone one of its forms. Dr. Baumert, along with others, on the other hand, while allowing that oxygen is allotropic, maintains that true ozone is a super-oxide of hydrogen. This gas or substance is prepared in various ways. If the electric spark be passed through oxygen, ozone is generated; and it is to it that the peculiar odour is owing, so well known to experimental Electricians. Ozone differs from oxygen in many of its relations; it has a smell as we have just said; it bleaches; it corrodes silver leaf; it tinges a mixture of hydriodic acid and starch blue. Oxygen in its ordinary state has none of these effects. But the question regarding it, of the greatest general interest, is a physiological one,—is it true, as suspected, that its absence from the atmosphere, gives rise, or at least free course to epidemics? The important bearing of such an inquiry on the laws of health, renders it next to imperative that an OZONOMETER be made part of the apparatus of every physical observatory. The instrument is described in next article.

Ozonometer. This instrument must always depend on the fact that ozone, or oxygen in this form if it be so, has much stronger affinities than oxygen in the ordinary form; and that through these affinities we may detect its existence, and approximate to its quantity. The ozonometer therefore, consists of slips of test paper, suitably suspended, which are coloured by ozone. The first is Schönbein's. This test paper is prepared with iodide of potassium and starch. When suspended in the free air, but screened from the direct rays of the sun, the paper becomes brown,—the ozone combining with the potassium and setting free the iodine, which immediately forms an iodide of starch. The intensity of the colouring, measures the quantity of ozone present: Schönbein's scale has ten gradations. An instrument much more delicate, and which certainly ought to be preferred, has been proposed by Dr. Moffat of Howarden, and is already extensively employed. Dr. Moffat requires that his test paper be suspended in a box so perforated as to admit a free passage of air, but not of light. When thus exposed the test paper becomes very sensitive, and if kept in the dark, it will retain its brown tints for two or three years. The paper should be had from Negretti and Zambra, and Dr. Moffat himself has published the requisite directions. An elaborate comparison of the two ozonometers, by Dr. Barker of Bedford, leaves no doubt as to which is preferable; and Dr. Barker's decision is confirmed by the solid authority of Mr. Glaisher.

P

Pallas. One of the ASTEROIDS (*q. v.*)

Pantograph. An instrument by aid of which a plan or figure of given dimensions may be easily reduced to a similar one of any other given dimensions. Its principle is that of *proportional compasses*. A new and exceedingly convenient form was given to this class of instruments by the late Professor Wallace of Edinburgh: his invention was named the EIDOGRAPH. It, of course, depends essentially on the same principle, but that principle is most ingeniously wrought out.

Papin's Digester. See DIGESTER.

Parabola. One of the conic sections. Its equation is $y^2 = p\,x$. If one vertex and focus of an ellipse be given while the major axis increases without limit, the curve will become a parabola.

Parallax. The apparent positions of the heavenly bodies will clearly depend, to some ex-

tent, on the place on our globe's surface from which they are viewed, and a correction is therefore necessary to reduce these apparent positions to what they would be, if seen from the centre of the earth. This correction is termed *parallax*, and is quite analogous to the process in geodesy technically known as "*The reduction to the centre of the Station.*" Parallax changes the apparent position of every body, in the plane passing through the observer's zenith, *depressing the body's Altitude:* it has no effect on the body in *Azimuth.* For any given body whose distance from the Earth is constant, it varies as *the sine of the zenith distance.* A simple geometrical construction will show that it is the angle subtended by the radius of the Earth, when that radius is seen from the star whose parallax is required. The *horizontal parallax* is the greatest angle subtended by the radius—or the parallax of the body when in the horizon. For the Moon at its mean distance, this quantity is 57′ 6″; for the Sun again it is only 8″·57; for Uranus it never reaches half a second. With regard to nearer bodies—as the Moon—their distance from the earth may be deduced by means of observed parallax: in case of the remote planets, the parallax on the other hand must be deduced from their distances. A peculiar application to the case of the Sun is explained under VENUS, TRANSIT OF. The fixed stars have evidently no appreciable parallax of the kind now described; —the radius of the Earth as seen from their enormous remoteness shrinking into a mere point. But it has recently been found that the diameter of *the Earth's orbit* does subtend appreciable angles from these bodies; and from this datum the distances of many of them have been computed. See STARS, DISTANCES OF.

Parallax Binocular. The general term parallax signifies the apparent displacement of an object as seen from two different stations. For example, in fig. 1, the object O, as observed from S, is seen in the direction S O, whereas if the observer move to S′, then O is seen in the direction S′ O′, and the difference between those two directions, viz., the angle S O S′, is the parallax for these positions. It is evident that the magnitude of this angle, or the amount of parallax, depends closely on the distance from S to S′, and also on the distance from these two stations to the object O. Geometry traces out the connection between these three quantities, viz., the distance between the two stations, the distance from the two stations to the object, and the parallax, in such a manner, that any two being known, the third may be calculated. It is in this way that the distance of inaccessible objects is made out. The distance between two stations on the earth, or between two points in the earth's orbit, being measured, and the parallax observed, it becomes possible to compute the distances of many of the heavenly bodies, and thence their magnitudes, and the speed of their motions,—calculations which occupy an important place in the science of astronomy. In the case of distant objects, the base or distance between the two stations of the observer must be considerable, to render the parallax sensible; but, for near objects, a very small change of station will cause an obvious displacement of apparent position. Even slight attention will point out to any one that his two eyes do not see a moderately near object in the same relative position with regard to the points of the background on which it is seen. Thus, in fig. 1, if S represent one eye, and S′ the other, the distance from S to S′ being supposed to be about 2½ inches, and O any object at a distance of a few feet from the eyes, then on noticing the point of a wall, for instance, against which the point O is seen projected with the eye S, while the eye S′ is shut, and if a mark be made at O, and again while S is shut, the same observation be made, without moving the head, by means of the eye S′, and the corresponding place O′ be marked, it will be found that O is a different point from O′. The distance from O to O′ will depend on the distance of the wall from O, but the parallax, or the angular displacement S O S′, has no connection with this, but is the same, however far the screen may be removed, as long as the distance between the two eyes remains the same, and the distance of the object from the eyes does not change.—Binocular, or Two-eyed Parallax, then, may be defined to be the angular difference of position of an object, as seen by the two eyes of an observer, the head being kept at rest. As any object, when seen by the two eyes, occupies two different positions, or is seen in two different directions, a question comes to be raised, how it is that two different impressions of the object are not presented to the mind, one by each eye? or, in other words, why objects are not ordinarily seen double when looked at with both eyes? This question has been long the subject of thought to philosophy, and even now, it must be confessed, that it is far from being satisfactorily set at rest. Recently it has risen into even greater importance from its connection with the theory of stereoscopic vision, and with the best practical modes of obtaining binocular pictures by means of the photographic camera obscura. In order to account for the fact, that when the two eyes are directed toward an object, only one impression of it is presented to the mind, though we know that a distinct picture of it is formed on the retina of each eye, and though, in the case of near objects, the rays fall in different directions on the two eyes, two theories have been proposed, and at present divide the scientific world. The first is the theory of *corresponding points*, which supposes that each point in the retina, or sen-

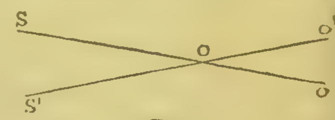


Fig. 1.

sitive nervous curtain of one eye, has a corresponding point in the other retina, which gives exactly the same impression of position or direction; so that from whatever quarter an impression on these points is produced, the sensations will appear to come from the same direction in both, or they will be seen as one point. For example, in fig. 2, if A A′ be two corresponding points, and O′ O″ be two other such points, while O is an external object, from which rays are entering the eyes, as represented by the lines O A, and O O″, then the object O would be seen double, because

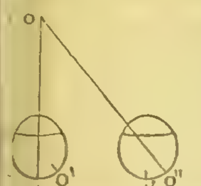


Fig. 2.

he eyes are so turned that the rays from O lo not fall on corresponding points. In order hat there should be only one impression, it is vidently necessary that the one eye should be urned round in such a manner that the line of ight O O″ should fall on A′, or that the other ye should be so turned that the ray O A should erminate on O′. Every one is familiar with the louble vision which results from a direction of he eyes similar to that shown in the figure. The traight line passing through the centre of the upil, and the centre of the eyeball, is called the ptic axis. It terminates on the foramen centrale (see EYE) of the retina, which is the only oint where the vision is perfectly clear and distinct. Now, it is certain, that, in the case of hese two points at least, the theory of corresponding points holds, for it is impossible by *ny means* to get two impressions on the points f distinct vision, which will yet appear to have lifferent directions or positions.—Before going urther, it will be necessary to state the second heory of single vision, with two eyes, which vas before alluded to. It is supported by the igh authority of Sir David Brewster, and is as llows:—"The impression produced on any art of the retina, always appears to come from direction perpendicular to the surface at that oint, whatever may have been the real direcon of the impressing rays of light which proiced it. This is called the law of visible rection. An object, then, is seen by each eye a different direction, and seems to the mind be placed where these two directions meet. Thus, in fig. 3, where the two optic axes are turned towards the object O, the lines of impression from A and A′, perpendicular to the points of the surface, pass again through O, and the object is seen in each of the directions A O and A′ O, and of course, in the same place or point, O. This seems in such a case plausible enough,

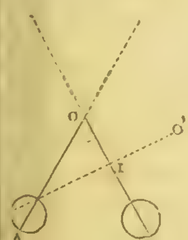


Fig. 3.

t if it happens always that an object appears in the position when the two lines of its visible directions cross each other, and if there are no such things as corresponding points, why does it not occur that any other object, such as O′ is seen in the same position as O, when their visible directions L O′ and A′ O cross each other at I? By similar reasoning it is easy to perceive that multitudes of different objects ought to appear to occupy the same position, or to be seen in the same direction by the two eyes, though, in reality, they occupy very different relative positions in external space; and, in fact, that confusion must result if there were nothing else necessary, in order to identity of position, but the crossing of the two lines of visible direction. The theory of corresponding points asserts, that from whatever direction impulses arrive at corresponding points on the two retinæ, the impression from the two will appear to be in one direction, or the vision of them will be single. For example, the two points of distinct vision are corresponding points, and they always give the same direction of vision, however varied may be the real directions of the normals to the surfaces or the axes of the eyes, or however different may be the real directions of the rays which have reached them, to produce the impressions. In the ordinary adjustment of the eyes for the distinct vision of any point, such as O, in fig. 4, the two eyeballs would be so turned, that the optic axis converged on O, and the two rays O A and O L, fall on the two points of distinct vision A and A′, so that but a single impression would be produced. But if by pressure on one of the eyes, or by squinting, or other

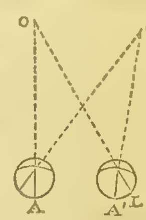


Fig. 4.

cause, the two axes are not brought to bear on O, but one of them is directed to another object O′, then, although it is evident that A O and A′ O′ never meet, still the object O′ will be seen on the point of distinct vision A′, and will seem to occupy exactly the same position or visible direction as O, which certainly could not be the case if the theory of crossing directions were true; while it is only what might be expected from the theory of corresponding points. It may be stated as a fact, all-important in such a question, that it is impossible by any amount of divergence of the optic axis to see two points *distinctly* which do not seem to occupy the same position, or to be in the same direction. Another argument in favour of the theory of corresponding points is derivable from the phenomena of "Ocular Spectres." See PERSISTENCE. It is as follows:—If the eyes be steadily directed towards any point near a bright object, such as a gas flame, or the solar disc, a persistent impression will be produced which can for a considerable time be rendered visible by rapidly closing and opening the eye-

lids, while the eyes are directed towards any flat surface, such as the ceiling or wall of a room. In such a case, there is an impression on the retina of each eye, and, it might be supposed that, by pressure on the eyeball, or by squinting, and thus altering the lines of visible direction, those two impressions could be separated from each other; yet it is not so, they continue to give but the impression of a single flame, a fact that is greatly more consistent with the theory of corresponding points than with that of crossing directions; as, by pressure, it is easy to cause the lines of direction to cease to cross, when, of course, the impressions ought to separate, which they do not. Again, on the theory of crossing directions, it is difficult to see how even in one eye, there should not be inextricable confusion of images, as all directions cross each other in the centre of the eyeball; and further, according to the same theory, it is difficult to see how in any case we can have double vision of any object by means of the two eyes, as the lines of visible direction are certain for objects in the same plane, to cross somewhere. For example, in fig. 5, where the two optic

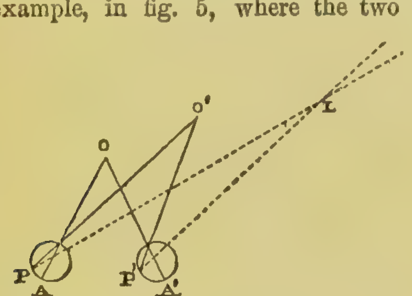


Fig. 5.

axes are directed towards the point O, there will be single vision of O by means of the two eyes, as the optic axes will also be the lines of visible direction or the perpendiculars to the surface of the retina at the points of impression. But in the case of some other point, O′, the incident rays O′ P and O P′ strike the retinas in their points P and P′. The perpendiculars to those points, or the lines of visible direction, P L and P′ L ought not to give separate images though they do not fall on similar points of the retinas, for they must cross somewhere, as in L, where the single image ought to be. But, say the advocates of this theory, the lines of visible direction must, to give single vision, cross in the position of the object itself. Now, however unfounded such an assertion may appear, it can furthermore be directly shown to be untrue, and that single vision may occur though the lines of visible direction do not cross each other in the actual position of the objects themselves, which appear to occupy the same line of direction. If one eye be pressed aside so as to cause apparent displacement of any visible object, it will be noticed that the displaced object, as seen by the eye on which pressure is exercised, appears to overlap, or to be identical in position with some other object as seen by the other eye, and not only so, but that the whole lines of objects in the directions passing from either eye through these points, seem to occupy the same position, or the directions seem to become one, which the slightest thought will show, could not be the case if the appearance of unity were only produced when the lines of visible direction crossed each other in the actual position occupied by the object itself. It seems on the whole, then, to be a fact in nature, that the two retinas are so constituted, that position on its surface determines direction, and that the two act together by means of corresponding points, in only giving, so to speak, one impression of surrounding space, and not two, made single by crossing lines. With regard to the uses derivable from the two eyes being placed so far apart as to give the Binocular Parallax which has been described, it may be stated, that, to a considerable degree, by means of it, judgment is formed of distance. Any one may convince himself of this by the well known experiment of holding up a ring at some distance in front of one eye, the other being closed; and while the edge of the ring only is seen, endeavouring to pass the finger through it. There is no doubt but that the sensational perception of the amount of convergence given to the two optic axes, in order so to arrange them that a distinct single view of any object be obtained by means of both eyes, is a mode by which distance is judged of. This is as much a consequence of one of the theories discussed above, as of the other; and there is also no doubt but that the double images of all objects which are not at the same distance as the one to which the optic axes are directly converged, are a chief means of pointing out their difference of distance. The exceptions to this doubling are pointed out under the head HOROPTER, and the further elucidation of the subject will fall more properly under STEREOSCOPE.

Parallel Lines. The characteristic of two lines in the same plane, to which the name of parallel is given in geometry, is simply this, that although produced ever so far either way they will never meet. The theory of these lines continues a stain on our Elementary Science. It is easy to prove that if certain conditions are fulfilled when two lines cut a transverse line, these two will never meet, or must be parallel; but to establish the converse—to prove, viz.: that if the two lines are parallel, these same conditions are lawfully predicable, has hitherto defied the logic of all geometers. A fact, certainly most remarkable in this purely deductive science; nevertheless, its causes do not appear remote. The existence of such a defect, unquestionably argues some oversight in the list of geometric axioms,—the oversight of the nature of some of our primal perceptions regarding magnitude; but it does not follow that the missing axio

has immediate relation to parallels, or that it ought to enable us to resolve *directly* the specific proposition at which the acknowledged difficulty first appears. On the very contrary, it may be asserted with abundant confidence, that nothing but failure could attend the effort—originated by Euclid, and since his time, all but universally followed—to supply the deficiency by new postulates or axioms regarding *parallel lines.* To prove, that under certain conditions, two lines will never meet, or what is the same thing, that no triangle can be formed in such circumstances, involves no conception with which we cannot readily cope; but to deduce the properties of two lines postulated as parallel, involves a direct dealing with the *positive idea of infinity*—a task utterly beyond reach of our faculties. Whether we have a positive idea of the infinite, is a question concerning which the profoundest metaphysicians have differed and continue to differ; but consideration of the origin and formation of language, suffices of itself to leave no doubt of the fact that we cannot *speak* of infinity otherwise than as a *negation;* and, therefore, that no positive axiom can be laid down respecting it. The truth is, there is an unfortunate and illogical inversion in the first book of the Elements. The proposition required by Euclid, is the *thirty-second,* viz., *that the sum of the three angles of a triangle is equal to two right angles;* given that proposition, the difficulty about parallels disappears, inasmuch as their properties may be deduced from it by the negative process, although with a certain difficulty; but Euclid deduces his proposition from the subject of parallels, having first assumed their theory under guise of what he most unjustifiably terms an *axiom.* If we are correct, then, the question turns on this—can we logically establish the *thirty-second proposition* without appealing to the doctrine of parallels?—Assuredly there is no reason on the face of the subject that should cause geometers shrink from this attempt; but it is just as certain, that the apparent difficulty of succeeding in it—witnessed by innumerable failures—indicates a defect in the statement of the usual axioms, or fundamental propositions regarding our primary discernments concerning space. The defect is a very important one: were it supplied, a great change would pass over all arrangements and methods of development in our elementary Geometry. The defect is in Euclid's inadequate conception of the *necessarily distinctive* nature of two *definite* attributes of geometrical quantity—*form and magnitude.* The Greek geometer did not trace out the manner in which we acquire our notions of these attributes; and he did not therefore recognize it as an axiom, that the attribute of *Form* has no dependence on the attribute of *Magnitude.* The phenomena of Universal Belief indeed amply sustain the proposition—"*If any figure exists or is conceivable, it must exist or be conceivable with the same form, whatever its magnitude;*" or any other statement involving the Truth, that *in our Perception of the geometrical qualities of an object, Form alone is definite; Magnitude being indefinite.* The physical process of Perception reveals the root of that belief; the notion of Magnitude involving an estimate of the *distance* of the object, while the notion of Form is, at its source, independent of every variable quantity.—These views may be sustained by the high authority of Laplace. The following is a note attached to his *Systeme du Monde:*—"*Les tentatives des geometres, pour demontrer le* POSTULATUM *d'Euclide sur les parallèles out été jusqu'a présent inutiles. Cependant personne ne révoque en doute ce* POSTULATUM *et les theoremes qu' Euclide en a deduits. La perception de l'etendue renferme donc une propriete speciale evidente par elle meme, et sans laquelle on ne put rigoureusement etablir les proprieties des paralleles. L'idee d' une étendue limitée, par example du cercle, ne coutient rien qui depende de sa grandeur absolue. Mais si nous diminũons par la peuseé son rayon, nous sommes portes invinciblement a' diminuer dans le même raport sa circumferénce et les côtés de toutes les figures inscrites. Cette proportionalité me parait être un* POSTULATUM *bien plus naturel que celui d'Euclide.*"—It is remarkable that Legendre's effort to demonstrate the thirty-second proposition by aid of a functional equation, is nothing else at its root, than an implicit statement of the very axiom we contend for. Sir John Leslie's objections to the process of Legendre were puerile; the Edinburgh Geometer wholly forgot that *constants* might enter into the equation; and that while there are *constants* among *angles,* there are none among *lines.* But these objections are not a whit more puerile than the subsequent defences of the Frenchman's process by Baron Maurice of Geneva.—The effort to remodel geometry by the lights now given, would assuredly not be an unworthy one.

Parallel Motion. A contrivance of Watt's for converting rectilinear into circular motion. The piston rod, whose motion was the source of moving power, went straight up and down, and it was attached to the beam, which, being fixed at its centre, described a circular arc. It was impossible, therefore, that this circular arc should be accurately described if the beam and piston rod had been directly connected. The contrivance through which they are connected indirectly, so as to convert the rectilinear into the circular movement, is called the parallel motion.

Paraslene. A mock moon. A meteor adjacent to the moon in the form of a luminous ring, whereby two or more images of the moon are sometimes seen. See HALOES, &c.

Parhelia, or Mock Suns. A phenomenon in which one or several mock suns appear beside the real one. The exact form which the mock sun assumes is not generally identical, except in breadth, with the real sun; and very frequently

there are coloured rings around the mock sun like haloes, sometimes with luminous tails. Frequently a number of them appear at once. Many exact descriptions of these appearances will be found in books of astronomy. No very accurate theory of their origin or cause has been assigned. It is supposed that the cause is the reflection of the sun upon clouds situate so as to receive it partially, which again throws it back upon others, until ultimately it is shown on the sides of clouds visible to us. See ANTHELIA.

Parthenope. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Paschal Cycle. See CYCLE.

Pavo. One of Bayer's constellations between Sagittarius and the South Pole.

Pencil. An optical term, applied to a number of rays which converge to, or diverge from, the same point. The same term is applied by analogy to a pencil of lines. *Harmonic Pencils* are referred to and their character explained under RATIO ANHARMONIC.

Pendulum. The pendulum is *simple* or *material: i.e.*, it may be conceived as a mathematical point having weight affixed to one end of a mathematical line, and constrained to oscillate or vibrate—the other extremity of the line being fixed; or it may be conceived as a rod or other form of material suspended by one of its ends, and made to vibrate. The latter is sometimes called the compound pendulum. Further, the pendulum may vibrate in vacuo, or within some resisting medium. These considerations explain the reason of the divisions and subdivisions of the following short article.

I. OF THE VIBRATIONS OF PENDULUMS IN VACUO.

(1.) *The Theory of the Simple Pendulum.*—The vibration of the simple pendulum may be confined within *one plane*, or the heavy point may describe some *comparatively complex curve.*

a. The former or simplest case amounts to this:—A heavy point is constrained to move to and fro within the concave circumference of a fixed circle, and on either side of a fixed point in that circumference,—required the laws of such motion? The following is the simplest most general investigation.

If N be the normal force which the curve exerts upon the point, and λ, μ, ν the angles which it makes with the axes, horizontal and vertical, we have these equations of motion—

$$\frac{d^2 x}{d t^2} = \text{N} \cos \lambda, \quad \frac{d^2 y}{d t^2} = \text{N} \cos \mu,$$

$$\frac{d^2 z}{d t^2} = -g + \text{N} \cos \nu.$$

Hence

$$\frac{2\, d^2 x}{d t^2} \cdot \frac{d x}{d t} + \frac{2\, d^2 y}{d t^2} \cdot \frac{d y}{d t} + \frac{2\, d^2 z}{d t^2} \frac{d z}{d t}$$

$$= 2\,\text{N}\left(\cos \lambda \cdot \frac{d x}{d t} + \cos \mu, \frac{\delta y}{d t} + \cos \nu \cdot \frac{\delta z}{d t}\right)$$

$$-2 g \frac{d z}{d t}$$

$$\text{or } \frac{d.}{d t}\left(\frac{d x^2 + d y^2 + \delta z^2}{d t^2}\right) = -2 g d z$$

$$d \,.\, v^2 = -2 g\, d z$$

$\therefore v^2 = -2 g z + \text{C}$. and if for $z = h$, $v = \text{K}$

$$v^2 - k^2 = 2 g (h - z). \qquad (1.)$$

This applies to the general case of a body moving upon a curve. Let the curve be a circle. Then $y = o$, $x^2 + z^2 - 2 a z = o$, if the axis of x be the tangent at the lowest point. Hence

$$\frac{d x^2 + d y^2 + \delta z^2}{d t^2} = v^2 = \frac{a^2}{2 z - z^2} \frac{d z}{d t^2}.$$

But $\qquad v^2 = k^2 + 2 g (h - z).$

therefore

$$d t = \frac{+ a\, d z}{\sqrt{2 a z - z^2} \sqrt{k^2 + 2 g h - 2 g z}} \qquad (2.)$$

This latter equation cannot generally be integrated.—From (1.), it is evident that v is a maximum for $z = o$, and is equal for equal heights on the two sides of z. Also its *minimum* value of v, corresponds to the maximum of z, that is $2 a$, if that value be a possible one, that is $k^2 + 2 g h - 4 g a$ be not negative, or if $\frac{k^2}{2 g} + h$ be $7\ 2 a$. If it be not so, the velocity becomes o, at $z = \frac{k^2 + 2 g h}{2 a}$ and the point turns there and re-begins its motion. If $\frac{k^2 + 2 g h}{z a} = 2 a$ it may be shown that the point will approach infinitely to the summit of the circle without over-reaching it.

Equation (2.), reduces to

$$d t = -\frac{1}{2} \sqrt{\frac{a}{g}} \cdot \frac{d z}{\sqrt{h z - z^2}} \times$$

$$\times \left(1 - \frac{z}{2 a}\right)^{-\frac{1}{2}}$$

which may be developed by the binomial

$$d t = -\frac{1}{2} \sqrt{\frac{a}{g}} \cdot \frac{d z}{\sqrt{h z - z^2}} \times$$

$$\times \left(1 + \frac{1}{2} \cdot \frac{z}{2 a} + \&\text{c.,}\right)$$

and this may be integrated term by term. Hence for the entire oscillation from $z = h$, $v = o$ back to that on the other side

$$\text{T} = \pi \sqrt{\frac{a}{g}} \left\{ 1 + \left(\frac{1}{2}\right)^2 \cdot \frac{h}{2 a} + \left(\frac{1\,.\,3}{2\,.\,4}\right)^2 \left(\frac{h}{2 a}\right)^2 + \&\text{c.} \right\}$$

If $\frac{h}{2a}$ be very small

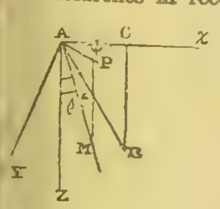
$$T = \pi \sqrt{\frac{a}{g}}, \text{ for a small oscillation.}$$

Neglecting all after the second term

$$T = \pi \sqrt{\frac{a}{g}} \left(1 + \frac{h}{8a} \right).$$

It is evident how the value of g may be discovered by actual observation from this, and also the length (a) of a simple pendulum to beat seconds. The following five theorems may be readily deduced from these formulæ:—1. *Whatever the amplitude of the arc of oscillation, provided that arc be small, the period of the oscillation will be invariable—other things remaining the same.* 2. *The lengths of two pendulums beating the same time—say seconds—are proportional to the accelerating force of gravity.* It is through this theorem that the pendulum has come to be employed as a means of determining the figure of the earth. 3. *The times of vibration are proportional to the square roots of the accelerating forces.* 4. *The periods of oscillation are proportional to the square roots of the length of the pendulums.* 5. *The squares of the number of oscillations made in the same time by two pendulums of the same length are as the accelerating forces.*

b. But the vibration of the pendulum may not be confined to one plane. Suppose the point of suspension to be the centre of a hollow sphere, it is manifest that the general problem may be expressed thus,—What are the conditions of the motions of a heavy point constrained to move on the concave surface of that sphere? A pendulum not confined to one plane, but simply to the concave surface of a hollow sphere, is termed the *conical pendulum*. We cannot offer more than the veriest outline of the theory of such motions. The student will find many admirable researches in recent numbers of the *Philosophical Magazine* and other scientific periodicals.



Take for origin A, for axis of Z the vertical downwards, and let X Z pass through the central position A B. Let M be the position at any epoch, of the point P, its projection on X Y; C that of B. Let B A Z = M A Z = θ, P A X = ψ, A M = a, A P = r, and call the initial velocity K. Decompose now the initial condition into the two following:—Suppose, in the first, the material point placed at B, and without velocity; and, in the second, suppose it in Z, and having the initial given velocity The composition of these two mutually independent displacements at any epoch, will show us the displacement of all which is wanted. If, in the first, ϕ be the variable angle which the pendulum forms with A Z, we shall have this approximate result.

$$\phi = a \cos t \sqrt{\frac{g}{l}}$$

If, in the second, ω be the angle formed by the pendulum with A Z, and if the direction of the axis of y's has been taken so that the initial velocity K carries the pendulum in the angle Z A Y, corresponding to positive values of ω, we shall have

$$\omega = \frac{K}{\sqrt{g\,l}} \sin t \cdot \sqrt{\frac{g}{l}}$$

These two equations give the position of the point at every instant, and therefore determine all the laws of its motion.

(2.) *The Theory of the Material or the Compound Pendulum.*—If instead of a heavy mathematical point and a mathematical line of suspension, we require to treat the case of a vibrating material rod, it is clear that new considerations must arise. But these are readily evaded, through effect of the theory of the CENTRE OF OSCILLATION (*q v.*). By aid of the doctrine of the Centre of Oscillation, the case of a material or compound pendulum may be at once reduced to a corresponding case of the simple pendulum.

II. THE VIBRATIONS OF PENDULUMS WITHIN RESISTING MEDIA.—It is well known that a body plunged into any fluid loses part of its weight,—as much, viz., as the volume of the fluid displaced by it weighs. Hence, a pendulum vibrating in the air will execute fewer vibrations in the same time than if it oscillated *in vacuo*. This effect, or the *co-efficient of buoyancy*, will of course be constant for the same medium. No doubt could exist as to the certainty of the foregoing result; but Bouquer was the first to announce the following general theorem, viz., *whatever the resistance of the medium the oscillations of a pendulum within it are isochronous:* the resistance merely diminishing the amplitude of the vibrations, and finally destroying or stopping them altogether. Borda proposed to demonstrate this truth; but it is to Poisson that we owe a mathematical analysis of the subject so complete, that, considered from his point of view, nothing further could be desired. The subject, however, was recently taken up by Mr. Baily, and examined experimentally with all the care, conscientiousness, and precision which this excellent physicist knew so well to bestow on delicate inquiries. And it did not appear that the theoretical results adequately agreed with the results of his observation. According to Poisson, the factor by which the correction for buoyancy must be multiplied to give the whole effect observed, is 1·5; Mr. Baily's experiments, on the other hand, gave an average factor of 1·8. The discrepancy has since been removed by Professor Stokes, who has reviewed

the entire subject, and taken especial account of *the internal friction of the fluid or medium.* This memoir by Mr. Stokes, is inserted among the transactions of the *Cambridge Philosophical Society;* and is in many ways very remarkable. Limiting his researches to the case of a ball pendulum, he found that the resistance encountered is proportional, not to the surface, but to the radius of the sphere; and, therefore, that the quotient of the resistance divided by the mass increases very rapidly as the radius decreases. Accordingly, the terminal velocity of a minute globule of water descending through the air, depends almost wholly on the internal friction of air: but since the Index of Friction is known from Baily's experiments, this terminal velocity can be calculated numerically for a globule of given diameter. This velocity proves to be so small in the case of globules, such as those of which the clouds are probably formed, that Mr. Stokes states, *the suspension of clouds* need not offer any difficulty. He has further applied this theory of internal friction to the calculation of the subsidence of a series of any oscillatory waves.

III. Applications of the Pendulum.—The applications of the pendulum in scientific research are manifold and replete with highest interest. If its *invariability* as to length is secured, it is the best measurer of *Time*, as well as of the variation of Gravity at different parts of the earth. The modes of securing that invariability now in use, have been already fully explained under Chronometer.—But it is capable of important applications of another kind, where no compensation has been employed, and no effort made to secure *invariability*. Suppose, for instance, it is a mere metallic rod, the rate of whose expansibility by heat has been rigorously determined. The clock regulated by such a pendulum, will not keep accurate time: it will go quick in cold weather, and slow in hot weather; and these seeming irregularities will exactly follow the changes of temperature. If a clock of this kind has, in a certain period of time, lost or gained a definite quantity, that quantity will probably be our best and easiest indication of the *mean temperature* during the period:—it will plainly be the *exact integral of all the momentary effects.*—So, likewise, as has been several times proposed, a clock may be constructed to present for any given time the mean pressure of the air, or the exact integral of all momentary changes of this paramount meteorological element. It simply requires that the pendulum rod consist of a *syphon barometer*. The rise and fall of the mercury in this barometer would evidently influence at every moment the position of the centre of oscillation, and thereby alter the clock's rate. See a full and conclusive investigation of the nature of such an instrument by Professor Rankine, in *Philosophical Magazine* for December, 1853.

Penumbra. The meaning of the astronomical terms *penumbra* and *umbra* has been explained at sufficient length in the article Eclipse, to which we refer. We merely wish here to notice the very remarkable phenomena visible in the case of a lunar eclipse, when the moon passes through the umbra and the penumbra. We have seen that the solar light, by the reflection of which the moon shines, is prevented from reaching her by the interposing body of the earth. We must consider the earth, however, as surrounded with a refracting atmosphere, and faintly illuminated at all parts by the sunlight which it absorbs in the day and gives off in the dark. The effect of the first fact will evidntly be to throw the visible ray that bounds the umbra, nearer the moon. The upper and lower common tangents (see figure for eclipse) do not proceed in straight lines, but are first refracted on entering the atmosphere, and then again on leaving it. The result is that these rays are bent inwards, and a lessening of the cone of the umbra is the result. The other pair of common tangents, which are the limits of the penumbral cone, are also bent inward, lessening its bulk. Here, also, the result is simply a diminution of the penumbral cone. If the air, then, were a homogeneous refracting mass, the effect, in both cases, would merely be such diminution. But the parallel rays that stream through the upper and lower regions of the atmosphere are not equally refracted. In the former they meet with enormously thin air, which scarcely refracts them at all; in the latter, they are powerfully refracted. Hence these refracted rays are spread in unequal amounts over the space which is thus cut off from the umbral and penumbral cones. At the outside, where we are near the borders of the geometrical umbra and penumbra, there is most additional light due to refraction, in the inside, near the inner borders of these cones, this refracted light darkens and disappears. In the lower strata of our atmosphere there are always hanging however, quantities of watery vapour, whether visible in mists and clouds, or not visible. This transmits but very few of the rays of the sun, and of these the red rays almost alone. Hence at this point of its passage the moon is likely to assume, and in fact does assume a deep blood-red tint, as we sometimes see the sun himself struggling through thick mist in autumn mornings. Then again, all the layers of the atmosphere act as prismatic dispersers of the white ray. They do not refract equally the different coloured rays. There is an unequal dispersion of light therefore, over the whole space between the geometrical and the true umbra and penumbra, and the general tint is a faint bluish-green. According to these remarks, it will be readily perceived that the state of the weather (the meteorological condition of the atmosphere) will exercise very decided effects on the phenomena of a lunar eclipse. When clouds are floating high up in broken masses, there will be a dispersion of irregular particles of red light over the inner half of the space just described. If it be

quite clear so far as we see, there will be a slight muddy tint, as the moon passes along the edge of the umbra or penumbra; and the moon in its whole passage through the intervening space will appear of a dim uncertain bluish tint. While the moon is passing through the penumbra, or this space between the geometrical and the true umbra—these refracted solar rays, or the partial solar illumination preceding them—enable us to see her orb. When she passes completely into the umbra, she appears at first dark, and remains so to the naked eye for some time. All this time a telescope can distinguish her mass, faintly illuminated by light from the earth; as she passes in, the eye, getting accustomed to the loss of the more powerful reflection of the solar rays, becomes much more sensitive than it was to feeble impressions, and even unassisted by the telescope, it can discern the moon passing through all delicate gradations of colours up to a fiery copper glow, like that of dull red hot iron, which has been left to cool. The side of the moon, turned from the sun at the time, has absorbed at previous periods an amount of light which it now throws upon every body capable of receiving its impressions, and to which the eye becomes more and more sensitive, as the common moonlight fades. These phenomena are all observable in a lunar eclipse.

Percussion See CENTRE OF.

Perigee. The point of the moon's orbit nearest the earth.

Perihelion. The point of the earth's, or of a planet's orbit nearest the sun.

Perioeci. Those inhabitants of the earth who live under the same latitudes, but at 180° of longitude distant from one another—that is, at diametrically opposite points of the same parallel of latitude. They have the same common seasons, because they are in the same relation to the ecliptic, and having the pole in the same position they have identical celestial phenomena. When, however, it is noon with the one, it is midnight with the other, and *vice versa*.

Periscii. The inhabitants of either frigid zone, where in the summer the sun moves simply round about them without setting, and where the shadow consequently turns round successively to all points of the compass.

Periscopic Spectacles. Generally speaking, distinct vision through spectacles does not take place unless one looks through the axis of the lens. Dr. Wollaston suggested, that, by the use of *menisci*, or concavo-convex lenses, objects might be looked at *obliquely*, without being much distorted, so that wearers of spectacles might turn the eye, without turning the head, as in cases of natural unassisted vision. These are the *periscopic spectacles*.

Permutations. The idea of combination is one of the earliest we have. Suppose that we have a number of articles, and that it is proposed to arrange them in all the forms of a given kind that are possible, the question is evidently suggested, how many of such forms are possible? This may be answered by actually exhausting all of them. If, however, some algebraic formula could be obtained to give the result of this exhaustion for every case, the general problem would be solved.—Suppose we have m things, which call a, b, c, d——l, and that we are required to find out how many combinations of these, two and two together, are possible.—It is evident that any one of the things a can be put once before any of the others b, c, d, &c. This will give $m - 1$ combinations in which a is first. Again, any other of the things b can be put before each of the others a, c, d, &c., and every member of this class will not only differ from each of its own class, but from every member of the former. Thus repeating the process for each of the m quantities, we shall have m classes of combinations, each containing $m - 1$ forms; that is $m \cdot (m - 1)$ combinations. Again, if we are to have all the forms obtainable by taking the quantities, three and three, together. Count off one of them, as a. Then the others, which are $m - 1$ in number, can be arranged two and two together, by the proof above $(m - 1)(m - 1 - 1)$; that is $(m - 1)(m - 2)$ times. Suppose we put a before each of these. Then we shall have $(m - 1)(m - 2)$ forms of arrangement of the original quantities by threes, in all of which a is first. Similarly counting off every one of the m quantities successively, and repeating the identical process here gone through we should have m series of arrangements, in each of which are $(m - 1)(m - 2)$ individual arrangements, all differing, classes and individuals. These are, therefore, $m(m - 1)(m - 2)$ of these arrangements. See, then, if we can guess at the law which seems to prevail. A law which evidently does hold for such combinations of m (any number) of things taken two together, and also taken three together, is that

$$m \, . \, (m - 1)(m - 2) \ldots\ldots (m - n + 1)$$

is the number, if the things be taken n together,—where each of the successive factors decreases by 1, until the last, $m - n + 1$ is reached. We have reached the law by induction then. But we can show by mathematical proof, that there is such a law; that if it do actually hold for any given number n, it will actually hold for the number immediately above it $n + 1$. For suppose one of the m things, a, to be counted off, then by the law supposed to hold for n things, we shall have all the combinations of the remaining $m - 1$ things taken n together as

$$(m - 1)(m - 2) \ldots\ldots (m - 1 - n + 1)$$

which is equal to

$$(m - 1)(m - 2) \ldots\ldots (m - (n + 1) + 1)$$

If to each of these a be prefixed, we shall have

$$(m - 1)(m - 2) \ldots\ldots (m - (n + 1) + 1)$$

combinations of m things, taken $n + 1$ together in which a is first. Similarly we should have a

like number of them in which b is first—in which c, d, &c., are so. These are, therefore, m classes —as many, that is, as the number of things—each consisting of

$$(m-1)(m-2)\ldots\ldots(m-(n+1)+1)$$

forms of combination. Hence there will be altogether

$$m(m-1)(m-2)\ldots\ldots(m-(n+1)+1)$$

forms of combination of m things, taken $n+1$ together, if the law above stated hold for them taken n together. That is, the same law (for this is identical with the old), if it hold for any one number n, will hold for that immediately above it. But it holds for the number 3, therefore it will for 4. But it holds for 4, therefore for 5. But for 5, therefore for 6, and so on evidently *ad infinitum*. That is, the law holds for any number of forms of combinations. Therefore, the number of forms of combination of m things, taken n together is

$$m(m-1)(m-2)\ldots\ldots(m-n+1)\ldots\ldots(1).$$

In this discussion, as will be seen on looking back at it, such a form as $b\,a$ has been taken as different from $a\,b$; $b\,a\,c$ as different from $c\,b\,a$, and so on. But it might well happen, that the *order* of events occurring might be immaterial to the calculator. He would then count $b\,a$ as equivalent to $a\,b$, $b\,a\,c$ to $c\,b\,a$, and so on. How shall we obtain the number of such combinations *independent of order?* Suppose, for example, we have m things, taken n together, and we wish to find only the number of such fundamentally different combinations. Take any one of them, consisting of n definite quantities. Then instead of the total number, in which these same n quantities suffer all different kinds of arrangement, we shall have only one. Can we find the value of this whole number? That is, can we find the number of combinations *dependent on order* of n things, taken n together? The reply is evident. Substitute in formula (1) n for m. We have then

$$n\cdot(n-1)(n-2)\ldots\ldots(n-n+1)$$

or $\quad n(n-1)(n-2)\ldots\ldots 1,$

or $\quad 1\cdot 2\cdot 3\cdot 4\ldots\ldots n\ (2),$

as the number of such combinations. Instead, therefore, of the $1\cdot 2\cdot 3\cdot 4\ldots\ldots n$ combinations independent of order into which any n definite things can be arranged, we shall have only one. We should have the same result for any other n definite things. We thus arrange the whole number into groups, each consisting of $1\cdot 2\cdot 3\cdot 4\ldots\ldots n$ individuals, and count the number of groups. That number is evidently, therefore,

$$\frac{m\cdot(m-1)(m-2)\ldots\ldots(m-n+1)}{1\cdot 2\cdot 3\ \ldots\ldots\ldots\ldots\ldots\ n}\quad(3).$$

The formula (3) can be put into a different form, that will give clear confirmation of its truth. Let its numerator and denominator each be multiplied by the number

$$(m-n)(m-n-1)\ldots\ldots 3\cdot 2\cdot 1,$$

and we obtain the equivalent form

$$\frac{m\cdot(m-1)\ldots\ldots(m-n+1)}{1\cdot 2\ldots\ldots\ldots\ldots\ldots\ldots n}\cdot\frac{(m-n)(m-n-1)\ldots\ldots 3\cdot 2\cdot 1}{1\quad 2\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ m-n}.$$

Now the numerator is evidently—changing the order of factors—the quantity

$$1\cdot 2\quad 3\ldots\ldots\ldots\ldots\ldots m.$$

Hence we have, equivalent to (3), the form (4)

$$\frac{1\cdot 2\cdot 3\ldots\ldots\ldots m}{1\cdot 2\cdot 3\ldots\ldots n\cdot 1\cdot 2\cdot 3\ \ldots\ldots m-n}\quad(4).$$

Let us consider what the meanings of the several parts of this formula, would indicate as its total meaning. Suppose m things arranged m together in any way, and divided into two classes, say an accepted set, consisting of n, and rejected set of $m-n$ individuals always. Then the number of combinations m together, regardless of order, is

$$1\cdot 2\cdot 3\ldots\ldots\ldots m.$$

Now, of these, if we take the number of combinations of n things n together, regardless of form, and of $m-n$ things $m-n$ together, regardless of form, we shall evidently have by combining the two sets the whole number of combinations, which we must in the actual case (m things n together, taking order into account), consider as for our purposes only one group. That is, we have, for every

$$1\cdot 2\cdot 3\ldots\ldots n\times 1\cdot 2\cdot 3\ldots\ldots m-n$$

forms, out of the whole

$$1\cdot 2\cdot 3\ldots\ldots m$$

only one group. The total number therefore of these groups, that is of combinations, taking the order into account, is the formula (4). It is clear that we may either call the n accepted—the class actually considered—and the $m-n$ rejected, thrown off and not to be actually considered, or reversely call the $m-n$ accepted, and the n rejected. That is, the number of such combinations of m things n together is the same as that of m things, $m-n$ together. We have used the general term *combinations*. What are technically called COMBINATIONS are *those arrangements in which the* ORDER *is not taken into account as a determining element*. Those in which it is so, are called VARIATIONS or PERMUTATIONS. The latter term is often restricted to the single case for which formula (2) applies,—that, namely, where the number of combinations regardless of form—that is, variations of n things, n together, is to be considered.—We may generalize the idea of combinations, by the consideration above—of classes—into which the total number of things is to be divided. Suppose we have a group of $m+n+p$ things which are to be combined into three partial groups, comprising respectively m, n, p, in each, we shall have

$$\frac{1\cdot 2\cdot 3\ldots\ldots\ldots(m+n+p)}{1\cdot 2\cdot 3\ldots\ldots m\cdot 1\cdot 2\cdot 3\ldots\ldots n\cdot 1\cdot 2\cdot 3\ \ldots p}$$

as the number of ways in which the distribution

might be made, and so whatever be the number of groups, each containing, suppose $a, b, c,$ &c., members, we have the general expression

$$\frac{1 \cdot 2 \cdot 3 \ldots\ldots (a + b + c + \text{\&c.})}{1 \cdot 2 \cdot 3 \ldots a \cdot 1 \cdot 2 \cdot 3 \ldots b \cdot 1 \cdot 2 \cdot 3 \ldots c \text{ \&c.}}$$

This will be the best place to indicate several other theorems of arrangement and combination. If, in the results, we are not excluded from repetition of the same number—thus, if $a\,a$ be considered a combination as well as $a\,b$, we should have m^2 combinations $a\,a$, $a\,b$, $a\,c$, &c., $b\,a$, $b\,b$, $b\,c$, &c., because in each of these groups there is n, and these are m groups. Similarly if the groups are to consist of n individuals, in which any one individual being repeated, may combine with any or no others, we shall have m^n as the number of combinations. Again, if we are to have only absolute combinations regardless of order, we have a more difficult problem.

$$\frac{1}{1 - ax} = 1 + a\,x + a^2\,x^2 + \text{\&c.},$$

$$\frac{1}{1 - b\,x} = 1 + b\,x + b^2\,x^2 + \text{\&c.},$$

$$\frac{1}{1 - c\,x} = 1 + c\,x + c^2\,x^2 + \text{\&c.},$$

Hence,

$$\frac{1}{1 - ax} \times \frac{1}{1 - bx} \times \frac{1}{1 - cx} \times \text{\&c.}$$
$$= (1 + a\,x + a^2\,x^2 + \text{\&c.},)\ 1 + b\,x + b^2\,x^2 \text{ \&c.},) \text{ \&c.}$$

Here evidently the co-efficient of x will be the sum of the combinations one and one together; of the quantities $a, b, c,$ &c.,—that of x^2 the sum of those two and two together, including repetitions of each individual—a^2, b^2, &c.,—and so on. The co-efficient of x^n will be the sum of the combinations of the m quantities $a, b, c,$ &c., taken n together. We want, however, to find the number. Evidently that will not alter, whatever the special values of $a, b, c,$ &c., may be. Suppose, then, all to become 1, then clearly the actual value of the co-efficient of x^n will be the same as the number of co-efficients, because each of the quantities a_n, $a^{-1}\,b$, &c., which make up the total number, becomes equal to 1. Now to find the value of this factor.

$$\frac{1}{1 - x} \times \frac{1}{1 - x} \times \frac{1}{1 - x} \times \ldots\ \text{to } m \text{ times}$$
$$= (1 - x)^{-m} = 1 + \text{\&c.}$$
$$+ \frac{m \cdot (m + 1) \ldots\ldots (m + n - 1)}{1 \cdot 2 \qquad\qquad n} \cdot x^n.$$

Therefore, the total number of combinations, where order is left out of account, but repetition permitted, is

$$\frac{m \cdot (m + 1)\ (m + 2) \ldots\ldots (m + n - 1)}{1 \cdot 2 \cdot 3 \cdot \ldots\ldots n} \qquad (5).$$

The special formulæ of the subject will be at once remembered by those who are familiar with those of the binomial theorem. In fact, the co-efficients in the latter merely consist of them; this theorem at bottom founding upon those permutation formulæ which we have been exhibiting.—A very frequent application of this theory is to the number of times that a special number can be thrown with dice cast any given number of times. Thus, suppose, we are to have the number of times that the number n can be thrown by throwing dice m times. Evidently the results of two throws will be all comprised in the formulæ

$$(x + x^2 + x^3 + x^4 + x^5 + x^6)^2,$$

and of m throws in

$$(x + x^2 + x^3 + x^4 + x^5 + x^6)^m,$$

that is, the number of times that n may be thrown in m throws, will be represented by the co-efficient of x^n in this expansion. That is, of x^{n-m} in the expansion of

$$(1 + x + x^2 + x^3 + x^4 + x^5)_m$$

or of $\left(\frac{1 - x^6}{1 - x}\right)$, or of $(1 - x^6)^m\,(1 - x)^{-m}$.

This subject will come out into distincter practical importance in the doctrine of PROBABILITIES, to which, indeed, that of permutations just treated, serves chiefly as introduction.

Perpetual Motion. If this famous appellation had simply meant perpetuity or indestructibility of Force, it would have stood for an important and undeniable truth. No force is lost in the Universe; we never discern the *loss*, but only the *conversion* of force, *e. g.*, when a machine is brought to a stand through friction, all that has occurred is—the force applied to move the machine, has—through the resistance we call friction—been converted into a mechanical equivalent of *Heat;* and this Heat, by communication and radiation, is in existence playing its equivalent mechanical part. But this is not the common or practical conception attached to the term perpetual motion. It has ever signified as follows:—a machine, whose characteristic is, that the initial or primary force shall be restored or replaced by the very movement it produces. Now, setting aside the fact, that, in every machine of earthly materials, part of the initial force must ever be converted into heat and dissipated through effect of friction, it is clear that, were such a machine consummated, the effect would be not *motion*, but *equilibrium* or *rest.* A machine is a mere medium of connection between *power* at one end, and *effect* at the other; and were these two equal, the machine would simply stand still. The negation of the possibility of perpetual motion may therefore be accepted as an axiom in mechanical science. Mr. Grove has recently shown, in a most ingenious essay read before the Royal Institution of London, that important uses may be made of this axiom as an aid in scientific research. He has illustrated,

by many important instances, that, considered as a test, it might enable us to discern in any experiment, to what degree of approximation we have obtained, from any given natural force, the total quantity of power it is capable of affording; and that it might also serve, on the discovery of any new phenomenon, to show, up to what point, that phenomenon might be put in relation with phenomena formerly known. Mr. Grove's essay well merits the attention of every one interested in the philosophy of the sciences.

Perseus. A constellation surrounded by Andromeda, Aries, Taurus, Auriga, Camelopardalis, and Cassiopeia. Its principal stars α Persei, and β Persei (Algol) are between the second and third magnitude, and the stars, γ, δ, ϵ, ξ, Persei, are of the third magnitude.

Persistence of Visual Impression. Observation and experiment concur to prove that the visual perception of an object does not cease for some time after the rays of light, proceeding from it, have ceased to enter the eye. To this phenomenon the name persistence of vision has been given. Many appearances, in nature and in optical experiments, are explained by reference to this fact. It is most easily observed in the common amusement of children, when a glowing ember is moved to and fro in the air so rapidly as to give rise to the appearance of continuous ribband-like lines of light. In such a case it is evident that the luminous point can only be in one position at a time, and it is known that the velocity of light is, in comparison to such small spaces, infinitely great. The rays which leave the object while occupying one of its positions must enter the eye and make their impression instantaneously, so that if the impression vanished from the sense of vision as rapidly, the object, however quickly it moved, would only appear in one position at a time; and, as this is not the case, it follows that the visual impression must continue or be persistent for some time after the rays of light producing it have ceased to impinge on the sensitive surface. If a white spot be made on the circumference of any revolving wheel, such for instance as the flywheel of an engine, it will be found that as the speed of revolution increases the appearance of the spot will be more and more elongated, till at last it seems to extend over the whole circumference. When this happens, it is evident that the duration of the impression on the eye must have continued from the moment that the spot left any one point in its track till it returned to it again, or, in other words, during one entire revolution of the wheel. This will happen at the same speed on whatever part of the wheel the mark has been made. For instance, if it be made on one of the spokes near the centre, it might at first seem that as the motion is so much slower there than at the circumference the revolution would require to be more rapid in order that appearance of a complete circle of light might be seen; but it must be recollected that though the motion is slow, in the same proportion a shorter space has to be traversed, and in both cases exactly the same time will elapse before each point again crosses any of its former positions in space, or, what is the same thing, any point of the image again passes over the same position on the sensitive curtain or retina of the eye. In this way we see that one entire radial line whitened would, at the same speed of revolution, give the appearance of a flat white disc. With regard to the absolute time of duration of the impression, it is evident that the time occupied in one revolution, when the speed is just great enough to render the circle of light complete, is the period in question. Experiment proves that on an average such a wheel requires to revolve eight times in a second, so that $\frac{1}{8}$th of a second is the time during which the visual impression persists after the object producing it ceases to send rays to the eye. It is found, however, that the more intense the light is, the longer does its impression continue; and also that the colours of the light produce variations in the time of duration—thus, red continues longest, then follow yellow and blue, and the most refrangible or violet rays leave impressions which persist for a much shorter period.

The cause producing this persistence of impressions is no doubt the nature of the physical action which constitutes the impression itself. Since the recent experiments of Fizeau on the velocity of light in traversing different media, have left no doubt but that a ray of light consists of a progressive undulation in the line of its direction, it follows that the retina of the eye must itself be capable of vibration, and must therefore consist of a surface exquisitely sensitive to such undulatory movement. It can easily be understood from the analogy of the surface of a pool of water which has from some cause been agitated, where the motion continues for a longer or shorter interval, depending in some degree on the strength of the disturbing cause, before undulation ceases, so likewise the tremulous surface of the retina itself continues to vibrate after the undulatory line of light ceases to disturb its surface. After ordinary exhibitions of this persistence the appearances vanish, as has been said, in a period of about $\frac{1}{8}$th of a second of time, but if the luminous impression has been very intense, and more especially if it has continued on the same spot of the retina for a great length of time, another variety of persistence, to which the name of ocular spectre has been given, makes its appearance. If the eye has been steadily directed to the sun or to a bright object, such as a candle or gas flame, on looking away at first a general haze of indistinctness occupies the field of vision, but after a few seconds the spectre appears in the shape of an image of the sun or other luminous object, projected on whatever surface the eyes are directed to, and frequently appearing even if the eyelids are alto-

gether closed. A striking peculiarity of these ocular spectra is the diversity of colour through which they pass, at first appearing of a similar shade to the object producing them, and shortly afterwards becoming of exactly the complementary colour; for instance, if a red sunset has been looked at and the eyes closed, a green coloured sun and clouds occupy its place on the retina, which, however, rapidly changes colour to violet, purple, and even black, the shade being modified by the hue of the surface on which the spectre is cast by the movement of the eyes. These varying hues are called accidental colours, and are generally accounted for by supposing that the portion of the retina, on which the luminous impression has been long and powerfully cast, becomes so fatigued as to be incapable for some time of again being excited by the impression of the same colour of light as that which has produced it, while at the same time it retains the capability of being excited by the other colours which constitute light,—hence, when the eye is withdrawn from gazing at a red sun, and directed to a white surface, the green colours of the white light can alone affect the retina, and therefore the fatigued portion of the surface shows only impressions of a green colour, whereas, if the eye had been cast upon the blue sky, an impression of the sun of a blue colour alone would have appeared. In some cases those spectral impressions have remained for many days, and have at last only been removed by keeping the eye at rest and in darkness. One striking peculiarity of these phenomena consists in this, that long after they have ceased to be observable, when the vision is directed steadily at any surface, they can yet be recalled by rapidly opening and shutting the eyelids, or moving to and fro some object before the eyes, so as to vary the appearance of the screen on which the spectre is thrown in the visual field, a circumstance probably to be explained by the supposition that the abnormal vibration of those points of the retina in which the spectre exists (so to speak, in a latent state), though really continuing, is yet so feeble as only to be seen in the interval between its adjustment from one state of vibration to that which is necessary to be established when vision is directed to another kind of surface.—There can be little doubt that the phenomena, of which account has now been given, have been the real origin of many of the so-called spectral apparitions which in all ages have haunted the field of human vision in the silence of night, when the eye, left to repose upon darkness or to wander among the dim shapes of doubtful objects, bodies forth on night's cloudy curtain the shapes of objects depicted on its surface in the hours of sunshine.—To return to the first mentioned, viz., the ordinary kind of visual persistence, when the impression lasts only during $\frac{1}{8}$th of a second, it may be stated that this is one of the few phenomena in the animal economy, to which no important beneficial effects can be traced. Indeed, it may be regarded rather as a defect of vision than anything like a provision in itself. If it had been of greater magnitude, rapid and distinct vision would have been impossible. As only one point of any object can be seen distinctly at a time, it follows that perfect sight of it can only be got by a rapid glance from point to point, a circumstance which would have produced complete confusion if the visual impressions had remained long on the single point of the retina which communicates distinct vision. We are rather led, therefore, to regard the persistence of impressions as a defect of vision, resulting necessarily from the nature of the retina as a vibratory surface, and to admire the facility which it has of again coming so far to rest as to receive a new impression altogether distinct so as to produce the perfection of vision which the human eye is known to enjoy.—While its effects are thus of short duration, they are yet abundantly traceable in natural appearances. Thus the falling rain-drop is globular, yet it appears as a continuous arrow or line of water. The lightning consists but of a single spark of small size, yet it is seen, as a continuous forked line at the same instant over the whole of its track. The revolving spokes of a wheel appear as a semi-transparent sheet. The colours on a revolving disc appear to be so perfectly combined as if they had been mixed in the paint. The flight of a rocket, or of a lighted bomb-shell, is perceived along its whole track at once. The objects on the embankment appear, to the rapidly moving railway passengers, lengthened out into long lines. All such cases, while they are easily understood, show at the same time the confusion of appearances which might arise from this peculiarity of the eye and the necessity for guarding against its undue extension in the construction of the organ.

Several philosophical toys are based upon the persistence of ocular images. The Thaumatrope, or the wonder-turner, consists of a piece of card board, on one side of which is painted *part* of any figure or figures, what is necessary to *complete* the figure being painted on the opposite side, so that when the card remains at rest, only part of the figure can be seen at a time; but if it be suspended by a thread, and whirled rapidly

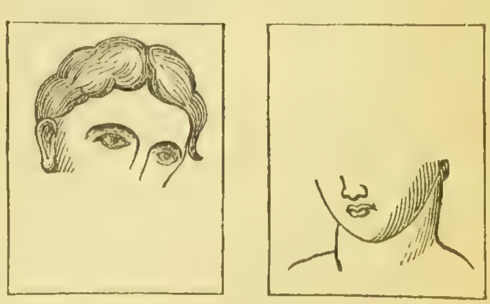


Fig. 1.

round, both sides will be seen at once, and the picture will appear complete.—The Phenakisto-

cope consists of a piece of card board ten or twelve inches in diameter, with slits cut in its circumference about half-an-inch broad and one inch deep. In the intervals between these slits are painted the successive positions of a figure supposed to be in motion. When the card is placed before a mirror, and while the eye is placed behind the slits, it is rapidly whirled round on a pivot at its centre. The images of the figure in successive positions come upon the retina so that the one appears just as the other vanishes,—the figure seems actually to change its position by an act of animated motion, so as in no small degree to imitate the appearance of a living object.—The following curious experiment also finds its explanation in the principle of persistence. A wheel, which may be a disc of pasteboard, with straight radial lines drawn on it as in fig. 2, or any kind of spoked wheel, is made to revolve, and while it is in motion a straight barred grating which may be such as in fig. 3, or the fingers of the hand slightly separated, is interposed between the eye and the disc. As long as the grating is kept at rest the rays appear straight, but when it is moved in a direction transverse to the bars the rays appear curved as in fig. 4. When the motion of the grating is reversed, the curves take an opposite curvature as at fig. 5.—In endeavouring to ascertain the cause of this curious appearance, it soon becomes obvious that,

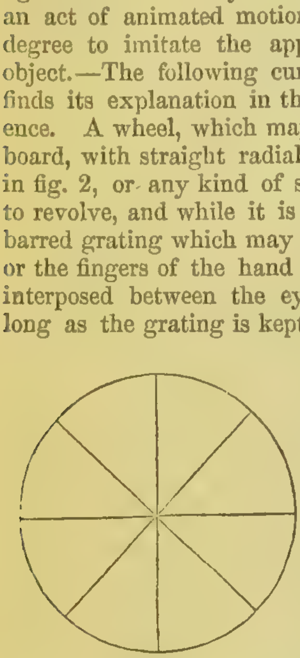


Fig. 2.

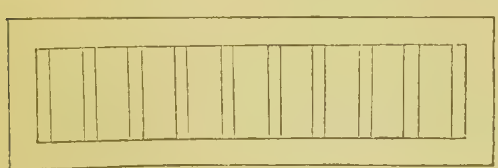


Fig. 3.

while the grating moves uniformly in all the length of its bars, the rays of the wheel, on the contrary, move more and more rapidly as the distance from the centre increases, so that while the vertical ray O A, fig. 6, starts from A and passes over the arc A B, the bar $a\,a'$ only reaches $b\ b'$, and now cuts the ray in the position of the first dot. Again, when the ray goes over an equal angle and is at C, the bar is at $c\ c'$, and cuts the ray in the second

Fig. 4.

position marked, and so on. Now, by the persistence of the images on the retina, these successive positions of intersection are all perceived at once and constitute the curve.—That this is the true explanation can easily be proved mathematically by investigating the properties of a curve which would be formed by the intersection of such an advancing line and a revolving radius, and then examining its properties, or constructing it by points, and observing that it is the same as the one alluded to. The polar equation of such a curve is easily proved to be

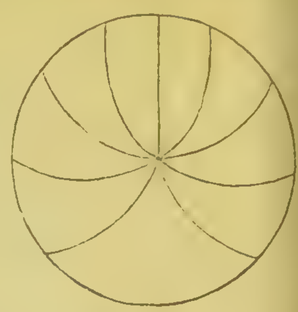


Fig. 5.

$$r = \frac{v\ \theta}{w\ \sin\theta}$$

where r is the radius vector referred to the centre as origin, v the velocity of the advancing bars, w the angular velocity of the revolving ray, and θ the angle which the radius vector makes with some fixed line of reference, in this case the vertical. It will be seen at once that for $\theta = 180^\circ$ the value of r is infinite, so that the downward branches in figure 3 would extend infinitely downward without ever meeting the vertical again, if the bars and rays were long enough. If instead of a series of straight bars, another wheel be interposed between the eye and the revolving rays, other curious curves are perceived when it is set in motion, forming a singular spectacle when thrown on a screen by means of a magic lantern, and affording ample scope for mathematical research.

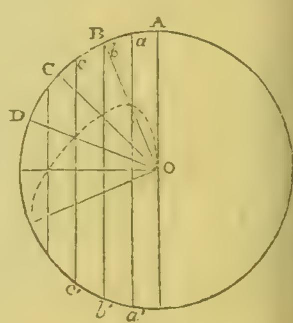


Fig. 6.

Recently, Mr. Rose of Glasgow has made a very important and ingenious addition to the means of illustrating persistence. His method consists in the employment of an apparatus for throwing flashes of light, at regular and short intervals, on revolving discs placed vertically in front of the source of illumination. The discs may be made of large size, and with the illustrations used in the thaumatrope depicted on them. When they are put into rapid rotation in ordinary light, the usual appearance of confusion or blending of the revolving figures takes place; but if the apartment be darkened, and by means of slits in a disc placed at the aperture by which light is admitted to the revolving figure, flashes, rapidly succeeding each other with inter-

ıls of darkness, be thrown on it, all the appear- ıces exhibited by the thaumatrope may be shown › a large audience. The sunlight might be used r this purpose, when admitted through the con- ·nsing lenses of a solar microscope; or the magic ntern may be employed to throw artificial light ı the revolving figure. The disc, having the its cut in its circumference, being placed in ont of, and close to the object-lens of the lan- rn, it is put into rotation with greater or less ·pidity, till the flashes of light reach the revolv- ·g thaumatrope figure in such numbers as to ·velop the desired effect. It is easy thus to ·hibit a rapidly revolving wheel as if it were at ·rfect rest, and, as has been said, to produce on a ·rge scale all the other effects of the thaumatrope. Still more recently, Mr. Edmund Hunt has ·hibited the effects of persistence under another ·rm, by means of what has been called the *colour* ·. This top consists of a metallic disc, fitted ·th a spindle projecting from its centre on both ·les—one end forming the point on which it ·rns, and the other the means by which it is set ·to rapid rotation, by the unwinding of a cord, ·in the common top. Discs, with radial bars of ·fferent colours, are fixed by a binding nut on the ·per side of the top; and after it is in motion, ·other disc, having a central aperture rather ·rger than the spindle of the top, is allowed to ·de down on this spindle. This latter disc is so ·nstructed as to have little weight, that it may ·retarded in its rotation to some extent by the ·mosphere. It is blackened, and has apertures ·different forms cut in it, so as to exhibit the ·loured disc revolving beneath. Great variety ı be given to the forms of the coloured spaces the under disc, and also to the apertures or ·s in the upper disc; and by this means many ·autiful effects of graduated and continually ·anging colours are produced, constituting a most ·tractive as well as instructive optical exhibition.

Perspective. The object of perspective is to ·ake such a representation of an object upon a ·face as shall present to the eye, situated at a ·rticular point, the same appearance that the ob- ·t itself would present were the surface removed. Perspective consists of two parts:—*First*, the ·urate delineation of the principal lines of the ·ture; and *second*, the shading and colouring the picture so as to produce the desired effect ·listance, &c. The first part, called *linear per- ·ctive*, is purely mathematical, and this part ·y will be considered. Perspective drawings ·y be made upon any surface, but we shall ·y consider them made upon a plane. The ·ne upon which the representation is made is ·ed the *perspective plane*, and is generally sup- ·ed to be *vertical*. The point at which the eye ·upposed to be situated is called the *point of ·ıt*. All that part of space situated on the same · of the perspective plane with the eye, is said ·e *in front* of the perspective plane; all on the ·r side is said to be *behind* the perspective plane. —A *visual ray* is any straight line passing through the point of sight. A *visual plane* is a plane passing through the point of sight. A *visual cone* is a cone whose vortex is at the point of sight.—The *perspective of a point* is the point in which a visual ray through the point pierces the perspective plane. If the given point and its perspective are on the same side of the eye the perspective is said to be *real*, if on opposite sides it is virtual.—The *perspective of a straight line* is the intersection of the perspective plane with the visual plane passing through the line.—The perspective of a curved line is the intersection of the perspective plane with the visual cone passing through the line.—The outline of the perspective of any body, is the intersection of the perspective plane with the enveloping visual cone; the line of contact of this enveloping cone with the body, is called the *apparent contour* of the body. The term cone is here used in its most enlarged sense. It may sometimes happen that the enveloping visual surface may be pyramidal, as is the case in finding the perspective of a cube, or other polyhedron, or it may be composed of both conical and pyramidal surfaces; all of these surfaces come under the general denomination of *conical surfaces*.—The perspective of a body is generally obtained by finding the perspective of the principal lines of the body, embracing all those included within the apparent contour. The perspective of any point of a body may be found by drawing a visual ray through it, and determining the point in which it pierces the perspective plane. This operation is tedious, and to shorten the process, other methods have been devised, the best of which is that of *diagonals* and *perpendiculars*. The following definitions of terms are given as necessary to a complete understanding of the method:—A *perpendicular* is a straight line perpendicular to the perspective plane. A *diagonal* is a horizontal line making an angle of 45° with the perspective plane. Through any point in space one perpendicular and two diagonals can always be drawn.—The *centre af the picture* is the point in which the perpendicular, through the point of sight, pierces the perspective plane. The *horizon* is the intersection of the perspective plane with a horizontal visual plane. It passes through the centre of the picture and is horizontal.—The *vanishing point* of a line is the point in which a line drawn parallel to it, through the point of sight, pierces the perspective plane. Every system of parallel lines has the same vanishing point, which is a point common to the perspectives of all the lines of the system. The centre of the picture is the vanishing point of all perpendiculars. If a line is parallel to the perspective plane, its vanishing point is at an infinite distance. The *vanishing points of diagonals* are the points in which the diagonals, through the point of sight, pierce the perspective plane. They are in the horizon of the picture and at distances

from the centre of the picture equal to the distance from the point of sight to the perspective plane.—Magnitudes, to be put in perspective, are given by their projections, or by their distances above a horizontal visual plane, and from the perspective plane. To find the perspective of any point draw any two lines through the point, and find their perspectives; their point of intersection is the perspective required. The most convenient auxiliary lines are the perpendicular and a diagonal through the point.—To find the perspective of the perpendicular, find the point where it pierces the perspective plane, and join it by a straight line with the centre of the picture; this will be the perspective.—To find the perspective of a diagonal, find the point where the diagonal pierces the perspective plane, and join it by a straight line with the proper vanishing point of diagonals; this will be the perspective of the diagonal.—To ascertain the proper vanishing point of any diagonal, conceive it produced till a part of the diagonal comes in front of the perspective plane, then if this line inclines to the right, it vanishes at the right hand vanishing point of diagonal, otherwise it vanishes at the left hand one.—The vanishing point of rays is the point in which a ray of light, through the point of sight, pierces the perspective plane; the vanishing point of horizontal projections is the point in which the projection of the same ray on the horizontal plane through the point of sight intersects the horizon of the picture. These two points are in the same straight line, perpendicular to the horizon. When the former is assumed or given, the latter can be found by drawing through it a straight line perpendicular to the horizon, and finding the point in which it intersects the horizon.—The shadow which any point casts upon any surface, lies upon the ray of light, and upon the projection of that ray upon the surface. Hence, to find the perspective of the shadow cast by any point upon a horizontal plane, find the perspective of the projection of the point upon the plane, and join it by a straight line with the vanishing point of horizontal projections of rays, join the perspective of the point with the vanishing point of rays; the point in which these two lines intersect is the perspective required. These principles are enough to find the perspective of all bodies, and the perspective of their shadows; but certain constructions, in particular cases, serve to facilitate the operations of finding the perspectives of bodies and of their shadows.—The principles of mathematical perspective are intimately connected with the arts of design, and a knowledge of their application is indispensable to the architect, the engraver, and the skilful mechanic. The practice of perspective is particularly necessary to the painter and the sculptor. Perspective alone enables us to represent foreshortenings with accuracy, and its aid is required in the accurate delineation of even the simplest of natural objects.

Perturbations. Under GRAVITATION and KEPLER'S LAWS, those conditions have been explained, to which one celestial body moving around another must be subject; and in LUNAR THEORY, a detailed exposition is given of the leading changes impressed on these conditions, in case of the presence of a third or disturbing body unequally affecting the two constituents of a double system. This latter, or the material of the celebrated problem of the Three Bodies, may serve as an introduction to the general problem of Planetary Perturbations—a problem of the greatest complicacy, and presenting difficulties so great, that we are fain to reach numerical and practical results by way of *approximation.* If the reader picture to himself the solar system as it is—composed of so many orbs ever shifting their relative distances and positions—and if he recollects at the same time that every one of these bodies is at every moment acting upon every other with a force varying with changes of distance and position, he will conceive very readily how arduous the task to estimate all these influences, and so to follow the course of any one planet through its complex path! It is a fortunate circumstance—*that* indeed, which alone renders the problem resolvable under our present analytic means—that the mass of the Sun so enormously exceeds that of any planetary orb. The influence of the grand centre of the system on each planet, on this account so greatly transcends the influences (always conflicting) of its companion planets, that in the main our Luminary is still the controller of every orbit. It is allowed us therefore to represent the perturbations as slight deviations only from Kepler's Laws; and still to assume these laws as the normal conditions of every body's course. The problem of perturbations then takes the following shape:—the normal orbit and habitudes of every planet are known, how can we best determine the small deviations from these? Even after this simplification, however, enough of difficulty remains; and to overcome it we must resort to *artifices* or *conventions.* The fundamental conception adopted almost universally by geometers is termed the *Variation of the Elements.* The meaning of the phrase being as follows:—An ellipse is determined by a few data, termed its *elements.* Now, a planet having, as stated above to move in an ellipse according to Kepler's Laws every element of that *ellipse is imagined in a state of slow but perpetual flux*; so that the ellipse in which the earth is moving at the present moment exists for the present moment only; at the next moment the elements having been altered, through effect of perturbations, the ellipse constituting its orbit will have altered also. The problem is thus transformed into the following—in what manner or according to what law, are these elements constrained to vary through the said effect of perturbations? By this convention, the inquiry appears put into an intelligible and seemingly

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manageable form. But now the analytical difficulties begin. It is impossible to present at once and completely the law of the variations of the elements for any one planet. Just as illustrated in the case of the Lunar Theory, this is accomplished only by a summation of separate changes or details,—now the effect of one class of perturbations being estimated, and now another; and the separate and special values are arranged in separate tables. It were vain to attempt to enter farther on this portion of the subject. The student must apply to the works of Pontecoulant, of Airy, and of Hansen; among separate memoirs, we would specify those of Encke, especially his recent important and very curious one, on the perturbations of the Asteroids. —There are two classes of perturbations, whose difference is well marked—the *Periodical* and the *Secular*. The *first* and simplest class comprises the effects produced on any planet by its companion orbs, in so far as these depend on their *directions* with regard to it, and of course on their magnitudes and average distances. Now, as the relative directions of any two orbs will always recur within a comparatively short time, and, after recurring, again pass through all their variations, this class is called as above, the *Periodic Inequalities*. Their periods, generally speaking, are not long, and their general effect is slightly to accelerate or retard a planet in its orbit. The *second* class again takes into account the *peculiarities of the paths* in which the disturbed and disturbing bodies are moving, peculiarities which immensely prolong the cycle of variations, and which may even be conceived to postpone the period of the recurrence of exactly the same relations, for centuries. These as above are termed the *Secular Inequalities*. Nor are the changes they produce less complex than prolonged. Instead of the mere acceleration or retardation of a planet in its orbit, they bring before the mind's eye, our system slowly changing in every element; orbits enlarging and again contracting; paths almost circular gradually flattening into well marked ovals, and those that were most elliptical verging towards circles; the positions of the ovals changing with regard to the sun and to each other, *i.e.*, their *perihelia* shifting necessarily and determinately through space; the inclination of the planes of the orbits to the plane of the sun's equator undergoing change; and the directions in which these planes lie, also subject to the same singular revolution of the *nodes* which enters as so essential an element in the theory of the eclipses of the moon. Surely when one gazes on a scene so intricate and so restless, astonishment will not be experienced that the immortal Newton, when the necessity of their incessant mutations first appeared to him, gave way for a moment to the dread that through effect of what seemed endless and ever-augmenting confusion, the order of our great system might one day come to a close, and require, to restore it, the intervention of the Creative hand! But as is ever the case, our discovery of this apparent confusion has only led to the perception of a loftier and nobler simplicity. This is manifested by the following two general propositions with which we close this notice:—I. The Law of Areas as stated by Kepler, if disturbed with regard to every single planet, holds most accurately with regard to the aggregate motions of the System projected on any plane; and this has further led to the discovery of that *great equator plane*, whose changes, innumerable centuries hence, will record in a literature intelligible by all the universe, every momentous phase in the destiny of our system. The nature of this extension of Kepler's Law, and the character of the great Equator Plane have been already detailed under AREAS.—II. But notwithstanding this permanence of the Law of Areas for the entire system, might not individual planet come, in the course of ages, to coalesce with individual planet, through the accumulated effect of separate but incessant changes; or, at the very least, might not the orbits of the different globes undergo such enlargement or diminution as would utterly destroy their existing adaptation to the organisms and vitality on their surfaces? This question has been answered and uneasiness all dispelled. Under NEBULAR HYPOTHESIS, we have spoken of certain constituent elements of our planetary scheme, as for instance, the facts that the Earth and its companions move in orbits of but small eccentricity, or almost in circles, that the inclination of the planes of their orbits are small, and that they revolve around our Luminary in one uniform direction. Now it appeared to Lagrange (a defect in whose process has since been supplied by Poisson) that the existence of such primal arrangements, impresses upon the effects of perturbation a condition, which, in the happy phrase of Sir John Herschel, may well be termed the MAGNA CHARTA of our system. Subjecting the entire question, taken in its highest generality, to treatment by his masterly and most penetrating analysis, Lagrange discovered that all changes of the *essential elements* of any orbit which can arise from perturbations, must be *periodical*. The *major axes* of the orbits, for instance, can neither increase nor decrease indefinitely; nay, their variations are confined as if by ordinance within the narrowest limits. But upon the size of the major axis the period of revolution depends; so that we are entitled to conclude that, through effect of disturbing agencies, no orb can ever be withdrawn by more than a quantity nearly unimportant from its normal position in reference to the sun, and that its cardinal period, or its YEAR, must through the long succession of ages abide nearly as it is. It is surely worthy of highest admiration, this simplicity amid so much that seems complex, this assured stability, with so

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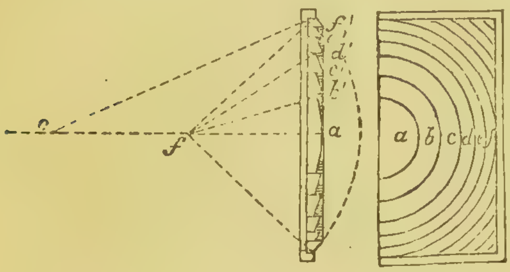
many varying causes around threatening disaster! Nay, our feelings much transcend mere admiration—they belong rather to that state of mind which leads one to adore!

Pharos, or **Lighthouse**.—We owe mainly to the illustrious Fresnel the best and most practicable idea of the mode of constructing lenses that will throw an artificial light to a very great distance, and so enable mariners to discern their precise position amid the rocks and shoals of the coast. The figure below represents an annular lens cut through the middle; it is composed of the segment of a sphere *a*, around which several rings *b*, *c*, *d* are placed—a section of which is seen as under *b′*, *c′*, *d′*.—The curve of



these rings is so calculated that each of them have the same focus *f* as the segment *a* has, so that if a light be placed at *f*, the whole rays falling from it on the lens shall form a set of rays nearly parallel:—they would be rigorously parallel if the source of light were a simple point, and therefore exactly in the focus *f*. The enfeebling of the intensity of the transmitted light being simply due to the *divergence* of the transmitted rays, it follows from the slight amount of divergence under such arrangements, that the luminous beams may thus be thrown, nearly unimpaired, through great distances.—It is by this principle, and by varying the forms of the refringent glasses, that Fresnel has founded that new system of lighthouses now accepted by the chief maritime powers of Europe. By adding a motion of rotation, the Pharos may be made to present momentary eclipses, or a succession of coloured lights.—On this subject we refer also to Mr. Stephenson's Memoir on *Kata-dioptric* lights; nor do his contrivances stand alone. There is a fine and most effective lighthouse in the small Scottish harbour of Kirkaldy, due to one of our best and most ingenious Inquirers and Engineers —Mr. Edward Sang.

Phenakistoscope. An interesting toy devised by M. Plateau, and depending on the principle of the *persistency of impressions*. Its structure is well known, as also the singular effects produced by it; such as the representation of actual motions—leaping, flying, &c.

Phocea. One of the Asteroids. For elements, &c., see Asteroids.

Phœnix. One of Bayer's southern constellations. It is close to Achernar, the bright star in Eridanus, though farther from the south pole. The star α Phœnicis is of the second magnitude; γ Phœnicis of the third, and β Phœnicis between the third and fourth.

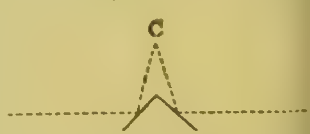
Photography, or Light Writing, as the word literally implies, must be reckoned one of the most admirable discoveries of the present age. It is wholly of recent origin, and can be traced to some accidental observations of Sir Humphrey Davy and M. Daguerre, on the blackening of salts of silver, as is familiarly seen in the case of marking ink, as it is called, or the liquid used for permanently writing on linen or cotton cloth. The superiority of this species of delineation over all others, is its absolute fidelity as contradistinguished from the work of even the most accomplished and accurate artist who, notwithstanding all his efforts to the contrary, yet invariably impresses his pictures with the mannerism which distinguishes his style, as is proved by the fact that they are easily recognized as his the instant they are seen. Thus, there is about photographs a certain degree of reliable veracity which give them, in many instances, incalculable value. The artist unconsciously selects, conceals, flatters, or ostentatiously displays; but the faithful eye of the camera records all alike, omitting no accessary or detail however apparently worthless, and thus such pictures are like nature itself, capable of being relished by all classes of mind, each seeing in them what it looks for, fearless of deceit. Already photography has been turned to a multiplicity of uses, in supplying faithful records of the human face and figure, obtainable without labour, and, it may be said, almost without cost. What would we not give for such exponents of the living features of the great and noble of past ages, or of the chief actors in events which history has rendered sacred? In affording perfect representations of all the most remarkable buildings and passages of natural scenery, photography has already, to a great extent, supplied to those, to whom extensive travelling is impossible, the means of enjoying at least one of its many advantages and gratifications; and ere long there is little doubt but that, so to speak, without the toil of travel, but merely by peering through the ubiquitous eye of the camera, it will be possible to view the natural and artificial productions of every climate of the globe, whether by sea or land, and this with a degree of vividness and accuracy which, in no period of the world's history, would till now have been by possibility attainable. Already photography, in procuring accurate details and materials for his studies, has done great things for the sculptor and for the painter. It registers by night and day to the meteorologist the minutest movements of all his instruments. The Astronomer is now enlisting it in his service. It is the willing slave of the Police, and, greatest of all, it promises in affording endless means of illustration to the Lecturer and the Schoolmaster, to be a most powerful assistant in the great work of education. It is scarcely, then, too much to

say, that its practice must become all but universal, and that every improvement in its processes is of public importance.

Recently it has been discovered, that many substances besides silver are sensitive to the action of light, and, indeed, that there are few kinds of matter which are not acted on by it, although, in most instances, not with sufficient rapidity to render them eligible as photographic agents. As might from this be expected, photographic processes are already numerous, and, fully to describe them, would require a considerable volume.—Under the heads COLLODION, CALOTYPE, and DAGUERREOTYPE, will be found descriptions of some of the most useful modifications. Here, all that will be done is to describe two processes not there alluded to, which are yet, from their applicability to landscape delineation, the portability of the materials, and the ease and certainty with which a traveller can by these means obtain what are called negative pictures of whatever he desires on his route, exceedingly valuable and interesting. The most important photographic processes at present known agree in this, that they all consist essentially in four steps, 1st, the preparation of the sensitive surface; 2d, the exposure of that surface to the image formed by the lenses of the camera obscura; 3d, the development or bringing out, by some chemical means, of the latent image; and, lastly, the fixing of the image so as to render it no longer liable to be affected by light, and, at the same time, protecting it from the injurious action of the air and other foreign agents. Thin films of some of the more sensitive salts of silver, such as the chloride, bromide, or iodide, spread on paper, or, by means of some vehicle on glass plates, constitute, along with the pure silver plate of the daguerreotype, the chief modifications of sensitive surface at present employed. The difference between these processes consists in the various degrees of rapidity with which the exposure in the camera can be completed, and the fact that some of them, such as the collodion process and the daguerreotype, give what are called positive or complete pictures at once, in which the lights and shades appear as in nature,—whereas the calotype, and the two other processes about to be described, are slow in their operation, produce negative pictures, which therefore require to be again used in an operation of printing, whereby their lights and shades are reversed, and then appear in their natural aspects. They have, however, the great advantage of simplicity and precision in their details, which admit of being performed, all except the exposure in the camera obscura, in the operating-room at home. The plates may then be preserved in boxes, so as to be transported in a dry state over long journeys, and for many weeks, and developed and finished at convenience.—The simplest of these negative modifications, is that known as the albumen process, from albumen forming the vehicle by which the iodide of silver is spread and retained on the glass plate. Its details are as follows:—the whites of as many fresh eggs as may be necessary are beaten up in a basin, with a glass rod till they form a complete froth, four grains of iodide of ammonium having previously been mixed with them for each egg used. When this froth has subsided for a few hours, and the limpid albumen has collected in sufficient quantity at the bottom, it is to be poured on the carefully cleaned glass plate, and spread by means of a feather, till it covers the whole surface; after which, the plate is to be held in a vertical position for half a minute or so, to allow of the superfluous albumen running off. It is then to be levelled carefully by means of a spirit-level, and allowed to dry, being kept free from dust. The most difficult part of this process consists in getting the plate flat and level, so as to secure a perfectly equable coating of albumen over the whole surface,—great care and the use of the best plate glass can, however, generally secure this. A simpler plan consists in fixing to the back of the glass plate a piece of gutta percha, to which is attached a rounded stick, and then having poured on and spread the albumen as before, to whirl the plate round in a vertical position before a pretty strong fire so rapidly as to prevent the liquid running more to one edge than another during the few seconds occupied in drying. The subsequent operations are to be done only by the light of a candle. Having got the albumen film properly formed by either mode, the next proceeding is to render it sensitive by immersion for twenty seconds in a bath composed of thirty grains of nitrate of silver and half a drachm of glacial acetic acid to each ounce of water. Having removed it from this bath, it is to be carefully washed in a stream of water, and either immediately exposed in the camera or put aside in a dark place to dry. The exposure in the camera must be prolonged for at least half an hour even in a good light, and in feeble light, or with a small aperture of lens, a much longer time must be given,—the great error being generally too short, rather than too long, an exposure. The image is to be developed, either immediately after exposure, or at a future time as may best suit, by immersing the plate in a saturated solution of common crystallized gallic acid in water, to which four drops of the same mixture as composed the silver bath, is to be added for each ounce of water. The gallic acid ought to be dissolved, and the silver added to it only before using, as it does not keep fit for use in a state of solution. In the course of half an hour or an hour, the picture will make its appearance in the form of dark shades where the lights of the camera acted on the glass, and generally with exquisite sharpness and precision. If the development is not sufficient, after a few hours, to give opacity enough to enable the high lights to print well, fresh developing solution can be used, and continued till this result is obtained. The fixing process is

then to be gone through, by first washing the plate in water, and then immersing it in a mixture of one part of a saturated solution of hyposulphite of soda in water, with two more parts of water to render it weak enough not to cause the lifting of the film from the plate, which is apt otherwise to occur. In the course of half an hour, the milky-like opalescence will be dissolved out by means of the soda, after which water is to be freely and repeatedly applied, so as to remove completely the soda from the film. The negative picture is now complete, and may be used for printing on paper, as described in CALOTYPE, or may be applied over another prepared albumen plate, and the two exposed to light and finished, as has now been described, so as to give the kind of transparent picture now so much used as a means of illustration in the stereoscope. The chief inconvenience of the albumen process, is the great length of exposure required in the camera, precluding the introduction of living figures into the views so taken, and thus giving the streets, of even the most busy cities, a death-like desertion. In September, 1855, M. Taupenot of Paris, invented a modification of the albumen process, which has since come deservedly into esteem, and is now known by his name. Its advantages are chiefly in the facility with which the film can be formed, the absence of the necessity of levelling the plate during the drying, and the much greater sensibility it possesses compared with the common albumen, only about one-tenth of the time of exposure being necessary. Taupenot's process may be briefly described thus:—A collodion plate is prepared in the manner stated in COLLODION, till it is ready for the camera; but instead of exposing it, it is washed well in water, and then, while moist, is used exactly as the common glass plate would be in the albumen process, that is, the albumen prepared in the same way, is poured over it, but instead of being levelled, it is held before a fire in a vertical position to dry, after which it is sensitized in the same bath, and for the same time as the albumen plate, and subsequently treated in the same manner. Taupenot's process, as will be at once obvious, is nothing more than this, viz., spreading the albumen film over a layer of collodion, containing iodide of silver, instead of over the bare surface of the glass. It is certain that the iodide of silver is not in this case the sensitive film, as it may be freely exposed to common daylight before the albumen is applied without any injury to the picture, and it is also all subsequently removed in the fixing process. Whence, then, comes the increased sensitiveness? Taupenot himself supposes that the presence of the iodide of silver in an active state, in close proximity to the albumen film, hastens, by some catalytic action, the effect of the light, but it seems to the present writer, more likely that the true cause consists in the fact that the white film of iodide of silver, immediately behind the transparent albumen, reflects the light back, and enables it to expend itself in effecting the chemical change, whereas it would have otherwise been in a great degree lost on the black surface of the camera slide.

Photometer. An instrument intended to enable the observer to compare the intensity of the light of different luminous points. The photometer is far from being so perfect or reliable, under any of its forms, as the thermometer; its indications are still ultimately dependent for evaluation on a judgment by the eye; and there is a material difficulty in the way of correct determination, in the fact that bright objects differ in the *quality* as well as in the *quantity* of the light they emit, *i. e.*, they are of different colours. This latter difficulty, indeed, can scarcely be surmounted; we have no means of estimating different degrees of brilliancy with any approach to correctness, unless the shining points are of the same colour, or very nearly so. —Several photometers have been proposed,—the chief ones being as follows:—1. *Rumford's photometer.* The indications of this instrument depend on our power of estimating the *equality of shadows.* It consists of a white screen, behind which two equal opaque objects are placed at the same distance. If two lights are to be compared, it is so arranged that each of them throw upon the screen the shadow of one of the opaque objects. The lights are then so placed, that the two shadows seem uniform; which being attained, the intensities of the two luminous points will be as the squares of their distances from the screen.—2. *Ritchie's photometer.* This instrument is extremely simple. By aid of two reflecting surfaces, joined at the apex as below, rays from the two luminous points are reflected upwards to meet the eye at C, so that images of both are seen. The brighter light is then with-



drawn until the two images appear *equally brilliant;* in which event the relative brightnesses are estimated by the same proportion as before. This instrument may be termed the reverse of Count Rumford's.—3. *Wheatstone's photometer.* A small sphere, with a reflecting surface, being placed between the two lights, each light is seen in it by the spectator—the two being reflected from different points of the sphere's surface. By an ingenious but simple mechanical contrivance, a rapid looped motion is communicated to the ball; and by the principle of the *persistence of impressions,* the spectator immediately sees two looped curves of different brightnesses. The brighter light is removed until these curves seem of the same brightness, and the intensities of the luminous points are then as the squares of the distances. Unquestionably this species of photometer enables the eye to judge more accurately of the equality of two adjacent lights, than any other contrivance hitherto proposed.—

4. *Babinet's polarizing photometer.* In this very beautiful instrument the two lights are so placed that the rays from a small disc, illuminated by one of them, shall be polarized by *reflection* from a bundle of plates of glass, while the rays from a similar disc illuminated by the other point *through* the plates, and therefore polarized by *refraction*, proceed along the same line with the other polarized rays. Both sets of rays pass together through a plate of quartz of double rotation, and are then seen by the experimenter looking through a doubly refracting achromatic prism. Since the plane of polarization by reflection is at right angles to the plane of polarization by refraction, it is evident that the plate of quartz will be presented to the eye, by the two sets of rays, with complementary colours. If the lights are of equal intensity, these complementary colours on blending, should give a *white* image of the quartz plate; if they are not equal, the plate will appear tinted. Let the brighter light be removed until all tint disappears, and the relative brilliancies will result as a consequence of the proportion already laid down.—It is clear, that in all these photometers, no provision whatever is made for differences of colour in the luminous points whose intensities are to be measured.—5. *Photometers for measuring the relative brilliancy of the Stars.*—These contrivances and instruments are quite as imperfect as the previous ones. A very complex although apparently effective apparatus, was devised some years ago by Steinheil of Munich, but its complexity, probably, has prevented its being put to use. The principle of all the methods generally employed is the following:—The brilliancy of the image of a star seen through a telescope, depends—other things being equal—on the size of the object-glass. By obscuring the outer rings of the object-glass, the image of a star of a high magnitude can readily be reduced to the light of a star of any lower magnitude seen through the entire object-glass. The relative brilliancy of the two bodies may then be inferred from the diameters of the free object-glasses employed. Much better is the use of two corresponding telescopes at the same time, as the eye can better compare two images actually before it than estimate from recollection. Or, the size of the object-glass may be noted, which suffices to endow a small star with the brilliancy of another star as it appears to the naked eye. Professor Manuel Johnson of Oxford at one time employed with much success, a method founded on the degree of illumination necessary for the observation of a star: but he has since largely used, for this purpose, the great Oxford Heliometer. In measuring the distance between two unequal stars, it is necessary to reduce the aperture of one of the segments of the divided object-glass. If then the aperture of the segment, through which the brighter star is seen, be reduced until both stars appear of the same magnitude, the ratio of light will be inversely as the diameters of the apertures. We already owe much invaluable information to researches thus conducted.—See STARS.

Photometry. The branch of science concerned with the methods of estimating the brilliancy of different lights, and the results of such inquiries. Nothing can be added here to the contents of the foregoing article.

Physics. A general term which applies to the study of those laws of natural phenomena in the appearance of which the element of life is not largely intermingled. It is in this country synonymous with the phrase Natural Philosophy. It includes also ordinarily the science of mechanics, *i.e.*, the application of force to inanimate matter—the force being that evident one which voluntary exertion or impact gives. All other developments of the action of forces on inanimate matter, vital forces excepted, are treated of under this same name.

Piezometer. An instrument (invented by Oersted) for measuring the compressibility of liquids. It is a vessel of glass, consisting, like a thermometer, of a bulb and a stem, and having a graduated scale attached along the stem. The upper end of the stem is open. The liquid whose compressibility is to be measured is introduced into the piezometer, so as not quite to fill it; and on the upper surface of the liquid in the stem is placed a globule of mercury to serve as an index. The piezometer, with its enclosed liquid, is then immersed upright in a very strong closed glass cylinder, which is completely filled with water, and to this water pressure is applied by means of a screw. The amount of the pressure is ascertained by observing the compression of the air contained in a glass tube, which is also immersed upright in the cylinder, with the upper end closed, and the lower end open. The water in the cylinder presses on the piezometer and its enclosed liquid equally in all directions; and the descent of the mercurial index in the stem shows the excess of the compressibility of the liquid above that of the glass of which the bulb is made. Piezometers with bulbs of different materials, filled with some liquid whose compressibility is known, have been used to ascertain the cubic compressibility of the material of the bulbs. In this manner it was discovered by Dr. J. D. Forbes that the cubic compressibility of caoutchouc is nearly the same with that of water; for when a piezometer with a caoutchouc bulb, filled with water, is compressed, the index remains nearly stationary in the stem.

Pisces. The last constellation of the zodiac. It consists of two fishes linked together by a string tied to their tails, the upper being close to Andromeda, and the lower close under the wing of Pegasus. The largest star, α Piscium, is of the third magnitude.

Piscis Australis. Also called Piscis Austrinus, the Southern Fish. It is one of the an-

cient constellations directly under Aquarius, by whom it seems to be in the act of being supplied with water. It has one star, Fomalhaut, of the first magnitude.

Piscis Volans. The Flying Fish. A southern constellation of Bayer's, between the north pole and the constellation Argo. It has no stars above the fifth magnitude.

Piston. A solid beam whose lower part performs the office of a cork closing the body of a cylindrical vessel in which it moves, wherever it is applied along the length. To this, in the centre, a rod is fastened, which rises or falls with it; and with this rising or falling, the motion of the machines which use the piston, is connected directly. In engines, the piston is moved by the expansive force of steam (in steam engines), or heated gas (in air engines); and the motion of the rod being communicated, the engine is set moving. It is of every importance that the piston should exactly fit the cylinder. If it do not, it is manifest, that the expanding steam or gases will not act exclusively on the piston, which is to be moved, but will make their escape by the easier passages. In order that it should fit, various ingenious contrivances have been made, the best of which appears to be that now used—where, round an accurate circular rim, a leather rim is put, kept distended and applied very closely to the cylinder by steel springs. Other methods also are employed.

Planetarium. See ORRERY.

Planimeter. A name given to instruments designed to measure by mechanical means and at once, the area of any plane figure drawn on paper. We owe an accurate and effective one, easy of management, and of quite transportable weight and dimensions to M. Amsler of Schaffhausen. There are others, likewise, especially an excellent one by Mr. John Sang of Kircaldy. He names it a PLATOMETER.

Plates Thick and Mixed, Colours of. The phenomena now referred to, are very various and remarkably beautiful. They are explicable by the Wave-Theory of Light in a way quite analogous to the subject of the following article; but as that article has assumed a considerable extension, we shall only briefly allude to these other appearances.—The colours of *thick plates* were first observed by Newton, and may be produced by the following simple contrivance. Having covered the interior of a shutter with a sheet of white paper, make a circular hole in it, and through this admit a ray of light into the room. If that ray is permitted to fall on a glass mirror about a quarter of an inch thick, and quicksilvered behind—both surfaces having a radius of curvature equal to the distance of the mirror from the shutter—a series of four or five coloured rings will be reflected on the paper; the colours succeeding each other in the order observed in the transmitted system in the case of thin plates. Sir David Brewster produced the same phenomena by using two plates of glass of equal thickness; for an account of which experiment we must refer to his treatise on *Optics*. This is also a fine case of interference, and admirably fitted to illustrate the cause of this whole class of appearances.—The phenomena of *Mixed Plates* are still more remarkable, and susceptible of innumerable variations. They were known to Dr. Thomas Young, who produced them by pressing portions of *water*, *butter*, or *tallow*, between two plates of glass, or between two object-glasses that give the ordinary rings of thin plates. The rings thus evolved are very large. Seen by the direct light of a candle, they begin from a white centre; but on the dark space next the edge of the plate there is another set complementary to the first and beginning from a black centre. For the complete development of this curious subject, and many fine contrivances for rendering the phenomena appreciable in all their varieties of forms, we must again simply refer to the Memoirs and other writings of Sir David Brewster.

Plates Thin, Colours of. The *colours of thin plates* were first noticed by Boyle and Hooke. They are displayed in every instance in which transparent bodies are reduced to films of great tenuity. Boyle succeeded in blowing glass so thin as to exhibit the phenomena; they are more readily developed in mica, and some other transparent minerals, which possess a lamellar structure; but the most familiar instance of their exhibition is in the froth of liquids—the fluid envelope of the bubbles which compose it being in general of extreme thinness. These colours vary with the thickness of the film, and disappear altogether when it passes certain limits. When the film exceeds a certain thickness all the colours are equally reflected, and the reflected light is therefore *white*. On the contrary, when the thickness falls below a certain limit, no light whatever reaches the eye, and the surface of the film appears absolutely *black*. These facts may be observed in the common soap-bubble, when properly defended from the disturbing influence of currents of air. If the mouth of a wine-glass be suddenly dipped in water, which has been rendered somewhat viscid by the mixture of soap, the aqueous film which remains in contact with it after immersion will display the whole succession of these phenomena. When held in a vertical plane, it will at first appear uniformly white over its entire surface; but as it grows thinner by the descent of the fluid particles, colours begin to be exhibited at the top, where it is thinnest. These colours arrange themselves in horizontal bands, and become more and more brilliant as the thickness diminishes—until, finally, when the thickness is reduced to a certain limit, the upper part of the film becomes completely black. When the bubble has arrived at this stage of tenuity cohesion is no longer able to unite the other forces which are acting on its particles, and it bursts. Similar phenomena

may be observed when a drop of oil is let fall on water—as it spreads rapidly over the surface, it is soon reduced to a very thin film, which displays the spectral colours. Every one has noticed the fact that steel and other metals when polished, acquire various shades of colour by exposure to the air. These colours are produced by a thin coating of metallic oxide which is gradually formed on the surface. The formation of this oxide is greatly accelerated by an augmentation of temperature, and the colour thus formed is so invariably connected with the thickness of the film, and this latter with the degree of heat, that artists are in the habit of measuring the temperature by the colour developed. Thus steel, in the process of tempering, is said to have received a yellow heat, a blue heat, &c. The same appearances are displayed in a still more striking manner by air itself, or even by a vacuum. If two plates of glass be pressed together by the fingers, we shall observe round the point of nearest approach, a succession of coloured bands of great brilliancy, which dilate as the pressure is increased, and the enclosed plate reduced in thickness.—But, in order to observe these phenomena, in such a manner as to be enabled to trace their laws we must follow Newton. Newton's experiment consisted simply in laying a convex lens of glass upon a plane surface of the same material. The thickness of the plate of air increases as the square of the distance from the point of contact, and is therefore the same at all equal distances from that point. Hence, as the reflected colour depends on the thickness, the bands of the same colour will be arranged in concentric circles of which that point is the centre. The same succession of colours is produced when any other transparent fluid is enclosed between the glasses. The colours, however, are more vivid, the more the refractive power of the plate differs from that of the substances within which it is enclosed. When we look attentively at these rings, the light being reflected always at the same angle, we observe that the central one is not a mere annulus, but a complete circle of nearly uniform colour.—If then we diminish the thickness of the plate of air, by pressing the two glasses more closely together, the central circle is observed to dilate, and a new circle of a different colour to spring up in its centre. This will dilate in turn, driving the former before it, and another circle appear within it; until at length a *black spot* shows itself in the centre of the system, after which no further diminution of thickness will alter the succession. When the black spot makes its appearance, we have obtained a plate of air so thin as no longer to reflect any colours, and the phenomenon is then complete. Newton traced seven coloured rings round this spot, the colours of which were said to be of the first, second, third, &c., order, according to the order of the ring to which they belong. Thus, the red of the third order is the red which is found in the third ring from the central black, &c. The whole succession of colours is called *Newton's scale.*—The principal laws of these phenomena are included in the following propositions:—1. In homogeneous light the rings are alternately *bright* and *black;* the thicknesses corresponding to the bright rings of succeeding orders being as the *odd* numbers of the natural series, and those corresponding to the black rings as the intermediate *even* numbers.—2. The thickness corresponding to the ring of any given order varies with the colour of the light,—being greatest in red light, least in violet, and of intermediate magnitude in light of intermediate refrangibility. In white or compound light, therefore, each ring will be composed of the rings of different colours, succeeding one another in the order of their refrangibility.—3. The thickness corresponding to the ring of any given order varies with the *obliquity*, being very nearly proportional to the secant of the angle of incidence.—4. The thickness corresponding to the ring of any given order varies with the *substance* of the reflecting plate, and in the inverse ratio of its refractive index.—In order to establish the first of these laws, it is necessary to employ homogeneous light. This may be obtained by means of the prism; or we may adopt the method suggested by Mr. Talbot, and illuminate the glass with a spirit lamp having a salted wick. The light of such a lamp being a yellow of almost perfect homogeneity, the rings will be ultimately *black* and *yellow;* and their number is so great as to baffle any attempt to determine it. The law of the thicknesses corresponding to the successive rings is easily established. Let o be the point of contact of the plane and spherical surface, and $a\ a'$, $b\ b'$, $c\ c'$, &c., the diameters of the successive rings formed round that point as a centre. It is evident that the thicknesses of the plate of air at the point where these rings are formed $a\ \alpha$, $b\ \beta$, $c\ \gamma$, &c., are

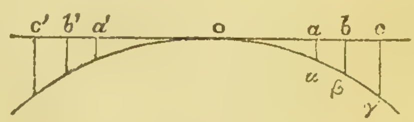


Fig. 1.

as the squares of the distances, o a, o b, o c, &c., or as the squares of the diameters of the rings. To determine the law of the thicknesses, therefore, we have only to measure these diameters. This was done by Newton with great accuracy, and it was found that their squares were in *arithmetical progression*, consequently the thicknesses at which the successive rings were formed increased in a similar progression. But Newton did not stop here. He ascertained further the *absolute thickness* of the plate of air at which each ring was formed, and that in the most exact manner. It is manifest that if the thickness of the plate be determined for any one of the rings,

that corresponding to the others will be given by the law just stated. Newton, accordingly, proceeded to ascertain this thickness for the dark ring of the *fifth* order. This was done by measuring its diameter accurately, and determining the radius of the spherical surface from the focal length of the lens and its refractive index. These two elements being obtained, the thickness is immediately deduced; for it is equal to the square of the radius of the ring divided by the diameter of the spherical surface. The value thus deduced being suitably corrected, it was found that the thickness of the plate of air was the $\frac{1}{17800}$ of an inch, at the dark ring of the fifth order, and this thickness being decuple of that corresponding to the first bright ring, it followed that the thickness of the plate of air, where the *first bright ring* was formed, was the $\frac{1}{178000}$ of an inch. Thus the *bright rings* of the successive orders are formed at the thicknesses $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, &c., and the intermediate *dark rings* at the thicknesses $\frac{2}{178000}$, $\frac{4}{178000}$, $\frac{6}{178000}$, $\frac{8}{178000}$, &c. These determinations belong to the most luminous rays of the spectrum, or those at the confines of the orange and yellow. The variation of the diameters of the rings (or of the thickness of the plate of air at which they are exhibited) with the *colour* of the light may be observed by illuminating the glasses with different portions of the spectrum in succession; or, yet more simply, by looking at the rings through coloured glasses; and it will be found that the magnitude of the rings is greater, the less the refrangibility of the light.—This being understood, it is easy to comprehend the succession of colours in each ring, when white or compound light is used. For the rings in this case are the aggregate of the rings of different colours; and these being of different magnitudes, the compound ring will be variously coloured, the more refrangible rays occupying the interior and the less refrangible the exterior parts of the ring. It is easy to see also, that, in this case, all phenomena of colour must disappear after a few successions, the rings of different colours, belonging to different orders, being at length superposed.—The variation of the rings with the *obliquity* of the incident light may be observed by depressing the eye. The rings are then seen to dilate rapidly with the obliquity of the reflected pencil; the thickness of the plate of air at which they are exhibited being nearly as the secant of the angle of incidence or reflection. —The fourth and last law, which expresses the dependence of the thickness at which any ring is formed upon the *refractive power* of the plate, is easily verified by introducing a drop of water between the glasses. The rings are then observed to contract, and if we compare their diameters in air and in water, it will be found that the corresponding thicknesses of the plate are as four to three, or in the inverse ratio of the refractive indices. We have hitherto spoken only of the reflected rings. There is another system of rings formed by *transmission*, but much fainter than the former. The transmitted rings are found to observe the same laws as the reflected rings, with this remarkable exception, that the colour transmitted at any particular thickness of the plate is always *complementary* to that reflected at the same thickness; so that, in homogeneous light, the bright transmitted ring is always at the same distance from the centre as the corresponding dark one of the reflected system. Such being the phenomena, the question arises as to their physical theory. Already on several occasions (see LIGHT) we have noticed Newton's very artificial theory of *fits*. The theory may be said to have been originally suggested by the colours of thin plates; but as it is now quite discredited and has been found wholly inefficient to explain the facts, we shall venture to lay it entirely aside. It soon appeared that the light reflected from *both* surfaces of the plate is essential to the production of the phenomena; and this fact led to the very threshold of the true explanation. The light incident on the first surface of the plate is in part reflected, and in part also transmitted. The transmitted portion undergoes a similar subdivision at the second surface; and part of the portion reflected at that surface will emerge through the first, and reach the eye along with that originally reflected there. Thus the reflected light consists of two portions, one reflected at the upper, and the other at the lower surface of the plate; and these two portions—one reflected at the upper, and the other at the lower surface of the plate—will *interfere*, and reinforce or weaken each other's effects according as they reach the eye in the same or in opposite phases. This mode of explaining the phenomena of thin plates was pointed out by Hooke in his *Micrographia*, some years before the subject was taken up by Newton. In this passage he very clearly describes the manner in which the rings of successive orders depend on the interval of retardation of the second "pulse" or wave, with respect to the first, and therefore on the thickness of the plate. But he does not seem to have had any distinct idea of the principle of interference itself; and his conception of the mode in which the colours resulted from this "duplicated pulse" is entirely erroneous. Euler was the next who attempted to connect the phenomena of thin plates with the wave theory of light, but the attempt, like all the physical speculations of this great mathematician, was signally unsuccessful, and the subject remained in this unsettled state until the principle of interference was discovered by Young. When this principle was combined with the suggestion of Hooke, the whole mystery vanished. The application was made by Young himself, and all the principal laws of the phenomena were readily and simply explained. Let *m o n* be the course of a ray reflected at the first surface of a plate; *m o p o′ n′* that

of the ray reflected at the second surface, and twice transmitted through the first. From the point o' let fall the perpendicular to $o' r'$, upon the reflected ray $o\ n$; it will be also perpendicular to $o'\ n'$, and will therefore be parallel to the front of the two reflected waves. Now, let us conceive a wave reflected at the first surface, in the position $o\ r'$, to meet at the same place an anterior wave reflected at the second surface, and let us calculate the original interval between them. From the time that they reached the first surface at o, one has travelled over the space $o\ r'$ and the other over the space $o\ p + p\ o'$. But, if we let fall the perpendicular $o\ r$ upon $p\ o'$ it is evident from the law of refraction that the spaces $o\ r'$ and $o'\ r$ are traversed in the same time in the two media; and, consequently, that the interval of retardation is the time of describing $o\ p + p\ r$. Now $p\ r = o\ p \cos 2\ o\ p\ q$, and therefore $o\ p + p\ r = o\ p\ (1 + \cos 2\ o\ p\ q) = 2\ o p \cos^2 o\ p\ q$. But $o\ p \cos o\ p\ q = p\ q$; and consequently, the interval is $2\ p\ q \cos o\ p\ q$. Or, if we denote that interval by δ, the thickness of the plate $(p\ q)$ by t, and the angle $o\ p\ q$ by θ,

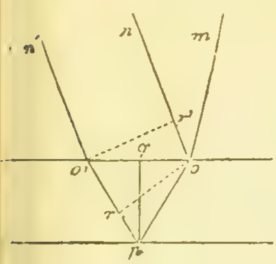


Fig. 2.

$$\delta = 2\ t \cos \theta.$$

Now, the two waves are in complete accordance or discordance, when the interval of retardation is an exact multiple of the length of half a wave, i. e., when

$$\delta = n\ \frac{\lambda}{2},$$

n being any number of the natural series. Equating these values of δ, therefore, we have, for the values of the thickness of the plate which will produce a complete accordance or discordance of the two waves,

$$t = \tfrac{1}{4}\, n\, \lambda \sec \theta.$$

We learn, then, 1st, that the successive thicknesses of the plate, for which the intensity of the reflected light is greatest or least, are as the numbers of the natural series; 2dly, that, for different species of simple light, these thicknesses are proportional to the length of the wave; 3dly, that, for different obliquities, they vary as the secant of the angle of incidence on the exterior medium; and, 4thly, that, for plates of different substances, they are proportional to λ, and therefore in the direct ratio of the velocity of propagation, or in the inverse ratio of the refractive index of the substance of which the plate is composed. There is one part of the preceding explanation which will require a little consideration. The two waves being in complete *accordance* when the interval of retardation is an *even* multiple of the length of half a wave, and in complete *discordance* when that interval is an odd multiple of the same quantity, it would seem, from the foregoing account, that the bright rings should be formed at all those points for which n is an even number in the formula above given (or the thickness an even multiple of $\frac{1}{4}\ \lambda \sec \theta$), and the dark rings at those points for which it is odd. If this were true, the point of contact should be a point of accordance, and the rings should commence from a bright centre, instead of a dark one. This apparent discrepancy is explained by the fact, that the two reflections take place under opposite circumstances; one of the rays being reflected at the surface of a *denser*, and the other at that of a *rarer* medium. The effect of this difference will be best understood by a simple illustration. When one elastic ball strikes another at rest, it communicates motion to it in all cases; but its own condition after the shock will depend on the relative masses of the two balls. If the balls be equal, the first will remain at rest after the shock. If they be unequal, it will move; and its motion will be *in the direction* of its former motion when its mass *exceeds* that of the second ball; it will be in the *opposite direction* when it is *less*. This will help us to understand what passes when a wave reaches the surface, separating two media. The particles of ether next the bounding surface communicate motion to the adjacent particles of the second medium, and thus give rise to the *refracted wave*. But the former particles will not remain at rest afterwards, unless the density and elasticity of the ether be the same in the two media. When this is not the case, the particles of the first medium will move, after communicating motion to those of the second, and in moving give rise to the *reflected wave*. Thus *refraction* is always accompanied by *reflection;* and this reflection is greater, the greater the difference of the densities of the ether in the two media. It appears also, from what has been said, that the *direction* of the motions, or the particles of the first medium, after they communicate motion to those of the second, will be different, according as the ether is *denser* or *rarer* in the first medium. In the former case the vibration of the particles is in the *same direction* that it was before; in the latter it is in the *opposite direction*. Thus there will be a reflected wave in both cases; but in one case this reflected wave is caused by a vibration in the *same* direction as that of the incident wave, in the other by a vibration in an *opposite* direction. The result of this difference is obviously the same as if one of the systems of waves were to gain or lose half an undulation on the other; so that when the two waves, reflected from the two surfaces of the plate, should be in complete accordance, as far as depended on the difference of the lengths of their paths, they will actually be in complete discordance, and *vice versa*. Thus the *dark* rings will be formed where the thickness of the plate is an *even* multiple of $\frac{1}{4}\ \lambda \sec \theta$; and the *bright* ones where that thick-

ness is an odd multiple of the same quantity; and thus the facts and the theory are reconciled. We have spoken of another set of rings visible by transmission. These are produced by the interference of the rays *directly* transmitted through the plate with those which penetrate it after *two interior reflections.* It follows from the preceding considerations that they must be *complementary* to those seen by reflection; and this is observed to be the case. The extreme paleness of the transmitted rings arises from the great difference in the intensity of the interfering pencils. The theory of thin plates, as it came from the hands of Young, laboured under an imperfection, which was, however, soon removed. It is obvious that the intensity of the two portions of light, reflected from the upper and under surfaces of the plate, can never be the same,—the light incident on the second surface being already weakened by partial reflection at the first. These two portions, therefore, could never wholly destroy one another by interference, and the intensity of the light in the dark rings could never entirely vanish, as it appears to do when homogeneous light is employed. M. Poisson was the first to point out and to remedy this defect in the theory. It is evident, in fact, that there must be an infinite number of partial reflections within the plate, at each of which a portion is transmitted; and that it is the sum of all these portions, and not the two first terms of the series only, which is to be considered in the calculation of the effort. When the problem is taken up in this more general form, it is found that where the effective thickness of the plate is an exact multiple of the length of half a wave, the intensity of the reflected and transmitted lights will be the same as if it were removed altogether, and the bounding media placed in absolute contact; so that when these media are of the same refractive power, the reflected light must vanish altogether, and the transmitted light be equal to the incident.—It ought to be added that the finest illustrations of the iridescent lines on the surfaces of metals, alluded to above, are produced when thin films of peroxide of lead are formed upon steel plates by the electrolytic decomposition of acetate of lead. These gorgeous tints were discovered by M. Nobili, and are commonly known as *Nobili's metallochromes.* See Taylor's *Scientific Memoirs*, vol. i.

Pleiades. A constellation in the sign Taurus, near Hyades. The latter form the forehead and eye of the bull; the Pleiades form the shoulder. Their name is said to be derived from the daughters of Pleione and Atlas; more probably from πλέω, to sail. There are said to be seven of them visible, but they are so close that the unaided eye, in our latitudes, can never make out more than six; the telescope reveals multitudes more. In the catalogue of Flamsteed, they rank as 17, 19, 20, 23, 25, 26 Tauri. The star Alcyone is the brightest of the Pleiades. This is the star around which as their common centre Dr. Mädler of Dorpat considers that the whole orbs of the firmament revolve. See Stars.

Pleochroism: Polychroism. Under article Dichroism, the nature of the phenomenon now referred to, has been explained. The latter term was first used to designate the phenomenon of two different colours observed through a perfectly homogeneous crystal, when that is turned in different directions; and, although these two principal colours are united—the one to the other by intermediate tints—the term dichroism is still retained when we speak of crystals with one optical axis. More recently, Soret and others have noticed in topaz and other species, three well marked tints, along three rectangular axes,—a phenomenon termed Trichroism. If the crystals in question are cut in the form of a sphere, we may notice at the sides of the pure or limiting tints, all intermediate tints: the name *pleochroism* or *polychroism*, has therefore been proposed to designate the entire phenomenon,—of course comprehending both *dichroism* and *trichroism.*—The following are some of the conclusions drawn by M. Haidinger, regarding this curious subject: for fuller information the student must refer to the original memoirs of this eminent mineralogist. In crystals, with two optical axes, the separation and distribution of shades or colours, depend as much upon their structure as in the case of crystals with one optical axis;—it takes place along the axes of elasticity, which are always rectangular. In orthotypic forms, these axes coincide with the crystallographic axes: in augitic forms, with the principal axis, the transverse axis, and the normal to these two: in anorthic forms, one of the axes of elasticity coincides with the principal axis, the second is perpendicular to the longitudinal face, and the third is perpendicular to these two. A crystal, with one optical axis, placed vertically before the opening of the *dicroscope* (see Microscope Dichroic), yields an ordinary superior image o, and an extraordinary inferior one e; these two images are sometimes differently tinted, and each preserves its colour while the crystal is turned round its axis. Often, however, the colour of the two images is the same. If a crystal, with two optical axes, is examined in a similar way—placing the three axes of elasticity successively in a *vertical* position—it will be found that the extraordinary image preserves its colour during the rotation, while the ordinary image changes its tint in two directions mutually perpendicular. But in turning the crystal, with reference to a horizontal axis placed *transversely*, the inferior ordinary image preserves its colour, while the extraordinary ray, indicated in the dichroscope by two inferior images of different colours, gives a maximum and a minimum. Haidinger has carried out these researches in the case of a great variety

of crystals; but our limits do not permit us to enter on them.

Pluviometer. See RAIN-GAUGE.

Pneumatics. We propose to give a mere outline of the general laws of pneumatical science — that is, of the mechanics of elastic fluids. For their various practical applications we must refer to articles under which pneumatical machines, such as AIR PUMP, BAROMETER, &c., are specifically treated.

1. THE STATICS OR LAWS OF EQUILIBRIUM OF ELASTIC FLUIDS. These laws are four in number:—

(1.) The LAW of *Boyle or Marriotte*, expressing the relation between the elastic force and volume when the temperature is constant. It is that the pressure of an elastic fluid at a constant temperature, varies inversely as the space which it occupies, and directly as the density. Experimental proof of this can readily be obtained. Thus, let a glass tube A B D, closed at A, be placed with the axis vertical. Pour a little mercury into B D, and let a little air be withdrawn from the tube A B, so that the mercury may stand at the same level P in the two sections. Then pour in *slowly* more mercury into D, until the level reaches, suppose C in the one and M in the other tube. Then if h be the height of the atmospheric column of mercury, the pressure in the tube, A M, will evidently be representable by $h +$ M C; that in the tube A P by h. Now, it will be found by measurement that the space A P bears to the space A M the ratio of $h +$ M C to h. The spaces A M, A P will be best measured by filling them respectively with mercury and comparing the amounts which they respectively contain. We thus find that when air is *more* compressed than the atmospheric air, the law holds. Suppose again a great deal of air withdrawn from the tube, so that the original level P may be very high. If, by means of a siphon, a considerable portion of the mercury be withdrawn —the level in the two arms will fall to M and C respectively. The pressure in A P will evidently vary with h, and that in A M will be h — M C. It will be found by similar measurements that the space A P is to the space A M as h — M C is to h. Hence in the case of *rarefied* air also, and *therefore in all cases*, the pressure, when the temperature is constant, varies directly as the space the elastic fluid occupies. It will vary, therefore, directly with the density. Hence if ϖ represent the pressure at 0°, and ϱ° the density $\varpi = \mu \varrho^{\circ}$, where μ is constant. We have also $\varpi = g\ \sigma\ h$, where σ is the density. By comparing these two, we find from Biot and Dumas' experiments $\sqrt{\mu} = 916{\cdot}188$.

Fig. 1.

Fig. 2.

(2.) The SECOND law is that of *Dalton and Gay Lussac.* This gives the relation between the volume or density and temperature when the elastic force is constant:—If $\sigma_t\ \sigma_0$ be the densities of mercury at $t°$, 0° Centigrade $\sigma_t = \dfrac{\sigma_0}{1+et}$; the density therefore diminishing as the temperature increases. The quantity e is constant, and for Centigrade degrees is ·00018018. Since in all practical cases $e\ t$ is a very small quantity

$$\sigma_t = \frac{\sigma_0}{1+et} = \sigma_0 \left(1 - et\ \ (e\,t)^2 + \&c.\right)$$

$$= \sigma_0 (1 - et), \text{ approximately.}$$

(3.) The THIRD great law is *Amonton's*, which states that to obtain the relation of the elastic force and temperature when the density is constant, we may compound the two laws, which each refer to part of the total phenomenon, into one. Thus:—Let u', τ', Π', be a given volume, temperature and pressure, u, τ, Π be the similar standard quantities; then the volumes of the two gases at temperature 0° under the actual pressures will be

$$\frac{u'}{1+\varepsilon\tau'},\quad \frac{u}{1+\varepsilon\tau} \quad \text{where } \varepsilon = {\cdot}003665\text{; this}$$

number being the expansion of air for one centennal degree. Now these volumes are inversely as the pressures; therefore

$$\frac{\Pi}{\Pi'} = \frac{u'}{1+\varepsilon\tau'} \div \frac{u}{1+\varepsilon\tau}$$

$$\therefore \frac{u'}{u} = \frac{\Pi}{\Pi'} \cdot \frac{1+\varepsilon\tau'}{1+\varepsilon\tau}$$

which expresses the proportion of the volume of a given quantity of air at any given temperature and pressure to that at the standard temperature and pressure.—We have said in the experiments illustrating Boyle's law, that the operations must be slow. This is necessary to allow the heat that is generated on compression and the cold of dilatation to disappear. Let us consider the case of ordinary dilatation and compression. Let Π, τ, ϱ, be temperature, pressure, density, at any given moment,

$\Pi + \delta\,\Pi$, $\tau + d\,\tau$, $\varrho + d\,\varrho$ those immediately after

$$\Pi = \mu\,\varrho\,(1 + \varepsilon\,\tau)$$

$$\Pi \quad \delta\,\Pi = \mu\,(\varrho + \delta\,\varrho)\,(1 + \varepsilon\,\tau + \varepsilon\,d\,\tau)$$

$$d\,\Pi = \mu\,\delta\,\varrho\,(1 + \varepsilon\,\tau) + \mu\,(\varrho + \delta\,\varrho)\,\varepsilon\,\delta\,\tau.$$

Dividing by

$$\Pi = \mu\,\varrho\,(1 + \varepsilon\,\tau)$$

$$\frac{d\,\Pi}{\Pi} = \frac{\delta\,\varrho}{\varrho} + \frac{\varepsilon\,d\,\tau}{\ \ \varepsilon\,\tau} \cdot \frac{\varrho + \delta\,\varrho}{\varrho}.$$

Now where ω is a constant quantity

$$d\tau = \omega\left(\frac{1+\varrho}{\varrho+\delta\varrho}\right) = \frac{\omega\,\delta\varrho}{\varrho+\delta\varrho} \text{ where } \omega$$

is constant, thus giving the expression which connects the sudden changes of temperature and density. Hence

$$\frac{d\,\Pi}{\Pi} = \frac{d\,\varrho}{\varrho} + \frac{\varepsilon\,\omega}{1+\varepsilon\,\tau}\cdot\frac{\delta\,\varrho}{\varrho}$$

$$= \frac{d\,\varrho}{\varrho}\left(1 + \frac{\varepsilon\,\omega}{1+\varepsilon\,\tau}\right).$$

Now it appears from experiment that $\dfrac{1+\varepsilon\,\omega}{1+\varepsilon\,\tau}$ is constant, that is, that ω is proportional to $1+\varepsilon\,\tau$. Call it κ. Then

$$\frac{d\,\Pi}{\Pi} = \kappa\,\frac{d\,\varrho}{\varrho}$$

Integrating $\log_\varepsilon \Pi = \kappa \log_\varepsilon \varrho + C$.

If Π', be any other pressure, ϱ' any other density

$$\log_\varepsilon \Pi' = \kappa \log_\varepsilon \varrho' + C$$

$$\therefore \log_\varepsilon \frac{\Pi'}{\Pi} = \kappa \log_\varepsilon \frac{\varrho'}{\varrho}$$

$$\frac{\Pi'}{\Pi} = \left(\frac{\varrho'}{\varrho}\right)^{\kappa}$$

But we have shown already that

$$\frac{\Pi'}{\Pi} = \frac{\varrho'}{\varrho}\cdot\frac{1+\varepsilon\,\tau'}{1+\varepsilon\,\tau}$$

$$\frac{1+\varepsilon\,\tau'}{1+\varepsilon\,\tau} = \left(\frac{\varrho'}{\varrho}\right)^{\kappa-1}.$$

The probable value for κ is found to be about 1 42.

Let us examine the physical meaning of this quantity κ.—Let τ', ϱ', Π' represent an initial state, add ε in heat, and let τ, ϱ, Π be the new state;

then $\dfrac{\varrho}{\varrho'} = \dfrac{1+\varepsilon\,\tau'}{1+\varepsilon\,\tau}$ because the pressure is unchanged.

Suppose the air now compressed till it comes back to the density ϱ'; the heat evolved will be

$$\omega\left(1 - \frac{\varrho}{\varrho'}\right) = \frac{\varepsilon\,\omega\,(\tau'-\tau)}{1+\varepsilon\,\tau}.$$

Hence the temperature of the air has been increased by $(\tau'-\tau)\left(1 + \dfrac{\varepsilon\,\omega}{1+\varepsilon\,\tau}\right)$.

Under the constant pressure, ε increases the temperature by $\tau'-\tau$. Hence

$$\frac{1+\varepsilon\,\omega}{1+\varepsilon\,\tau} = \frac{\text{specific heat of air under constant pressure.}}{\text{specific heat under constant volume.}}$$

These three laws of Marriotte, Gay Lussac, and Amontons then, express the relations between elastic force, density, and temperature.

(4.) A Fourth Law is required however, to express the relation between the volume and the temperature when the quantity of heat is constant. The experimental result is, that in all cases of sudden rarefaction or condensation, the difference of temperature varies as the cube of the rarefaction. Thus, if ω_1 ω, be the difference of temperature for 1, and δ of rarefaction, respectively —these expressing the proportions of the increase of volume to the original volume, then—

$$\frac{\omega}{\omega_1} = \frac{\delta^3}{1^3}$$

$$\omega = \omega_1\,\delta^3$$

This we may lay down as a fourth law. ω_1 is found experimentally by Professor Potter (*Philosophical Magazine*, September, 1853) to be ·2077.

To find the ratio of the two specific heats spoken of above.

Let c = capacity of air for heat at a constant pressure.

,, = quantity of heat which raises 1 grain of air 1° Fahr.

And Γ = quantity of heat to raise it ε° Fahr.

Then $\Gamma = c\,\varepsilon$.

Let c' = capacity of air for heat at constant volume.

= quantity of heat which raises 1 grain of air 1° Fahr. under constant volume.

Then Γ will raise it higher than ε degrees, because expansion is not here allowed, let it be $\varepsilon + \omega$. Then $\Gamma = c'(\varepsilon+\omega) = c\,\varepsilon$.

$$\therefore \frac{c}{c'} = 1 + \frac{\omega}{\varepsilon}$$

Now if no heat be lost, ω is the temperature which would be lost when expansion takes place down to the original pressure, and

$$\omega = \omega_1\,\delta^3$$

Let v_0 v v' be temperatures at freezing, θ° above freezing, and $(\theta+\varepsilon)$° above freezing, of any gas—

Then $v = v_0\,(1+\alpha\,\theta)$

$$v' = v_0\left(1+\alpha\,(\theta+\varepsilon)\right)$$

$$\therefore \frac{v'}{v} = \frac{1+\alpha\,\theta+\alpha\,\varepsilon}{1+\alpha\,\theta} + \frac{1+\alpha\,\varepsilon}{1+\alpha\,\theta}$$

And δ, the rarefaction

$$= \frac{v'-v}{v} = \frac{v'}{v} - 1 = \frac{\alpha\,\varepsilon}{1+\alpha\,\theta}$$

[Ience $s = \frac{\delta (1 + \alpha \theta)}{\alpha}$

$$\therefore \frac{c}{c'} = 1 + \frac{\omega}{s} = 1 + \frac{\omega_1 \delta^{\cdot 3} \alpha}{\delta (1 + \alpha \theta)}$$

Ience putting $\alpha = \frac{1}{490}$, for Fahr. degrees, = ·3665 for Centigrade, as above noted, and using $\omega_1 + 2077$, we have

$$\frac{c}{c'} + 1 + \frac{\delta^2}{2359} (1 + \alpha \theta).$$

ι formula in which we have varying values for he ratio of specific heat. Where the rarefac-ιon is very small or inversely, the condensa-on $\frac{c}{c'}$ is very nearly equal to 1.—These four ιws constitute what we may call the STATICS of is subject.

II. A few words remain to be said as to the YNAMICS of elastic fluids. In Hydrodynamics, ıe theorem of Torricelli states that molecules of uid in leaving an orifice, issue with a velocity ıe same as if they had fallen freely in the va-uum from a height equal to that of the level ove the centre of the orifice. Assuming this hold for air and gases, Bernouilli deduced the llowing expression :—

$$= \sqrt{2 g \cdot p \cdot \frac{\tilde{\omega}}{\tilde{\omega}'} \cdot (1 + a t) \left(1 - \frac{h'}{h}\right)}$$

here v is the velocity of flow per second, g is ıe expression for gravity, 32·2 in feet; p the ight of the liquid column which measures the ormal pressure of the elastic fluids; $\tilde{\omega}$ $\tilde{\omega}'$ the eights of unit volumes of the liquid which serves standard for measuring the normal pressures the elastic fluids and of the gas at 0° and nder that pressure; a the co-efficient of dilata-on for the gas—t the temperature—h h' the terior and exterior pressures. Introducing d r density of any gas different for air, and sub-tituting the usual constants, we find

$$v = 430 \sqrt{\frac{1 + a t}{d} \left(1 - \frac{h'}{h}\right)}$$

alculating for air at 0°, and at barometric ights near 760 millimetres, which is the stan-rd, we find these results :—

Excess of Pressure h' over 760 mm. = h.	Velocities in metres per second.
2 mm.	20
5	32
10	45
20	64
30	78
40	91
50	101
60	111
100	135
150	160
200	180

As in the case of fluids however, this is not found accurately true. Call v the theoretical velocity, and v' the experimental. Then

$v' = .65\, v$ for orifices in thin wall.
$= .93\, v$ for cylindrical spouts.
$= .94\, v$ for conical, and narrowing from the wall of the gasometer.

The hypothesis about the narrowing of the stream of air after emission which reconciles theory with experiment for fluids, may be repeated here.—We may compare the foregoing velocities with a rough tabular estimate of velocities of wind :—

	Velocity in metres.
Wind just sensible,	1
Moderate,	2
Fresh breeze—extending sails,	6
Wind most suitable for mills,	7
Good breeze,	9
Stiff breeze, requiring topsails to be reefed,	12
Very strong wind,	15
Great tempest,	27
Hurricane,	36
Hurricane which overturns buildings,	45

Bernouilli's formula answers for the case of openings in thin walls, but not well for long tubes. No very accurate theoretical account can be given either for those cases, or where the difference of exterior and interior pressures is great, or for the changes which are to be made for gases different from air.

III. We shall trace a few applications of these principles.

(1.) Let two gases under pressures M, Π, occupy spaces u, v, and let them be allowed to interpermeate—there being no chemical action. Suppose we have to find the volume of the mixtures under the pressure P

$$u \text{ under pressure P becomes } u\ \frac{\text{M}}{\text{P}}$$

$$v \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ v\ .\ \frac{\Pi}{\text{P}}$$

∴ When mixed, under pressure P, we shall have

$$\frac{\text{M}\, u + \Pi v}{\text{P}} \text{ as the total volume.}$$

(2.) Our space does not permit us to repeat the statements regarding vapours, elsewhere fully illustrated. We shall assume them to be known. They hold with regard to all fluids in varying degree. The fundamental principle is that all fluids whose surfaces are open, only cease evaporating when a layer of their own vapour of given amount for each temperature lies above them. Suppose we take the important problem, to discuss the comparative density of dry and moist air under the same temperature and pressure.—Let Π be the total pressure, and Υ that due to aqueous vapours. Then Π — Υ is the pressure of the air: and if ϱ be the density of dry air at

Π, then $\frac{\Pi - \Upsilon}{\Pi} \cdot \varrho$ is the density of the air. The density of vapour under pressure Υ is $\frac{\Upsilon \varrho}{\Pi} \cdot m$, where m is the ratio of specific gravities of air and vapour. Hence the mass, a volume V of the mixture, will be equal to a mass.

$$V \cdot \frac{\Pi - \Upsilon}{\Pi} \cdot \varrho, \text{ of dry air } + \frac{V \cdot \Upsilon}{\Pi} \cdot m \, \partial \text{ of}$$

vapour. But the mass is $V \varrho$; therefore the densities—proportional to the masses—are proportional to $\frac{\Pi - \Upsilon + m\Upsilon}{\Pi}$. It is only necessary to vary the conditions of the question to frame as many questions upon this subject as we choose. In all of them we have merely the application of Boyle's and Dalton's simple temperature law.

(3.) We shall give the process by means of which heights are calculated by means of the barometer, as a last illustration.

Let H, Π, K be the pressures at H, P K, S the temperature at H, T at K; P Q adjacent points, M P $= x$ P Q $= dx$. Then the excess of pressure at Q over that at P is ultimately $d\,\Pi$. We may suppose the temperature between H and K to be uniform and mean between S and T. Hence the density of the air at P is $\frac{\Pi}{\mu} \frac{1}{1 + \frac{1}{2} \epsilon} (s + \tau)$.

But the excess of pressure at

$$P = g \cdot PQ \cdot \frac{\Pi}{\mu} \frac{1}{1 + \frac{\epsilon}{2}} (s + \tau)$$

$$= -g \frac{\Pi}{\mu} \frac{dx}{1 + \frac{\epsilon}{2}} (s + \tau)$$

$$\therefore \frac{d\,\Pi}{\Pi} = -\frac{g}{\mu} \cdot \frac{dx}{1 + \frac{\epsilon}{2}} (s + \tau)$$

$$\therefore \log_\epsilon \Pi = -\frac{g x}{\mu \left(1 + \frac{\epsilon}{2} (s + \tau)\right)} + C$$

Hence for $x + 0$, $x = h$, where $h =$ H K

$$\log_\epsilon K = -\frac{g h}{\mu \left(1 + \frac{\epsilon}{2} (s + \tau)\right)} + C$$

$$\log_\epsilon H = 0 + C$$

$$\therefore \log_\epsilon \frac{H}{K} = \frac{g h}{\mu \left(1 + \frac{\epsilon}{2} (s + \tau)\right)}$$

which, since H, K are given by the barometer, will give h, s, and τ being known by the thermometer.

Points. Had space permitted, we should have discoursed here at some length on the subject indicated under INVOLUTION. The reader, however, must be referred to RATIO ANHARMONIC. The relations of systems of points now forms an important part in the modern Geometry. See Chasles and Mulcahy.

Polar Co-ordinates. A system of reference for positions and magnitudes which, as of the highest importance in analytics, we give here. Suppose a point O, and a line O A be taken as fixed. Then one refers any point P, to O A and O by specifying O P $= r$, P O A $= \theta$. For several lines in the same plane that have previously been referred to co-ordinate axes O X, O Y, we have, if X O A $= \alpha$, $x = r \cos (\theta - \alpha)$, $y = r \sin (\theta - \alpha)$.—Where we have points in space referred to polar co-ordinates—suppose, for instance, O X, O Y, O Z to be the ordinary lines of reference. Then the polar quantities for the point P are O P $= r$, Z O P $= \theta$, and if a plane be passed through Z O, O P, its line of section with O X y, makes an angle, X O M, which is the other polar dictum $= \phi$. It is easy to show that $z = \alpha \cos \theta$, $y = r \sin \theta \sin \phi$, $x = r \sin \theta \cos \phi$.

Polar Forces. There are two classes of Forces at the root of Natural Phenomena, of which we have now a distinct idea. The first, *simple* Forces such as Impulse or Gravity; forces that impress one single and simple effect. The *second* class is much more complex; and the rise of clear conceptions regarding them may be said to be coincident with the rise and development of all modern Physical Science. These second forces are termed *Polar Forces;* and their characteristic is this;—they are developed and act *in pairs.* No force belonging to this class can act or even exist singly: for instance, one Magnetic Force cannot be developed without the simultaneous development of its opposite: so likewise with Electricity; there is no such thing as an Electric attraction unaccompanied by a corresponding or equal repulsion, nor can a voltaic current be developed without an accompanying *opposite* or *polar* current. These Polar Forces govern nearly the entire domain of Physics. To them must be referred all phenomena of Magnetism, Electricity, and Chemical Action; nor does their influence stop with the limits of these sciences:—their pure application to the phenomena of Light, will yet suffice to disembarrass this branch of physics of the hypotheses that now encumber it.—One of the most clamant desideranda of our Time, is a pure *Principia* of Polar Forces. Existing modes of treating them are only tentative,—consisting of expedients suggested by actual difficulties. System and adequate Methods will assuredly and duly come; but they must bring along with them an entire remodelling of our Analytical Sciences.—See next article.

Polariscope: Polarimeter. The name given to instruments meant to discover and estimate Polarized Light. See POLARIZATION. Every such instrument consists of a *Polarizer* and an *Analyzer*; the former polarizes the ray, the latter discerns that it is polarized, and enables

s to trace its characteristics. Of course the means re various. The polarizer may be a reflecting late,—a bundle of transparent plates—a plate such a crystal as the tourmaline—or a doubly efracting rhomb. The analyzer, again, is generally another reflecting plate, capable of being laced in any azimuth and inclination to the lane of the polarized ray,—a second plate of ourmaline,—or another doubly reflecting crysal—say a *Nicol's prism* (*q. v.*) The details of ie instruments greatly differ, as well as their xpense. Some of them, for ordinary use—ufficient to show the superb colours developed y the polarized ray—are simple and cheap; ut the best instruments cannot be obtained at low price. Their forms and prices may now e seen in any catalogue of optical instruments. he student is again referred to Sir David Brewer's *Optics*; and *inter alia*, to recent *Memoirs* r Dové.

Polarity. The conception of Polarity in rerence to Natural Phenomena is, in its purity, of rth so recent that it may well be questioned hether it has yet attained to due distinctness, id been fully disembarrassed of accessary conlerations, in the minds of many inquirers. Sugsted originally by special facts—*e.g.*, the phenoena of Magnetism—it long retained, connected ith it and apparently inseparable, the notion of OLES, or distinct seats of action; and, more or ss, it has been encumbered ever since, in all the partments of physics of which it constitutes an sential part, with different and limited hypoetical subsumptions. Separated from everything ctitious, the idea of Polarity is simply that *of ual and opposite powers called into existence a common condition*; or, as Dr. Whewell exesses it still more generally, it is a condition der which we have a *contrast of properties rresponding to a contrast of positions.* Rerded in this light, the idea of Polarity is as rely *rational* as the Laws of Motion, and may equally made the foundation of a *Rational namics.* It has nothing to do with the fancy Terrestrial Magnets, with Hypotheses concern- impalpable and incognoscible Fluids, with oms having Poles, or with doctrines concerning hereal vibrations. Hypotheses like these are *impure* part of our modern physics, although y have, doubtless, often assisted the Inquirer; l they are clearly quite separable from the ion of Polarity in itself,—the notion which really given these hypotheses by far the ger portion of the efficacy they possess.—The nain of the idea of Polarity, discerned simply l abstractly, stretches very far. It includes the ire phenomena of Magnetism, of Electricity, of emical Affinity, of Crystallization, and the jor part of the phenomena of Light. In these ious departments of physics it manifests itself various ways,—ways, however, not necessarily erse. In fact, most of the grandest discoveries ecent times, lie in the direction of establishing what Dr. Whewell has happily called the *connection of Polarities*; by which is meant the discernment of a correlation among different manifestations of Polar Forces. It is in this research that Ampere achieved his signal triumph. That a connection exists between the Magnetic and Electric Polarities, could no longer be doubted after the brilliant experiment by Oersted; but Ampere determined the nature of that connection, laid down its fundamental laws, and showed how to deduce its inevitable results. The essay of Ampere was the earliest, and may still be termed the only *rational* attempt to deal largely with Polar Forces. Hence, he has not unworthily been termed the Newton of Magnetism and Electricity.—In the essays of our illustrious Faraday, there are endless materials and suggestions of inappreciable value, towards the pursuit of the same difficult course. Nor can we omit notice of the remarkable contributions of Professor William Thomson. Nevertheless there is a great void. General methods are wanting; and to supply these our modern geometry and our analytical resources must be stirred to their depths.—For further illustrations see POLARIZATION.

Polarization. A name applied to a class of very peculiar phenomena, the nature of which is described in the articles below.

Polarization of Heat. Radiant Heat appears subject to all the changes incident to Radiant Light: it is reflected, refracted, doubly refracted, and *polarized.* Likewise something akin to Polarization of Heat by the Atmosphere, seems to exist. We do not as yet know anything similar to coloured Polarization, in the case of Radiant Heat; although the discoveries of Melloni indicate that a Ray of ordinary Heat is—in analogy with a sunbeam—rather a sheaf of diverse rays. Revelations on this difficult and delicate subject, will doubtless be portion of the harvest of the Future.

Polarization of Light. Under this term is comprehended a variety of phenomena of a singular description which are presented by a Ray of Light under certain circumstances. The subject is now so extensive and complex, that it were vain to attempt a complete survey of it within the limits of this Cyclopædia: we shall essay to sketch merely its leading features under appropriate heads.

I. THE SIGNIFICATION OF THE TERM POLARIZATION.—The adoption of this name has unquestionably been facilitated by certain physical theories concerning the *cause* of the phenomena it indicates. But the primary fact which the term marks out and distinguishes, may be presented and the name justified, apart from all physical theory or hypothetical substratum. That fact, in its simple purity, is the following:—under certain circumstances, a ray of ordinary or indeed of *any* Light, appears to assume what may be designated as *sides*, and, therefore, to

present *opposite* or *polar* properties according as these *sides* happen to lie towards the observer. For instance, no solid body except a sphere, will appear of the same uniform shape, wherever the observer's eye is placed;—a flat scale, looked at edgeways, is a thin line; looked at sideways, it is a flat parallelogram. The word *side*, as at present employed, is not intended to involve any theoretical notion whatsoever: it is used only as a convenient expression by which the existence of opposite or polar properties in the Rays of Light, may be indicated.—But the subject will become clearer, if, from general illustration, we descend to the actual facts of the case. There is a crystal called the Tourmaline, dark but still transparent, especially in thin plates. If a direct ray of ordinary Light is looked at through a thin plate of Tourmaline which has been suitably cut, it will appear rigorously of the same colour and brilliancy, *whatever the position of the Tourmaline.* But with a ray, affected by various circumstances, this by no means holds. Should the observer look through the Tourmaline plate, not at a *direct* ray, but a *reflected* one,—say reflected from the surface of a sheet of polished glass—he will find (at certain angles of reflection), that, while on the Tourmaline being held in one position the whole reflected ray is transmitted, the very reverse takes place when the crystalline plate is turned round ninety degrees:—*i.e., no light whatever is then transmitted.* By that act of reflexion then, the ray of Light has been endowed with a certain determinate peculiarity. It passes through the Tourmaline plate, if that plate is held in one position; but if the plate be turned round ninety degrees the ray cannot at all pass through it. Instead of being as formerly *indifferent* to the position of the Tourmaline, the Ray now manifests *opposite* qualities in its relations to that plate: it seems to have obtained *sides*, or a polar character; *i.e.*, it has been POLARIZED.—One other fact will enable the student to fix this curious change more firmly in his mind. It is well known that certain crystals divide a ray of Light that passes through them, into two distinct parts; and this not through any dispersive efficacy, because the two rays that issue continue *white* if the intrant light were white. Of these crystals Iceland Spar is an available instance. Place a rhomb of that spar over a clearly marked point, and two images of the point will usually appear,—*i.e.*, the ray issuing from the point has been divided by the spar into two parts or rays pursuing two distinct directions. Now, if these two rays are looked at through a Tourmaline plate, it will be found universally that one certain position of the plate extinguishes or refuses transit to one ray, while it fully transmits the other ray, and *vice versa*. These two rays therefore, have properties with regard to the Tourmaline quite as opposite as the two *poles* of a *Magnet:* in the true and only pure sense of the word they are *oppositely Polarized;* and as it is expressed technically, they are polarized in planes at right angles to each other.

II. CIRCUMSTANCES UNDER WHICH A RAY OF LIGHT IS POLARIZED.—These circumstances are mainly the following:—

(1.) *Polarization by Doubly Refracting Crystals.*—This, as being the most effective mode of polarizing Light over which the physicist at present has command, is mentioned first. The phenomena of Double Refraction are very various, and are explained under REFRACTION. Two phenomena alone, require description here; and these may be discerned by any student in possession of two good Rhombs of *Iceland Spar.* Let the two images—either of a black or of a bright point—seen through one Rhomb, be looked at through another whose position with regard to the first Rhomb may be altered at will. Several important changes in respect of these two images occur as the second Rhomb is turned round. Generally speaking, one finds four images presented to the eye, endowed with different intensities of Light. But when the two Rhombs are placed in certain relations there are only *two images.* If, for instance, the second Rhomb is placed with its principal plane parallel to the principal plane of the first Rhomb, that ray which was ordinarily refracted before is only ordinarily refracted again (*i.e.*, according to Snell's Law); while the other ray, extraordinarily refracted before, is again extraordinarily refracted. Reverse the positions of the Rhombs—that is, place their principal planes at right angles to each other—and the reverse takes place. The ordinary ray through the first Rhomb is now extraordinarily refracted as it passes through the second; while the extraordinary ray of the first is now refracted in strictest accordance with *Snell's Law* Several important and direct inferences are sustained by this phenomenon. *First*, it cannot be doubted that—as by the Tourmaline, so by the new test of the second Rhomb—it is established that these two rays issuing from the doubly refracting substance, are, in the most strict sense of the word, *polarized;* and, still further, that these planes of polarization are at right angles to each other.—*Secondly*, the second Rhomb—over whose positions we may have the completest command—serves as a ready and unquestionable *analyzer* of the Light transmitted through the first; *i.e.*, it is as capable as the Tourmaline to inform us, whether, and in what plane, any ray is polarized.—*Thirdly*, the close relation between these singular phenomena, and the axes of crystals—whether manifested by the Tourmaline, or by the action of the Iceland Rhomb—induces us to connect them with crystalline structure, or with the *elasticities* that prevail in various directions within crystalline minerals. These varying elasticities cannot but influence the character of *any mode of propagation* through them.—It may be mentioned, *Lastly*, that while we obtain, through effect of the Iceland Spar, a complete, entire, and entirely opposite

›olarization of Rays, the separation of these Rays nay be made so pure and complete by the thick-ıess of the crystal, that, as already stated, no ·gency need be expected more capable of bringing ınder the power of the experimenting physicist, ͨays of Light in these remarkable and opposite tates.—See PRISM, NICOL'S.

(2.) *Polarization by Reflection.*—It has been xplained above, that a Ray of Light reflected :om many surfaces is found to have undergone n important change. That this change be com-·lete, or rather that it reach its maximum, the rdinary Ray must fall on the reflecting surface t an angle depending on the nature of that sur-ıce. The discovery of the Law which determines ıe amount of this angle is due to Sir David ·rewster. The Law is this:—*The index of Re-·action characterizing the reflecting substance is ıe tangent of the angle of maximum polarization.* ·rom which it follows, that the refracted position of the incident light pursues a path at right angles to the reflected polarized parts. On reaching the second surface of bodies, a second reflection takes place, and a new polarization, of which the law is the following:—*The polarizing angle at the second surface is equal to the complement of the polarizing angle at the first surface, or to the angle of refraction.*—There are two special points demanding notice. 1. *The different rays of the spectrum have different angles of polarization.* When homogeneous light is used—say the red or yellow ray—the polarization may be made complete,—*i.e.*, no light whatever will pass through the analyzing plate of Tourmaline: but when white or common light is employed, the polarization is never complete. The following table, given by Sir David Brewster, shows the amount of this difference in the case of the various simple rays:—

		Index of Refraction.	Maximum Polarizing Angle.	Differences between the greatest and least Polarizing Angles.
Water,	Red rays,..................	1·330	53° 4′	
	Mean rays,.................	1·336	53 11	15′
	Violet rays,...............	1·342	53 19	
Plate glass,	Red rays,..................	1·515	56 34	
	Mean rays,.................	1·525	56 45	21′
	Violet rays,...............	1·535	56 55	
Oil of cassia,	Red rays,..................	1 597	57 57	
	Mean rays,.................	1·642	58 40	1° 24′
	Violet rays,...............	1·687	59 21	

—Light is always polarized in *degree*, al-ough the incident ray does not fall on the re-cting surface at the maximum angle. And ıat is very peculiar, it can be polarized to its ıaximum, in that case, by *successive reflections.* ıe following table, also given by Sir David ·ewster, exhibits the number of reflections re-ıired to effect maximum polarization at differ-t angles of incidence:—

BELOW THE POLARIZING ANGLE.		ABOVE THE POLARIZING ANGLE.	
No. of Reflections.	Angle at which the Light is Polarized.	No. of Reflections.	Angle at which the Light is Polarized.
1	56° 45′	1	56° 45′
2	50 26	2	62 30
3	46 30	3	65 33
4	43 51	4	67 33
5	41 43	5	69 1
6	40 0	6	70 9
7	38 33	7	71 5
8	37 20	8	71 51

ıe important question here is—what is the con-·ion of this partially polarized ray? Is that ʳ a compound of a ray perfectly polarized, and a beam of ordinary light? Or is it a compound two or more rays perfectly polarized, but so ·clined to each other that in no case can perfectly polarized light be detected by the Tourmaline? Further notice of this question, in connection with the phenomena of *metallic reflexion* will be found elsewhere in our Dictionary. See especially REFLEXION.

(3.) *Polarization by Simple Refraction.*—That light is polarized by *refraction* may be said to have been ascertained by Huyghens. He examined indeed only the two rays, or the divided ray, through Iceland spar: nevertheless it was his observation that induced Newton to say, "This fact implies, in the *sides* of the ray, a faculty of disposition having relations of correspondence with, or a sympathy, correlative dispositions in the crystal: it is thus that the poles of two magnets mutually correspond." About the year 1811, however, it was discovered through independent observation by Sir David Brewster, Malus, and Biot, that the peculiar change impressed on an ordinary ray of light by *reflexion*, can also be impressed on it by *simple refraction.* We cannot do better, in explanation of this mode of polarizing light, and its laws, than reproduce a few pages from Sir David Brewster's recent and almost exhaustive work on *Phenomenal Optics*:—

"To explain this property of light, let R *r*, fig. 1, be a beam of light incident at a great

angle between 80° and 90° on a horizontal plate of glass, No. 1; a portion of it will be reflected

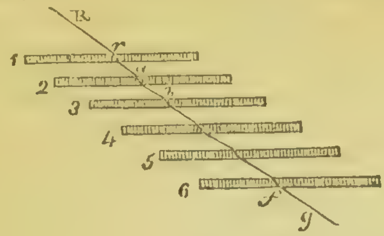


Fig. 1.

at its two surfaces, r and a, and the refracted beam a is found to contain a small portion of polarized light.—If this beam a again falls upon a second plate, No. 2, parallel to the first, it will suffer two reflections; and the refracted pencil b will contain more polarized light than a. In

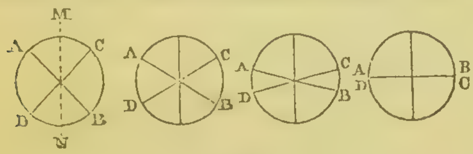


Fig. 2.

like manner, by transmitting it through the plates, Nos. 3, 4, 5, and 6, the last refracted pencil, $f\,g$, will be found to consist entirely, so far as the eye can judge, of polarized light. But, what is very interesting, the beam $f\ g$ is not polarized in the plane of refraction or reflection, but in a plane at right angles to it; that is, its plane of polarization is not represented by the ordinary ray in Iceland spar, or as light polarized by reflection, but by the extraordinary ray in Iceland spar. From a great number of experiments, I found that the light of a wax candle at the distance of ten or twelve feet was polarized at the following angles, by the following number of plates of crown glass :—

No. of Plates of Crown Glass.	Observed Angles at which the Pencil is polarized.	No. of Plates of Crown Glass.	Observed Angles at which the Pencil is polarized.
8	79° 11′	27	57° 10′
12	74 0	31	53 28
16	69 4	35	50 5
21	63 21	41	45 35
24	60 8	47	41 41

It follows from the above experiments, that if we divide the number 41·84 by any number of crown glass plates, we shall have the tangent of the angle at which the beam is polarized by that number.—Hence it is obvious that the power of polarizing the refracted light increases with the angle of incidence, being nothing or a minimum at a perpendicular incidence, or 0°, and the greatest possible or a maximum at 90° of incidence. I found likewise, by various experiments, that the power of polarizing the light at any given angle increased with the refractive power of the body, and consequently that a smaller number of plates of a highly refracting body was necessary than of a refracting body of low power, the angle of incidence being the same.—As Malus, Biot, and Arago considered the beams a, b, &c., before they were completely polarized, as *partially polarized*, and as consisting of a portion of polarized and a portion of unpolarized light; so, on the other hand, I concluded from the following reasoning that the *unpolarized* light had suffered a physical change, which made it approach to the state of complete polarization. For since sixteen plates are required to polarize completely a beam of light incident at an angle of 69°, it is clear that eight plates will not polarize the whole beam at the same angle, but will leave a portion *unpolarized.* Now, if this portion were absolutely unpolarized like common light, it would require to pass through other sixteen plates, at an angle of 69°, in order to be completely polarized; but the truth is, that it requires to pass through only eight plates to be completely polarized. Hence I conclude that the beam has been nearly half polarized by the first eight plates, and the polarization completed by the other eight. This conclusion, though rejected by both the French and English philosophers, is capable of rigid demonstration, as will appear from the following observations:—In order to determine the change which *refraction* produced in the plane of polarization of a polarized ray, I used prisms and plates of glass, plates of water, and a plate of a highly refractive metalline glass; and I found that a refracting surface produced the greatest change at the most oblique incidence, or that of 90°; and that the change gradually diminished to a perpendicular incidence, or 0°, where it was nothing. I found also that the greatest effect produced by a single plate of glass was about 16° 39′, at an angle of 86°; that it was 3° 5′ at an angle of 55°, 1° 12′ at an angle of 35°, and 0° at an angle of 0°.—A beam of common light, therefore constituted as in fig. 2, No. 1, with each of its planes A B, C D inclined 45° to the plane of refraction, will have these planes opened 16° 39′ each, by one plate of glass at an incidence of 86°; that is, their inclination, in place of 90° will be 123° 18′ as in No. 2. By the action of the other two or three plates they will be opened wider, as in No. 3; and by seven or eight plates they will be opened to near 180°, or so that A B, C D nearly coincide as in No. 4, so as to form a single polarized beam, whose plane of polarization is perpendicular to the plane of refraction. I have shown, in another place, that these planes can never be brought into mathematical coincidence by any number of refractions; but they approach so near to it that the pencil is, to all appearance, completely polarized with lights of ordinary strength. All the light polarized by refraction is only partially polarized, and it has the same properties as that which is partially polarized by reflection. A certain portion of the light of a beam thus partially polarized, will disappear when reflected at the polaris-

g angle; and this quantity, which I have ewhere shown how to calculate, is given in e following table for a *single surface* of glass, ıose index of refraction is 1·525 :—

Angle of ıcidence.	Inclination of the Planes of Polarization A B C D, fig. 2.	Quantity of transmitted Rays out of 1000.	Quantity of Polarized Rays out of 6000
0°	90° 0′	956·77	0
20	90 26	956·59	7·22
40	92 0	950·90	32·2
56 45′	94 58	920·5	79 5
70	98 56	837·33	129 8
80 40′	104 55	608 3	156·7
85	108 44	383.72	123·7
90	112 58	0	0

" Although the quantity of light polarized by raction, as given in the last column of this le, is calculated by a formula essentially erent from that by which the quantity of ıt polarized by reflection was calculated; yet s curious to see that the two quantities are cisely equal. Hence we obtain the following :—*When a ray of common light is reflected refracted by any surface, the quantity of light ırized by refraction is exactly equal to that ırized by reflection.*—This law is not at all licable to plates, as it appeared to be from experiments of M. Arago.—When the pre- ing method of analysis is applied to the light ected by the second surface of plates, we ob- the following curious law:—*A pencil of t reflected from the second surfaces of trans- ent plates, and reaching the eye after two re- tions and an intermediate reflection, contains ll angles of incidence, from 0° to the maxi- ı polarizing angle, a portion of light polar- in the plane of reflection. Above the polar- angle, the part of the pencil polarized in the e of reflection diminishes, till the incidence mes 78° 7′ in glass, when it disappears, and whole pencil has the character of common . Above this last angle the pencil contains a ıtity of light polarized perpendicularly to the e of reflection, which increases to a maximum, then diminishes to nothing at 90°.*—As a lle of glass plates acts upon light, and polar- it as effectually as reflection from the sur- of glass at the polarizing angle, we may titute a bundle of glass plates in place of s of glass. Thus, if A, fig. 3, is a bundle of

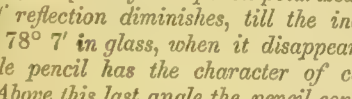


Fig. 3.

plates which polarizes the transmitted ray hen if the second bundle B, is placed as in igure, with the planes of refraction of its s parallel to the planes of refraction of plates of A, the ray *s t* will penetrate the d bundle; and if *s t* is incident on B at polarizing angle, not a ray of it will be reflected by the plates of B. If B is now turned round its axis, the transmitted light *v w* will gradually diminish, and more and more light will be reflected by the plates of the bundle, till, after a rotation of 90°, the ray *v w* will disappear, and all the light will be reflected. By continuing to turn round B, the ray *v w* will reappear, and reach its maximum brightness at 180°, its minimum at 270°, and its maximum at 0°, after having made one complete revolution."

(4.) Among other modes by which polarization may be effected, we may just refer the action of certain crystal plates, such as plates of tourmaline and agate, and the phenomena of what has been termed *Lamellar Polarization.* The student must consult the essay by Biot, and Sir David Brewster's researches.

III. General View of the Theory of Polarization.—When Sir Isaac Newton first became aware of the existence of phenomena like those of polarization, he considered it a conclusive argument against the conception which had been powerfully advocated even in his time, that the propagation of light is due to the propagation of waves. Nor can it be doubted that the existence of *sides* in a ray, is irreconcileable with propagation by waves akin to the waves of *sound.* These latter waves or undulations take place *along the line of propagation*—that is to say, the vibrating or oscillating molecule of air moves to and fro in the direction in which the waves of sound pass; and one cannot discern the possibility of impressing on such motions any change or modification that would cause them possess *sides*, or appear different whether looked at from above or from below, from one side or another. No such change, indeed, is conceivable, unless the vibrations take place athwart the line of general propagation, or are Transverse; and therefore rather resemble the vibrations that pass from one end to another of a musical string. But the general conception of the undulatory theory of light, places no restriction whatsoever on the nature of the waves it subsumes; this on the contrary, has to be discerned through phenomena. And although it is not to be disguised that difficulties still attend the application of that theory in some cases—difficulties, however, mostly appertaining to the inadequacy, or rather intractability of our transcendental analysis—the assertion may safely be hazarded, that, on the ground of certain subsumptions regarding the nature of these vibrations, which in no case can be refused, as mechanically or abstractly inadmissible, a fabric of deductive science has been reared, which, measured by its power not merely to explain but to predict phenomena, has not at present any superior unless it be the subject of Celestial Dynamics itself.—To obtain clear possession of this remarkable theory the student must, in the first place, reach a distinct idea of the nature of a *transversal vibration.* In the ether, whose waves give rise to the onward propagation

and to all the molecular phenomena of light, the molecules are supposed to oscillate *across the onward path of the wave.* As already said, this onward motion is propagated somewhat in the way in which vibrations proceed along an elastic string held in high tension. Now, so soon as this conception is exactly attained, it will be clear that no restriction is thereby placed on the manner of the motion of each or of all the molecules, save the very general one that each molecule must vibrate in a plane at right angles to the direction of the propagation of the ray. Consistently with this only fundamental restriction, the molecules may describe various curves in such a plane; they may all describe one curve, or each may describe its own; or they may describe no curve at all but each oscillate to and fro in a straight line, athwart the direction of the ray. It will be seen on a glance that as every such modification of oscillation must carry with it mechanical consequences—altering the relations of a luminous ray to the bodies through which it passes—how fertile is the undulatory theory at its foundation; and how it may be capable of reducing within law, numerous varied and even apparently conflicting classes of phenomena.—Let us briefly and very generally explain the leading modes of molecular oscillation—of which at present that theory takes account.

(1.) *The case of a Ray of Ordinary Light.*—The most probable form of such wave-oscillations around the axis of a ray and in the plane perpendicular to it, is when the molecule describes an ellipse, and when the major axis of these various ellipses do not preserve one invariable direction, but shift round and round like the apsides of the planets. There is nothing whatever in such a wave to impress on it *sides*, or to cause it manifest polarities, under whatever mode it is viewed or by whatever test it is examined. Such a ray will be subject to the ordinary influences of *reflexion* and *refraction*; it will, under suitable circumstances, manifest the phenomena to be expected from *interference;* and if it is made up of sets of molecules moving with different velocities, we would expect also the phenomena of *dispersion.* It is not to be supposed, however, that in passing through various bodies, or on being subjected to various influences, such a system of vibrations would escape without serious and often clearly distinguishable modifications.

(2.) *Plane Polarized Light.*—It is easy to conceive, in the first instance, that in passing through certain media, or being subjected, say to certain reflexions—the liberty of these molecules to oscillate in a curve is impaired—in other words, that one side of their motion is enfeebled or even destroyed; so that the wave comes to consist of rectilineal motions in some plane perpendicular to the axis of the ray. Or it may farther be conceived that on the curvilineal oscillation entering a medium, it is divided into *two* sets of rectilineal oscillations perpendicular to each other as well as to the direction of the ray, and propagated along different paths. In the former simple case we should have the ordinary ray converted into one ray *with sides;* and in the latter case we should have what occurs in double refraction, the ordinary ray emerging in the form of two distinct rays oppositely polarized. The student will observe two important truths connected with this remarkable but very simple theory. *First*, the view it gives, closely appertains to the molecular structure of the bodies which are supposed to impress such changes on the oscillations constituting the waves of light; and we should expect accordingly, as has turned out, to find the phenomena of polarization by aid of crystals, indissolubly connected with the axes of elasticity of these bodies. *Secondly*, the entire subject is thus converted into a mechanical one; and we are entitled to demand of the theory a full and *à priori deduction* of those various and fundamental empirical laws of its phenomena, whose discovery so distinguished the career of Sir David Brewster. And a reply most adequate has been given, in the first place, by the masterly analysis of Fresnel. An outline of that analysis, or rather of the march of its results has long been before the English inquirer in the most lucid work by Professor Lloyd, whose illustrations are repeated in the *Repertoire* of Abbé Moiguo; and we observe with pleasure that Professor Powell, to whom the theory of light in many respects owes so much, has recently undertaken a fresh review of the subject, part of which he has already communicated to the *Philosophical Magazine.* With a reference to these several works, we must in the meantime be content to rest; only remarking further, that the theory has accomplished more than offering a ground for existing laws; it has gone a-head of discovery, and been ever prepared to grasp and resolve new unexpected facts.

(3.) *Circular Polarization.*—The student has now to be introduced to another set of phenomena. The modification impressed upon a ray of light by the mere suppression of one of the axes of the ordinary elliptic wave, or its reduction to the state of a *plane wave*, is evidently only one of the changes to which we may suppose a set of oscillations of such a kind to be subject. It is easy to conceive that, through effect of some other descriptions of action, the axes of the ellipses may become *equal*—or that the ellipse is changed into a *circle.* When the vibration assumes this form, the ray is said to be *circularly polarized.* It is clear that to such a ray peculiar properties must belong, distinguishing it very broadly from common Light as well as from plane polarized Light. According to the rigid significance of the word *polarity*, that term can evidently not be applied to such a ray except in one limited and special sense: a wave so propagated can have no *sides*, unless in reference to

the directions in which the particles of the ether are moving—that is, whether they are moving in the direction of the hands of a watch, or in the contrary direction, constituting the distinction of a *left-handed* and a *right-handed* circular polarization. The mode of discriminating between Light in this condition, and Light in any other condition, has been already described under CIRCULAR POLARIZATION, so that we shall not recur to that subject. It is right, however, to allude once more to the fundamental truth on which Fresnel rested his memorable theory of Circularly Polarized Light, and from which he deduced the necessity, according to the Undulating Theory, of a large and most peculiar class of phenomena that might have seemed utterly puzzling. The Truth or Law in question is this: *If two rays of plane polarized light, whose planes of polarization are at right angles to each other, meet, or are superimposed,* IN PHASES DIFFERING FROM EACH OTHER BY ONE-FOURTH OF A VIBRATION, *a circular oscillation immediately results, or the wave is circularly polarized.*—Two things must be evident. *First,* if this law be true, the theory of the phenomena of *circular polarization,* must be at once reducible to the theory of *plane polarization,* because every circular wave must comport itself in all circumstances, as two plane waves in the foregoing relative conditions, inasmuch as it may always be *resolved* into the elements of which it is *compounded.* And, *secondly,* the law itself must be susceptible of mathematical demonstration, for it involves nothing other than a question of the composition of definite motions. It is confessedly difficult, however, for the student to *conceive* how a combination of two such *rectilineal* motions must produce a *circular* one; on which account, we esteem the whole subject largely indebted to Professor Powell for the following illustrative mechanical device. Let an arm F attached to any

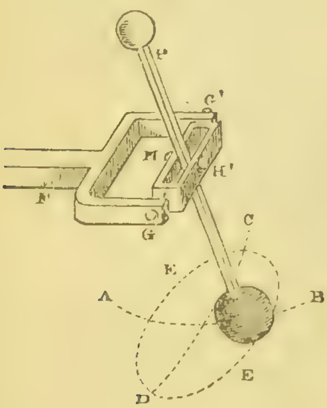


Fig. 4.

convenient support terminate in two branches, in which, by the points G G′, a small frame swings: in this frame, by the points H H′, whose axis is at right angles to G G′, a pendulum P, with a weight at the lower end, can vibrate. Now, on the pivots H H′, the pendulum can evidently only vibrate in the plane of C D: and by the pivots G G′ it can only vibrate in the plane of A B, at right angles to the former. But if motion be communicated to it in one of these planes, and, at *an instant after,* in the other also, the result of the compound motion in this discordance, will be that it vibrate in a CIRCLE, if the interval between giving the two impulses be exactly one quarter the time of its vibration in the first plane. If the interval be different, the pendulum will vibrate in an ELLIPSE.—The circular vibration or oscillation, depends, therefore, essentially *on the phases of vibration* that are combined.

(4). *Elliptical Polarization.*—One other probable modification of the ordinary ray of light remains to be noticed. The waves may be neither circular nor plane, but *elliptical,* only with this modification distinguishing them from ordinary light—there may be no apsidal motion, in other words, the two axes of the elliptic wave may continue *in the same planes.* In case of such a wave, there must clearly be a polarity of two kinds: first, the two sides of the wave will not be the same; and, secondly, as in the former case, there may be a right-handed and a left-handed revolution or oscillation of the ethereal particles. The treatment of this new phase of the subject is necessarily much more complex than that of the former; but the remark made at the conclusion of last section indicates the scientific key,—*Elliptical,* as well as *Circular* Polarization, is the result of the combination or composition of two rectangular plane waves, *in different phases of vibration.* The reflecting student will not fail to remark, that notwithstanding the order in which we have arranged these different polarizations, the form or mode last mentioned, is that which every consideration of probability, would induce us to expect to find most frequently in Nature. Vibration in a single plane, and vibration in a pure circle, ought to be about as rare an occurrence as a perfectly circular orbit amid planetary motions. They are but the incidents, the *exceptions;* while Elliptical Polarization, as the result of circumstances so much more likely to occur, ought to be found to be the *rule.* As minuter discovery advances, this fact will doubtless become generally descried; nor even now is there want of sufficient ground for the foregoing assertion, as we shall show abundantly under REFLEXION and REFRACTION.

(5). Lastly, one other change may be supposed to be impressed on a polarized ray of light,—let us say, for purposes of distinctness, a ray plane polarized. Is it not possible that such a ray may, under certain circumstances, constantly *change its plane?* Or, that instead of proceeding onwards like a straight flat rod, it may, as it passes through certain media, take on the form of a flat

rod, twisted like a cork-screw,—or into the shape of a spiral staircase? The extraordinary phenomenon now referred to is a real one, and seems reducible to the foregoing geometrical conditions. It is the phenomenon termed *Rotatory Polarization*, or, as is preferable, *the Rotation of the Plane of Polarization*. It is clear that a change of this sort is altogether distinguishable from *Circular Polarization*, although some writers have chosen to treat it under that head,—nor is it to be denied that by an analytical artifice the two changes may in so far be assimilated. The essential distinction is this: in Rotatory Polarization the vibrations are plane, but the plane of the wave has become a twisted surface; while the vibrations are themselves circular in the other case, and there is no plane of polarization. The facts that led to a suspicion of the existence of such a change, were, we believe, first noticed by M. Arago. The subject has been cultivated by many distinguished inquirers: but it is due to M. Biot to distinguish him from all others; for to him is owing that continuous and successful series of efforts to convert the phenomena now referred to, into a key to the molecular or chemical constitution of solutions. See especially his *Memoir* reprinted in the Messrs. Taylor's *Repertory*.

Such, in outline, the Theoretical view taken by the Undulatory Hypothesis, of the immense, complex, and distinctive class of phenomena, ranged under the term *Polarized Light*. That, in our present condition of knowledge, this theory is encumbered and obstructed by difficulties, may well be conceded: the Modern Analysis with all its grasp is unequal to the requirements of the problems that ever and anon come up; and over the intimate or molecular causes of the changes supposed to be impressed upon the mode of vibration, a profound obscurity—in some cases probably impenetrable—still hangs. But notwithstanding these difficulties, how wonderful its triumphs, and effected by how simple means! The foregoing few hypotheses have been capable of grasping and grouping a multitude of phenomena such as never have been brought within the power of any individual Principle since the discovery of the Law of Gravitation. If it must be acknowledged, that, even in presence of these triumphs, an air of unsubstantiality continues to overshadow these curious and ethereal subsumptions—a degree of artificiality not altogether agreeable in solid physical science; no doubt need be entertained that, however hypothetical their form, they must be the precursors of mighty and unshakeable Truths. Even when we shall have entered the Temple, we shall assuredly never forget the services that conducted us to its Threshold.

IV. Evolution of Colours by the Treatment of Polarized Light.—The subject on which we now briefly enter may be termed in one respect a special one—*i. e.*, its results are no part of the general theory of polarization, they merely follow from it; but whether it be considered in reference to the variety and brilliancy of these results, or the light it has thrown and is destined to throw on the remotest physical inquiries, it is second in interest to no special department of Inquiry within the entire range of science. Unfortunately we cannot do more than advert in the most cursory way to the phenomena themselves, and the rationale of their production. The phenomena are simply as follows:—A Polariscope (*q. v.*), as is well known, consists of a polarizing crystal or plate, and an analyzing apparatus. The latter is always so fitted that it may be turned round on its axis, so that—as in the case of the plate of tourmaline, described at the opening of this article—the opposite effects on the polarized ray, springing out of the different positions of that axis, may become visible. If in such an instrument a thin film of any doubly refracting crystal be placed between these two plates, the eye directed through the analyzer immediately descries the most remarkable display of colours—systems of prismatic rings of greater or less intricacy—crosses, bright and dark—and the most curious inversions of these systems when the analyzer is rotated. These coloured systems vary with the species of crystal interposed, clearly depending on the nature of that crystal; and they also vary with the form in which the light that passes through it has become polarized. They are descriptions in fact, in language the most gorgeous, of large classes of phenomena and forces, that are not at present revealed in any other manner. The student must go for details on this engrossing subject to works on optics—we have already often specified Sir David Brewster's: but, better still, let him experiment for himself, having procured so effective, pleasant, and cheap an instrument as that at present made by Mr. James Bryson, Optician, Edinburgh. The rationale of the evolution of these phenomena is very simple, and will become palpable by aid of the subjoined diagram, copied from the instructive posthumous work by Dr. Pereira.

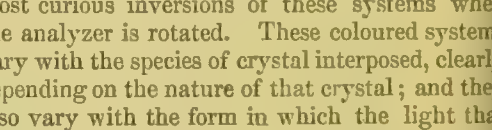


Fig. 5.

In the foregoing arrangement B is a polarizing plate of tourmaline and G the analyzer;—a doubly refracting prism capable of being rotated on its axis. D is the thin film of crystal, sometimes called the *depolarizing plate* for reasons that will be immediately obvious. Let us now trace the progress of a ray of common light A as it passes through this arrangement. On emerging from the tourmaline B, that ray will of course be found converted into another ray, C,

that is plane-polarized. This plane-polarized ray enters the doubly refracting film of crystal D; from which it issues *doubly refracted; i.e.*, separated into two rays O and E (the ordinary and extraordinary), polarized at right angles to each other—one of which, the ray E, is found to have lagged behind the other, in virtue of a principle already adverted to under INTERFERENCE. They come out therefore in different phases of vibration, as well as oppositely polarized. These two rays on passing through the analyzer G are again divided, each into two, viz., the ray E into E *o* and E *e*, and the ray O into O *o* and O *e*. Now, as represented in the diagram, E *o* and O *o* will emerge together, while E *e* and O *e* emerge together; and as may also be readily traced E *o* and O *o* are polarized in the same plane, while E *e* and O *e* are also both polarized in the same plane; but the one set at right angles to the other set. Now as the two vibrations that are in the same plane are not in the same phase (because of the principle of lagging or retardation just spoken of), they will interfere, and therefore as the necessary result of interference of rays polarized in the same plane, *they must produce colour.* But, besides, the two systems are in different states—the vibrations in the one plane *conspiring*, while those in the other are *opposed.* Whence, the colour evolved by the one set of interferences must be *complementary* to the colours evolved by the other set. Suppose, then, that the analyzer is a *tourmaline*, it is manifest that during its rotation we shall have the evolution of two sets of colours produced by interference, that are exactly complementary to each other. The student is further referred to the lectures of Pereira.

V. POLARIZATION BY THE ATMOSPHERE.—One other point demands a passing notice; unquestionably a much more important one than it yet appears. As the light of the sun is reflected towards us by the atmosphere at all varieties of angles, it is clear that this atmospheric or indirect light, ought to reach us in all stages of polarization—stages passing from a *maximum* to a *minimum*—according to the angle of its reflexion. The subject early attracted the notice of Arago, next of Babinet, and its entire empirical laws have recently been laid down by Sir David Brewster. The laws in question are fully represented by the latter philosopher in an expressive map, in Johnston's exquisite *Physical Atlas.* They are briefly these:—Terming, the point in the sky opposite the sun, the antisolar point. Arago discovered a *neutral* point, or a point of no polarization, situated about 30° above the antisolar point. Babinet discovered, in 1840, another neutral point, about the same distance above the sun. In 1841, Sir David Brewster discovered a secondary neutral point, accompanying Arago's, about 12° below it; and the same philosopher has, more recently, detected a fourth neutral point *below* the sun, about 15°. These neutral points form the starting parts of the map referred to. Lines or curves of equal polarization are traced around them, and all the phenomena of atmospheric polarization clearly depicted. Sir David has added valuable explanations and empirical formulæ to this map. See Johnston's *Atlas.*—No evidence yet exists whether this action of the atmosphere is influential over the economy of our globe.

VI. Other phenomena connected with polarization are amply treated in various portions of this volume, especially under INTERFERENCE, MAGNETISM, REFLEXION and REFRACTION, and UNDULATORY THEORY. But with all these, the view given is extremely meagre. The student must supply the deficit by reference to special works, many of which have been mentioned in the previous articles.

Polars, Theory of. A method of Inquiry in pure Geometry, capable of being carried out very far. There is so much power in it—it so grasps and lays hold of the relations of pure space—that we think it advisable to give a brief account of it.—One special form of the theory is connected with the circle. Let Q P be a circle, and O any point; draw the

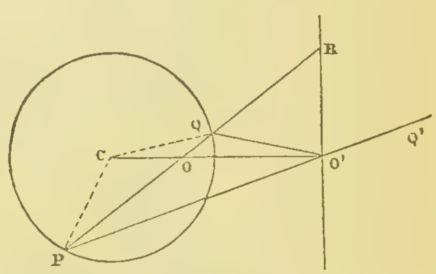


Fig. 1.

line $O'R$ perpendicular to CO, and at such a distance that $CO : CO' = CQ^2$. The line $O'R$ is called the *polar* of O: the point O is called the *pole* of the line $O'R$. To find it when $O'R$ is given, we have evidently this inverse process—draw a perpendicular on the line from C, and take CO, so that $CO \cdot CO' = CQ^2$. To show the chief properties of the polar, let us draw any line through O, meeting the circle and the polar in P, Q, R, then this line is harmonically divided. Since $CO' \cdot CO = CQ^2$ we have the proportion $CO : CQ :: CQ : CO'$, and hence (VI. 6) $CQO = CO'Q$. Similarly, $CPO = CO'P$. Now, $CQO = CPO$, therefore $CO'Q = CO'P$. Also $CO'R$ is a right angle, therefore $QO'R = Q'O'R$. Hence (Euc. VI. 3. A),

$$PO' : QO' :: PO : QO$$

and $$PO' : QO' :: PR : QR$$

therefore $$PO : QO :: PR : QR,$$

therefore, by Euclid's definition, the line is cut in harmonic progression. It will be clear, that if O lie upon the circumference—O, and O′ coincide, and the proposition becomes worthless, geometri-

cally. If O be outside of the circle we should have a quite similar construction and proof suited to that case.—From this it will be easy for the reader to deduce.

1° If any number of points on a right line be taken as poles, then polars, with respect to a given circle, pass through one point, the pole of the given line. Thus suppose points taken on O′ R, say R—and let each of them be joined with O—then, since R Q O P *is* cut harmonically, and since *wherever* the polar of the point R cuts R Q P, it will cut it harmonically, O will be the point where it so cuts it. Since O then depends on the line O′ R and does not alter with the position of the varying point R, the polars of that varying point all pass through O.—It is easy to see that if a pair of tangents be drawn from a given point, the chord of contact, the line joining these points of contact will be the polar of that point. Hence, generally, if from all points of a given line, pairs of tangents be drawn, all the chords of contact will pass through one given point—the pole of that line.

2° If any number of right lines pass through a point, the locus of these poles, with respect to a given line, is a right line, namely, the polar of the point. This one immediately sees, from the reciprocal relation of the pole and the polar, to be the mere inverse of the last proposition.

3° If through a point A we draw any two chords A B C, A B′ C′ to meet the circle, and join

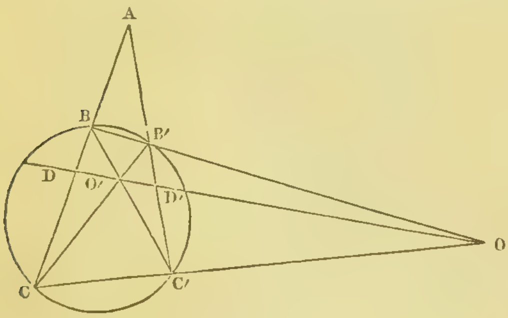


Fig. 2.

B B′, C C′, B C′ B′ C, then A is the polar of O O′, O of A O′, O′ of A O.—It is a property well known in the theory of harmonic pencils that A B′ C′ is cut harmonically—so also A B D C. Hence, the line O O′ cuts these *two* lines through A just as the polar of A does, so that O O′ is the polar of A. Just in the same way it may be shown that A O′ is the polar of O, and it follows from this by proposition 1°, that O′, the intersection of the two polars, is the pole of the line A O upon which the two poles lie.

4° If four poles lie on a right line, these polars form a pencil the anharmonic ratio of which is the same as that of the four poles.—This follows from these two considerations: The first, that the polars meet in a point, since the poles lie on a straight line—the second, that since the polars are perpendicular to the lines joining the poles to one fixed point, the centre of the circle, the angles which they contain are equal to those contained by these lines; and as the anharmonic ratio of the four poles is expressible in terms of these latter angles, it follows that the anharmonic ratio of the polars is the same with it.

5° If a quadrilateral be circumscribed about a circle, and an inscribed quadrilateral be formed by joining the successive points of contact, the diagonals of the two quadrilaterals intersect in the same point, forming a harmonic pencil, and the third diagonals are co-incident. P Q R S, A B C D are the quadrilaterals. Since S is the polar of A D, and Q of B C, S Q is the polar of O′. Similarly P R is the polar of O. Therefore, V the intersection of diagonals of the *outer* quadrilateral is the pole of the third diagonal of the *inner*. But, as proved in 3°—the pole of the third diagonal is also the intersection of the diagonals of the *inner*, and as the pole of a definite line with respect to a given circle can only be one quite determinate point, the two intersections of diagonals coincide.—Also, since P R passing through V, the pole of O O′ is the polar of O, it must also pass through O′, because (3°), O′ V is the polar of O. Also, O is the pole of S Q. Hence, the lines A D O′, B C O′ are cut harmonically, and the lines at V make a harmonic pencil.—Also the points X, Z

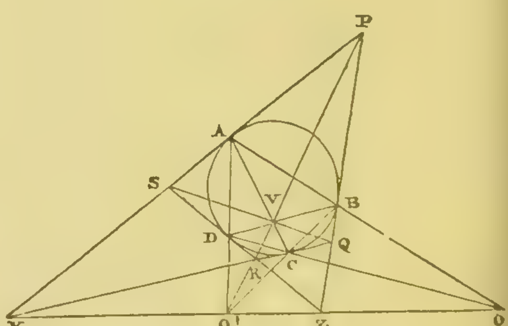


Fig. 3.

are poles of A C, B D respectively, and therefore, X Z is the polar of V. But so also is O O′. Hence, X Z O O′ is a straight line, seeing that any given point has with reference to a given circle a determinate polar line. It merely expresses that X O′ Z O is the polar of V, to say that a line, say C V through V and the centre C would be perpendicular to it, and if it meets it in V′ that C V · C V′ = the square of the radius.—We have given enough to suggest the processes, and to supply the chief principles of the purely geo-

ıetric method of polars. What follows is in ex-
:nsion of that to the ordinary conics. One pro-
erty of the polar we saw to be that for points
ithout the circle, they coincide with the chords
f contact of tangents to the circle. Suppose we
efine the polar of points outside of any conic
:ction to be similarly the chord of contact.
'his would hold with reference to any point out-
.de; but a more general definition is required.
iven any point o′ let it be joined with the centre
f the conic c, and c o′ taken in it so that c o,
o′ shall be equal to the square of the semicon-
.igate diameter; and through o′ let a line be drawn
arallel to this semiconjugate diameter—then it
ill be found that this line has quite similar
roperties with respect to o as the polar line has
ith respect to the pole in the case of the circle.
Ve call the point and the line mutually pole
ıd polar with respect to the conic. It was
ear in the circle that the polar is always per-
endicular to the diameter at whose extremity it
—that is, parallel to the semiconjugate diame-
:r. We see this same to hold here. Analyti-
lly the expression is the same for the chord of
ntact and the polar, the co-ordinates of the
ıle being substituted for the co-ordinates of the
int through which the tangent passes. This
cures, as the analytical reader will readily see, the
ırallelism of the chord of contact and the polar.
-Two propositions with respect to the polars in
rcles which are more readily proved by analyti-
l than geometrical principles, we merely state.
-1°. Given any two points A B and their polars,
c be the centre of the circle, then C A : C B in the
.verse ratio of the perpendiculars let fall from each
int on alternate polars. 2°. Given a circle and
triangle A B C, if the polars of A′ B′ C′ be taken
'B C, &c., then A A′, B B′, C C′ will pass through
ıe point. We can readily prove the same pro-
sition as 1° with respect to the polar of a point
in general—that if O R′ R R″ be drawn, any
ne intersecting the conic, the polar and the conic
ccessively, it will be harmonically divided.
'e have also the same proportion as 3°, in gen-
al, for any conic. These are the principal
ıeorems which we think it necessary to state
ith respect to direct polars. But there is a
ethod of obtaining from every polar theorem an
verse theorem, which serves as a means of dis-
very of problems. Let O be a given point—the
ntre of a circle, not described in the figure—and
t S P P′ S be a curve in any way connected with
. Then draw O perpendicular to any tangent
T, and so that O T, O p is equal to the square of
e radius. A curve $s\,p\,p'\,s$ will evidently be de-
ribed by the extremities p, in which each point
corresponds to one of the given ones P. Still
ore generally, had not a circle but a conic been
t down as the curve of reference, and the polar
P been described, or rather the successive points
through which it is constructed, obtained, we
ould have had a different curve—varying with
e species and particular description of conic—
for the reciprocal. The circle is most generally chosen. In the general case, however, we can

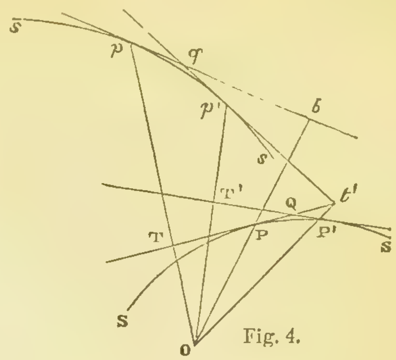


Fig. 4.

show what the properties are in which the reciprocity consists.—Suppose P P′, $p\,p'$ to be corresponding points, then, by definition p is pole of the tangent P Q, and p' of P′ Q. Hence $p\,p'$ is the polar of Q. Let these points P P′ $p\,p'$ come as nearly as possible to coincide, then $p\,p'$ will become the tangent at p, Q will come to coincide with P. Hence in the limit we have P the pole of the tangent at p. Therefore, generally, any point in the one curve—and that either of them—has its polar or tangent to the other, and *vice versa*.—The reciprocal theorems are obtained, in fact, in this way—if we have given in any statement of certain poles lying on a right line—we know that those polars meet in a point. Taking this as example, we see that in many cases data with respect to the one set of points—considered as poles, will furnish information with respect to the other considered as tangents. Thus the following theorem is called Pascals. If a hexagon be inscribed in a conic, the intersections of its opposite sides lie along a right line. Take the reciprocal polar conic—with respect to any curve of reference—and take the polar corresponding to the angular points of the hexagon. In fact, interchange lines for points and points for lines, and we find this theorem—if a hexagon be circumscribed about a circle its successive diagonal, through opposite angles, meet in a point This is called Brianchon's theorem. This method, after a little application and use, will come to consist in the mere mechanical substitution of *point* for *line*, *inscribed* for *circumscribed*, *envelope* for *locus*, &c., and *vice versa*. Thus to write down only other two theorems.—If two vertices of a triangle move along fixed right lines, while the sides pass each through a fixed point, the locus of the third vertex is a conic section. The reciprocal theorem is seen to be this:—If two sides of a triangle pass through fixed points while the vertices move on fixed right lines, the envelope of the third side is a conic section. It is easy to show that if two conics touch, these reciprocals also will touch,—if there be double contact, there will be the same in the reciprocals. We fear it is impossible here to give any further developments of the theory. The reader is referred

to Mulcahy's *Modern Geometry* for the purely geometrical applications, and especially to Mr. Salmon's *Conic Sections*, and the admirable chapter upon *Reciprocal Polars* in that work.

Polar Triangle. See TRIGONOMETRY, SPHERICAL.

Pole. A word originally meaning a turning point. Hence it came to signify the extremities of the axis upon which the earth turns round in its diurnal motion. When transferred to the heavens, it came to mean the points round which they seem to turn—the two diametrically opposite points, that remain unmoved in the general motion of all around—the points in fact, where the production of the axis of the earth cuts the celestial sphere. In transferring the term to any spherical body under rotation, or considered as possibly under rotation, it is evident that the two points, where the axis cuts the spherical surface, would necessarily be chosen. The term, however, has come to have a still wider meaning—and now signifies almost any point of direction that may be mentioned. In co-ordinate geometry, the pole is the origin—or point of primary reference—and this is the use to which the term is generally put. The sciences of optics, magnetism, light, &c., all have their poles; and in them generally, this meaning, as the point of primary reference—the point where the most noticeable effect is produced or such like, prevails. See POLARITY.

Pole Star. That one of the stars visible to us which is not affected by the diurnal apparent motion of the heavens, is, in accordance with the definition of the Pole, called the Pole Star. It is that star which the produced axis of the earth would reach, in the visible heavens. In the hemisphere beneath our horizon there will be another Pole Star; of course we do not see it. No bright star is in the exact continuation of the line of the earth's axis at present. The point of the pole, besides, does not remain stationary in the heavens (PRECESSION, NUTATION.) Hence it is usual to call a bright star near the Pole, which therefore does move in a very small diurnal circle round it, the Pole Star. The actual Pole Star is one in the constellation—Ursa Minor (*q. v.*) This will not remain always so, because of the shifting of the pole itself, but it will be so sensibly for a very long period.

Polhymnia. An Asteriod discovered in November, 1854.

Pollux. One of the parts of the constellation Gemini (*q. v.*) The other is Castor; and the constellation is sometimes called instead, Castor and Pollux.

Pomona. One of the Asteriods discovered in November, 1854.

Pores. Interstices in bodies between their constituent particles, where no matter is supposed to exist or only air through its interpenetration. Some bodies, such as sponges, possess great porosity. The compressibility is nearly in the inverse ratio of the porosity. The metals have but few pores. In animated beings, pores are the instruments of the processes of exhalation and absorption.

Porism. A name given to a remarkable class of propositions in the ancient geometry, whose nature long continued enigmatical in modern times, and which, even yet, remains to a large extent obscure. The great work upon the subject seems to have been a special treatise by Euclid, the only relics of which are contained in the description given by Pappus Alexandrinus. The object of Pappus, however, having been to give a summary and not an exposition of the Greek geometry, he wrote so briefly, that although his account may have been perfectly intelligible to those who knew what a Porism meant, it was quite a riddle to mathematicians after the signification of the Porism had been forgotten and wholly lost. The entire chapter of Pappus is most tantalizing. He begins by speaking of Euclid's work as "*Collectio artificiosissima multarum rerum quæ spectant ad analysin difficiliorum et generalium problematum;*" but on his proceeding to state what a Porism is, and to illustrate his definition by the enunciation of thirty separate instances (unfortunately unaccompanied by diagrams), we find ourselves within a maze of vagueness and perplexity. Few acuter men have lived than Dr. Halley, yet, even he, after carefully examining the Greek text, gave up the attempt to unravel the mystery in despair. Before his time indeed, a certain but very imperfect light had been thrown upon the subject by the illustrious Fermat; but the conclusions at which that geometer had arrived, were so special and confined, that the general nature of a Porism remained nearly as obscure as ever, nor does he appear to have even suspected that this class of problems required for its investigation a peculiar process of geometrical analysis. The enigma, in great measure, yielded at last to the penetration of Dr. Robert Simson—whose treatise in his *Opera Posthuma*, will ever be cherished as a gem. The definition at which Simson arrived was the following: "A porism is a proposition in which it is proposed to demonstrate that one or more things being given between which and every one of innumerable things not given, but assumed according to a given law, a certain relation described in the proposition is to be shown to take place." Even this definition at first sight appears somewhat obscure; but it is so chiefly from its generality. Simson restored the right analysis and detected the meaning of *six* of the special enunciations of Pappus. Simson's treatise being now difficult of access, the student is referred to a dissertation by Professor Playfair in the *Transactions* of the Royal Society of Edinburgh, and reprinted among his miscellaneous works. This essay is distinguished by the remarkable lucidity characterizing all the writings of Playfair; but it may be questioned whether the difficult

subject is exhausted by it. It is the ingenious conception of Playfair, that Porisms arose necessarily out of the logical *completeness* which the ancient geometers sought to bestow on every problem undertaken by them. A partial solution was not held satisfactory,—that is, a solution for merely *one* condition or relation of the data. The solution obtained for one state of these data, they inquired what would occur were these data related to each other in all conceivable ways, and in course of this inquiry, they hit upon the truth, that there often were relations *rendering the problem indeterminate*,—just as when the value of two variables are sought to be determined by two equations with equivalent forms and constants. These indeterminate cases according to Playfair, form the subject-matter of Porisms:—hence his definition. "A porism is a proposition affirming the possibility of finding such conditions as will render a certain problem indeterminate or capable of innumerable solutions."—That Playfair's view enables us to construct many propositions that are Porisms is unquestionable; nevertheless, it cannot be supposed that anything so simple and limited has entirely surveyed a field, the culture of which the Greek geometers considered so very important. Upon the whole, it is safest at present to adopt the sentiments of Chasles, "a thick veil still covers the doctrine of Porisms; for, independently of the twenty-four enunciations of Pappus to which we can as yet attach no meaning, we require to know the reason of the unusual form of these propositions, the whole idea on which they repose, their nature or mathematical character, the reason of their alleged eminent utility in the art of geometry, and the transformations which this doctrine has subsequently undergone, so that it penetrates—unknown to us—among modern methods." Were we to rest with Playfair's idea, we should find the subject of Porisms entirely included within the modern doctrines of *Continuity*, *Correlation*, and *Deformation*; but as we have said, it probably extends much farther. The time, however, seems to have come when a geometer with suitable knowledge and tastes, might achieve new triumphs in this special and most interesting region of inquiry.—We cannot omit a tribute to the labours of Mr. Babbage. He desired to discover the connection between the ancient analysis applied to Porisms, and some method of the modern analysis; and he found the link in a favourite subject of his own, the subject of *functional equations* chiefly of the *periodic* class. This remarkable memoir will be found in *Transactions* of Edinburgh Royal Society, vol. ix.

Position (Angle of). The angle which the line between two stars makes with any fixed line—usually with a circle of declination.

Potential Energy is the capacity for performing work possessed by a body, in virtue of the forces exerted either between its parts or between it and other bodies, and is called "potential," to distinguish it from *Actual Energy*, which is the capacity for performing work possessed by a body in virtue of the condition of each of its parts independently of the others. Heat, light, electric currents, and the *vis-viva* of a moving body, are examples of actual energy. As an example of potential energy, due to a force exerted between two bodies, we may take the case of a mass suspended at a given height above the earth's surface; that mass, in consequence of the attraction between it and the earth, possesses potential energy or capacity for performing work to the amount expressed by the product of its weight into the height at which it is suspended. As an example of potential energy due to the forces exerted between the parts of a body, we may take the case of a bent spring, which possesses potential energy to the amount expressed by the product of the mean elastic force which would be exerted in the act of unbending, into the distance through which such force would act. See MACHINE, *ante;* also the following papers, in which the terms "Actual Energy" and "Potential Energy" were first introduced:—Rankine on the *General Law of Transformation of Energy*, *Phil. Mag.*, Feb., 1853; Rankine on the *Science of Energetics*, *Edinburgh Philosophical Journal*, 1855. So far as the works of Aristotle can at the present day be made intelligible, it appears that the term δυναμις may be translated by "potential energy," and the famous word εντελέχεια (so long a mystery to commentators), by "actual energy."

Potential Function, or simply *Potential*, is a function of the masses of a body or system of bodies, and of the position of such body or system relatively to a given point, of such a nature that the component parallel to a given direction of the attraction or repulsion of the body or system upon a particle of the mass unity, situated at the given point, is the differential, co-efficient of the potential relatively to an ordinate measured along the given direction; or, in other words, the rate of variation of the potential along the given direction. To express this symbolically: let T denote the potential of the attracting or repelling body or bodies relatively to a given point O, at which point let a particle of the mass unity be situated; through O, in any direction, conceive a straight line, Ox, to pass; take a point, P, on the line Ox, at the distance Δx from O; let T' be the value of the potential for the point P; then must the potential be a function such, that the limit towards which the quantity

$$\frac{T' - T}{\Delta x}$$

converges, as the point P is brought nearer and nearer to O (a limit denoted symbolically by $\frac{dT}{dx}$) shall be the component in the direction Ox of the attraction or repulsion on the unit of mass at O.

If three axes at right angles to each other, $\text{o}x$, $\text{o}y$, $\text{o}z$, be conceived to pass through O, then will

$$\frac{d\text{T}}{dx},\quad \frac{d\text{T}}{dy},\quad \frac{d\text{T}}{dz},$$

in like manner, be the three components of the attraction or repulsion on the unit of mass at O along those three axes; so that the whole or resultant attraction or repulsion will be

$$\sqrt{\left(\frac{d\text{T}}{dx}\right)^2 + \left(\frac{d\text{T}}{dy}\right)^2 + \left(\frac{d\text{T}}{dz}\right)^2} = \text{R},$$

and the cosines of the angles made by that resultant with the three axes respectively will be

$$\frac{\frac{d\text{T}}{dx}}{\text{R}},\quad \frac{\frac{d\text{T}}{dy}}{\text{R}},\quad \frac{\frac{d\text{T}}{dz}}{\text{R}}.$$

It is evident, that in order to fulfil the above condition, the difference $\text{T}' - \text{T}$, between the potential at P and the potential at O, must be the work which an unit of mass is capable of performing in moving, under the influence of the attraction or repulsion from O to P; that is to say, that difference must be the *potential energy* of an unit of mass at O relatively to P. If the attraction or repulsion *resists* motion from O to P, the difference $\text{T}' - \text{T}$ will be negative. From these considerations, it appears that the potential, at a given point O, is to be determined in the following manner: conceive an unit of mass to start from a locality at which the attraction or repulsion is null (infinitely far off, if necessary), and to move to the point O. The work performed by the mass during the process (increased or diminished by a constant quantity, if convenient), is the potential of the action of the attracting or repelling bodies on an unit of mass at the point O. The potential of the joint action of any number of bodies is the sum of the potentials of their separate actions; and the potential of the whole action of a body is the sum of the potentials of the actions of its particles. Hence, to find the potential of the action of a body of any figure, conceive it to be divided into indefinitely small particles; find the general expression of the potential for a single particle, and integrate this for the whole body. The potential of a single particle is found as follows:—Let the particle be situated at M, and let m denote its mass; let the distance, $\text{M} = r_1$. Conceive an unit of mass to move from an infinitely great distance towards M; let its distance from M at any moment be r; let the force exerted between m and the moving unit of mass at that distance be

$$\text{C}\, m f(r)$$

where C is a constant, and $f(r)$ a function of the distance, and the force is regarded as positive if repulsive, negative if attractive. Then will the work performed by the moving unit of mass, by the time it has reached O, be

$$\text{T} = -\text{C}m \int_{r_1}^{\infty} f(r)\, dr \ldots\ldots (1.)$$

and this is the *potential* of the action of the particle m at M, on an unit of mass at O. In the case of the force of gravity, we have

$$\left.\begin{aligned} f(r) &= -\frac{1}{r^2} \\ \text{and consequently } \text{T} &= \frac{\text{C}m}{r_1} \ldots\ldots\ldots\ldots \end{aligned}\right\}(2.)$$

The symbolical expression for the potential of the action of a body of any figure on an unit of mass at O is as follows:—Conceive the body to be divided into small particles; let $d\text{V}$ be the volume of any one of those particles, ϱ its density (so that the whole mass of the body is $\int \varrho\, d\text{V}$), and r_1 its distance from O. Then in the formula (1.), $\varrho\, d\text{V}$ is to be substituted for m, and the expression so obtained integrated for the whole body, giving the following result:—

$$\text{T} = -\text{C} \int \left(\varrho \int_{r_1}^{\infty} f(r)\, dr \right) d\text{V} \ldots (3.)$$

In the case of gravitation, this becomes

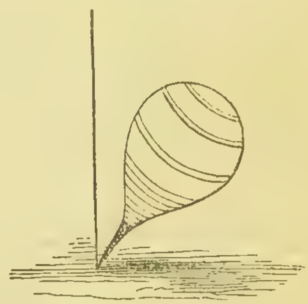
$$\text{T} = \text{C} \int \frac{\varrho}{r_1} d\text{V} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4.)$$

An *Equipotential Surface* is a surface traversing all those points at which the potential of a body or system of bodies, has some given value. It is everywhere normal to the direction of the resultant force. A free moving body, in passing from one equipotential surface to another, has the half-square of its velocity increased or diminished by an amount equal to the difference of the potentials, according as the second potential is greater or less than the first. The first introduction of the potential function into mechanical investigations is due chiefly to Legendre, Lagrange, Laplace, and Poisson, who applied it to the theory of gravitation. It was applied to the theory of terrestrial magnetism by Gauss. Its use was extended and perfected by many subsequent authors, especially by Green, who first gave it its name of "potential." It may be remarked, that something like the germ of the idea of a potential function is to be found in the 39th and 40th propositions of Newton's *Principia.*

Power. A term equivalent to *force*, or rather to the origin of force.

Precession of Equinoxes. The place of a star in the heavens is determined by its right ascension and declination, or by its latitude and longitude. Each of these is a measurement referring the place of the star to a point relatively fixed; so that, such points derive their value from their real or apparent *fixity* in the celestial sphere. If it be found that they move, the measurements that are taken will have to be retaken, from year to year; or, at all events, theoretical corrections will have to be made upon them after we have discovered the amount of

variation. The change of the fixed point by reference to which right ascension or longitude is measured, is the source of what is termed the Precession of the Equinoxes. The *vernal equinox* is this point; *i.e.*, the intersection of the equator and ecliptic. Does it change its position in the heavens, or is it a constant point? It does alter by a slow movement. It appears to travel for the most part along the ecliptic, backwards from east to west—that is, in the direction of *apparent* diurnal motion—and so, contrary to the direction in which the sun moves in the ecliptic. The equinox is thus carried constantly in apparent advance of the stars, and from this comes the name—*Precession of Equinoxes.* It is found that the motion is very regular, so that a constant correction may be applied for it. The amount by which the point of the equinox retrogrades along the ecliptic is about $0° 0' 50{\cdot}10''$ per annum. In 25,868 years, the equinox will go completely round the ecliptic. The manifest effect of this will be to bring some of the fixed stars nearer to the point of reference, and drive others farther from it, as regards their longitude; and as we employ this point constantly—though it is a changing point—as if it were a fixed point, the longitude of all the stars must seem directly affected by that change: *i.e.*, there must be an apparent regular increase of longitude in all the stars. Taking the Ecliptic as a fixed circle (although it is not so, quite accurately), let us look at the other circle—the Equinoctial. It may change its position. But that change must indicate some alteration in the circumstances upon which its origin depends. There must be, therefore, some motion of the pole of the heavens—that is, *some motion of the axis of the earth* to account for precession. It is found that the change which would fully account for precession consists in the rotation of the pole of the equinoctial—round what we may suppose, for the time, a fixed point—the pole of the ecliptic. That pole then, has a slow motion, in a circular curve, and with a uniform motion. It completes this in about 25,868 years. It is evident that this shifting of the pole will, as long as we use it as the point of reference—that is, as the stationary point to which all others are referred—cause an apparent shifting of all the heavenly bodies in the opposite direction. Thus, that star which we now call the pole star—a bright one in Ursa Minor, which is not really the pole, but the nearest bright star to it, being distant only $1° 24'$ on the celestial sphere—is stated in the earliest catalogues, to have been as many as $12°$ from the pole. In about 12,000 years the star α Lyræ, which is the brightest in our Northern Hemisphere, will be within $5°$ of the pole, and will then be chosen doubtless as the pole star.—The phenomena whose general effects are now noticed, will be best understood by aid of a further illustration. The earth has three motions in absolute space. *First*, it wheels round the Sun in its annual orbit; *Secondly*, revolves on its axis; *Thirdly*,—and this is the conclusion to which, as above explained, the facts of Precession lead,—the axis of rotation itself moves *in Space.* Observe, *in Space;* for the axis in respect of the Earth itself, is permanent; nor do astronomical phenomena afford any countenance to the hypothesis of shiftings of that axis. Spin a *tee-totum* or a top. The toy as it spins rolls over a space, as the Earth around the Sun; it spins on its axis likewise, as the Earth round its polar axis; but the axis *does not remain upright.* On the contrary, it is performing a regular gyration, one, whose mechanical cause is subtle but determinate; and the axis of the Earth gyrates in a similar way; which motion represents



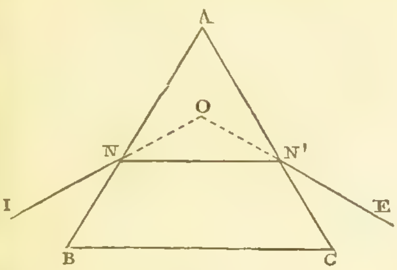
exactly the *Precession of the Equinoxes*, as deduced above. We cannot avoid referring here to the admirable contrivance by which Mr. James Elliot of Edinburgh has succeeded in manifesting that exact motion in a free rotating sphere.—Let us turn next to the *physical cause* of Precession. If the Earth had been a sphere or perfect globe, there would have been no such gyratory motion of its axis. It would have spun evenly and for ever, its axis remaining upright—pointing undeviatingly through all ages to the same point in the sky, as the Polar Star. The cause of this gyration is solely the *equatorial protuberance or bulging;* nor will it be difficult to show why this bulging must so act. In the first place, suppose the ring or bulging portion separated from the Earth, which would then be reduced to a simple sphere, rotating of itself, with an axis permanently pointed toward the Heavens. This ring may be fancied isolated in the meantime, and its fortunes traced as if it were so isolated. We may suppose the ring also not a continuous mass, but a succession of Moons so close, that they touch each other, all revolving round the Earth at the same time:—it must, of course, be manifest that what befalls *one* of these moons, must befall *them all*, or the *ring* they constitute. Now in articles ECLIPSES, LUNAR THEORY, it has been explained that the *nodes* of the *Orbit* of any such single moon must *regress:*—the nodes of our own moon making a circuit of this kind around the Heavens, in about eighteen years. But each

of the small moons of which we have been speaking, must exhibit precisely such changes;—so therefore must their total, viz., this equatorial ring. This curious jolting of such a ring may also be familiarly illustrated. Take a *crown piece*, and spin it on its edge or rim. By and by it will fall; but observe it *for a short time before it falls.* The high and low parts of the rim do not remain in the same place, but pass round a circle, in a regular way, and the line of the middle part of the piece of silver also turns round with the same regularity—a line we would term, speaking scientifically, the *line of nodes:*—and with precisely the motion of the nodes of our great Equatorial *belt* or *hoop.*—This phenomenon distinctly understood, one step farther will lead us to the physical causes of the actual Precession. Let the following picture be realized—a vast sphere, a globe whose uniform diameter is its length from pole to pole, rotating round a fixed motionless axis, and an equatorial girdle rotating in the same time but with the peculiar jolting, or nodal motion just described. Suppose next, that these are not separated—that the girdle of moons is not isolated, but on the contrary, that it *clasps* the spherical Earth. What must ensue? This unquestionably: the girdle must communicate its peculiar nodal motion to the globe it encircles, and therefore cause the axis of that globe to gyrate like the axis of a *top:* but, in communicating that motion to so vast a mass, the girdle must sacrifice its *velocity:* the force expended in constraining the central sphere to partake of that motion, and in constraining its otherwise fixed axis to gyrate, cannot be exercised unless at the expense of its own motion,—just as one ball moving swiftly cannot accelerate a sluggish one, without, by the very act, abandoning a portion of its own superior speed. Now, even as the motion of the lunar nodes can be computed, so may the motion of the nodes of the free ring be numerically estimated also; and the masses of the ring and the interior sphere being determined, it is evident that the actual period of this motion of the Earth's axis or the amount of Precession, may be determined with the last precision.—Such the general character and physical origin of this remarkable motion. The actual calculation of its amount, however, is not a little complex. Part of the motion depends on the influence of the Sun, part on that of the Moon, and part on that of the external bodies acting through gravitation on the Earth. The annual precession depending on the Sun and Moon jointly—of course, by far the largest portion—is the *Luni-Solar Precession;* while the total annual effect is termed the *general Precession.* Still farther, this precession, owing to the slightly changed relations of all these bodies, through the action of *perturbations* (*q.v.*), is not the same from year to year, but varies by a small quantity: this is termed the *Secular Variation.* The mode of eliminating this influence from the apparent places of the Stars, is explained under STARS CORRECTIONS FOR.—It is impossible to pass without notice in this place the remarkable application by Mr. Hopkins of Cambridge of the phenomena of Precession, in elucidation of one of the vexed points in Geology; the question, viz., whether the interior of our globe is solid or liquid, that is, molten? As stated above, the solid motion of the protuberant ring is accurately calculated, and its mass is also known. Its power *to drag* is thus a fixed quantity. Now, in dragging the earth, it expends, as above, a certain quantity of its force; and as the amount of force so expended, or the actual retardation of velocity in this case, is known from observation—(the amount of Precession being established by observation)—it follows that the magnitude, or rather weight or reaction of the body dragged, that is of the *spherical globe*, must be deducible. Now, it has been supposed to be indicated by important geological phenomena that the solid crust of the globe is not more than *one hundred miles in thickness.* Were that the case, or anything approaching to it, what would be the effort required from the Equatorial rings? Clearly this shell would *slide* over the surface of the internal liquid—even were that liquid viscous—and although a certain amount of friction would impede this motion, the effort required would still not greatly exceed that necessary to move or drag such a shell. But we know the mean or average density of the Earth; and it clearly appears that no shell of dimensions approaching to the foregoing, could have sufficient inertia to retard the swift nodal motion of the Equatorial Ring, so that it assume the recognized period of Precession; nay, the shell in question, must, in order that it promote such a result, be at least one thousand miles thick. There is nothing indeed in this inquiry rendering it impossible that the globe is solid throughout, and special although limited physical objections may be urged against one part of the foregoing reasoning: but a distinct *negative* certainly appears to be given by it to a whole class of geological conceptions, on grounds vastly more solid and defensible than any that are alleged to sustain them.—See the remarkable Memoirs by Mr. Hopkins in *Transactions of Royal Society;* also TEMPERATURE.

Pressure. A term taken from the perception of the action of forces which are resisted by muscular power. The transference of this to the action of forces in any case is natural enough. It is only important, that the student should not conceive that pressure—from its acting through matter in contact with that acted on—is anything essentially different from other forces. It is to be remembered also (see IMPULSE), that wherever there is the action of force in this direct way—of matter in contact with matter, it is through pressure acting through measurable intervals of time. There is no such thing possible *in rerum naturâ* as the action of an instantaneous force.

Primum Mobile. The name given, in he ancient Ptolemaic system, to the imaginary phere which carried all the heavenly bodies ound upon it and along with it.

Prism. A prism is any transparent medium f a triangular shape, as below. Such a confor-



nation has the effect upon rays of light passing hrough it, of subjecting them to two refrac-ions, and therefore of manifesting the dispersing ffects of refraction in a very high degree. For nstance, the intrant ray I N, after being first efracted and transmitted along the line N N′, is gain refracted in the opposite direction, and hen emitted, pursues the path N′ E. If dis-ersion depends on difference of refrangibility, the rism is therefore a potent means of dispersing r separating the various rays of which the *sheaf* r beam of ordinary light is composed. Hence has always been employed to analyze ordi-ary light, or to divide it into its differently oloured rays. The angle formed by the intrant nd emitted beams, or the supplement of the ngle N O N′, is called the *angle of deviation.* This nstrument has many other uses.

Prism, Nicol's. A most valuable ana-zer of polarized light, and also an excellent olarizer. Take a rhomb of Iceland spar, be-ween a half-inch and an inch in length, and rithin a quarter of an inch of breadth; cut into two, along a plane perpendicular to the lane of the larger diagonal of the base, and assing through the obtuse angled corners; then eunite these two halves in the same order by anada balsam,—that is a Nicol's prism. The erfect transparency of this little apparatus, gives an inappreciable value in many researches egarding polarized light.

Probabilities. In this article we shall as-ume, as already known, those principles of the eory of Combinations which are exhibited under ERMUTATION.

I. In games of chance, such as that depending 1 drawing from fifty-two cards, one with a ecial mark, the condition of the question points it several hypotheses as to what will occur, as jually probable before the event. If the cards arranged inside of a box in a manner quite nknown, arrangements may be readily conceived hich will make it almost impossible, or highly nprobable, that some particular card should be awn. But in total ignorance of these arrange-ents, the hypothesis that any one given card will come out, has just the same likelihood, to us, as the hypothesis that any other will come out. As regards our knowledge, then, there are fifty-two different results, all equally probable. Suppose we represent certainty by 1, then clearly the *chance*, or measure of *à priori* probability, that any given card come out, would be measured by one fifty-second part of what measures *certainty*, or, on the scale supposed, by $\frac{1}{52}$.—Suppose an urn to be known to contain 15 black and 20 white balls. Then the chance for any one marked ball being drawn is evidently $\frac{1}{20+15} = \frac{1}{35}$. The chance, however, for a black ball being drawn will clearly be the sum of all the chances for each black ball; that is, $\frac{15}{35}$. The chance for a white one will similarly be $\frac{20}{35}$, and the sum of the two chances will be 1, as it ought, since it is certain that upon one drawing, either a black or a white ball will be drawn. The total quantity $\frac{15}{35}$ then informs us, as it stands, of three things—the total number of chances, the number favourable to the given event—the drawing of a black ball, and the absolute value of the chance for its happening. This *latter* will not be altered, though the former become disguised. In fact, $\frac{15}{35} = \frac{3}{7} =$ chance in favour of drawing a black ball. Hence we see what is of great importance, that the thing to be attended to in calculating chance, is to know the *ratio* of the number of favourable events to the total number of events equally probable *à priori.* This ratio is called the mathematical *probability* of the event.—Certain general mathematical principles of the theory can be deduced at once from this definition.

(1.) The probability of an event which may happen according to different hypotheses, the probabilities of which are equal or unequal, is the sum of the probabilities of each hypothesis favourable to the event. Thus, let us suppose N balls in an urn, n being white, n' red, n'' yellow, &c.; and suppose we seek to know the chance of drawing a red, or white, or yellow ball. Then

$\frac{n}{N}$ is the probability of a white,

$\frac{n'}{N}$ is the probability of a red,

$\frac{n''}{N}$ is the probability of a yellow.

And since for the player's purpose all the balls may be considered as belonging to one class,—the red-white-and-yellow class, we have his chance

$$\frac{n+n'+n''}{N} = \frac{n}{N} + \frac{n'}{N} + \frac{n''}{N} =$$

sum of probabilities of separate hypotheses.

(2.) The relative probability of an event is the quotient obtained by dividing its absolute probability by the sum of the absolute probabilities of the events with which it is compared.—Suppose we wish to compare the probability of the player's drawing a white, with that of his drawing a red

or a yellow ball. Then, clearly, we may absolutely neglect all other drawings, and

$$\frac{n}{n + n' + n''}, \quad \frac{n'}{n + n' + n''}, \quad \frac{n''}{n + n' + n''}$$

will be the probabilities of drawing a white, a red, a yellow, respectively, if we neglect the others. But

$$\frac{n}{n + n' + n''} = \frac{n'}{\text{N}} \div \frac{n + n' + n''}{\text{N}} =$$

the quotient obtained by dividing the absolute probability of the event—drawing a white ball—by

$$\frac{n}{\text{N}} + \frac{n'}{\text{N}} + \frac{n''}{\text{N}}, \quad =$$

the sum of the absolute probabilities of the events with which it is compared.

(3.) The absolute probability of an event compounded of two others, the second of which can only happen when the first happens, is obtained by multiplying the absolute probability of the first, by the probability, that if the second happen the first also will.—Thus, suppose we wish the chance that the player will gain by the extraction of a white ball. That depends upon his gaining at all, the probability of which is

$$\frac{n + n' + n''}{\text{N}}.$$

And, the probability that if he gains, he will do so by the extraction of a white ball, is

$$\frac{n}{n + n' + n''}.$$

Hence, multiplying these we have

$$\frac{n + n' + n''}{\text{N}} \cdot \frac{n}{n + n' + n''} = \frac{n}{\text{N}},$$

for the probability according to this rule; which, as we know, *is* the probability of his gaining, by the extraction of a white ball.

(4.) The probability of an event compounded of several independent events, that is, consisting in their *concurrence*, is the product of the probabilities of the simple events.—Thus, suppose two urns, the one containing m white, and m' black balls, and the other n white, and n' black balls. The total number of drawings probable for the one are $m + m'$, for the other $n + n'$. Hence the total number of combinations of a special drawing from the one, with one from the other, is $(m + m') \cdot (n + n')$. Also the total number of combinations of a white ball from the one, with a black ball from the other, will be $m\, n$. Therefore, the probability of drawing two white balls will be

$$\frac{m\, n}{(m + m') \cdot (n + n')},$$

because that is the ratio of the *favourable* to the *total* events. But the probability of the simple events are

$$\frac{m}{m + m'}, \quad \frac{n}{n + n'},$$

and the product of these is

$$\frac{m\, n}{(m + m')\,(n + n')}.$$

Hence the rule.

(5.) The absolute probability of an event which has different probabilities on different hypotheses, is the sum of the compound probabilities obtained by multiplying the probability of each separate hypothesis by the probability of the event upon that hypothesis. This proposition follows, as a natural consequence, of the last. It must not, however, be considered a mere truism. An instance of a not unnatural mistake will show its importance. Suppose two urns, one containing 1 white and 2 black balls, and the other 5 white and 3 black; and suppose the chances of laying one's hand upon either urn to be equal, then, by the rule, the total chance of drawing a white ball is $\frac{1}{2} \cdot \frac{1}{3} + \frac{1}{2} \cdot \frac{5}{8} = \frac{23}{48}$. But it is not unnatural to suppose that since it is an equal chance which urn be drawn from, we could reach the result more rapidly by *mixing* the two urns. This would give 6 white and 5 black balls, and the chance of drawing a white ball would be $\frac{6}{11}$. The true chance, therefore, would be less than $\frac{1}{2}$, the false chance greater.—We have spoken of play and betting, assuming what is an evident mathematical principle, that the stakes ought to be in exact proportion to the chances that each player has of winning. In betting, the players stand in the precise condition of men about to draw from the urn, supposing their *knowledge of the contents of the urn* to be equal. If both equally knew the whole data on which judgment is to be formed, a fair bet would be made. Even supposing that made, we shall see hereafter that, in consideration of moral advantage, a bet upon such equal terms is of necessity a dead loss to either party before the event; and, that where the play is to be continued for a certain time, it may be shown to be nearly certain that one of the players will be ruined. In such games as rouge et noir, lotteries, and the like, and—as in principle, although otherwise in their moral aspect, they are the same—in life and fire assurances, a considerable unfairness of betting takes place always in favour of the banker, the holder of the lottery or the assurance company, so as to cover expenses of management.—Before passing from these laws of the composition of chances, it may be well to illustrate their importance. Thus it is found in Brabant that the probability that the barometer will fall before rain, is $\frac{759}{1177}$. Hence the chance that for the three next successive rains their coming will be indicated by the barometer, is $\frac{759}{2177} \times \frac{759}{1177} \times \frac{759}{1177} = \frac{1}{5}$ nearly, that is, although the probability is about $\frac{3}{5}$ that any one fall will be preceded by rain, it is about $\frac{1}{5}$ only that three successive ones will so be.—Similarly, suppose we seek the probability that a

man aged forty, and his wife aged thirty, will both be alive at the end of 10 years. The Belgian tables give, for the probability of a man living in the towns and aged forty, living 10 years ·832, and for his wife aged thirty, ·862. Hence the probability that they will both be alive is ·832 × ·862 = ·717. Consider, indeed, all the possible cases:—

The husband and wife will be both alive	·832 × ·862 = ·717
They will be both dead....	(1— ·832) (1— ·862) = ·023
The husband will be alive and the wife dead	(1— ·862) × ·832 = ·115
The husband will be dead and the wife alive	(1— ·832) × ·862 = ·145
Total probabilities = certainty	= 1·000

We may note how slight the chance of both dying within 10 years is; not more than 1 chance in 43.

II. From the Law of the composition of chances, we may pass to consider the *repetition of events*, which depends upon the composition of *equal* chances.—Let p', q' be the chances of one event happening and failing,—p'', q'' be the chance of another event happening and failing,—p''', q''' be the chance of a third event happening and failing, and $p^{(m)}$ $q^{(m)}$ be the chance of an mth. Then, clearly, $(p' + q')$ $(p'' + q'')$ will, when developed, give in its different terms all the four chances arising from combination of the first two events or non events. Thus $p' p''$ is the chance of both happening, $p' q''$, $q' p''$, those of the first or second happening and the other failing, $q' q''$, that of both failing together. Similarly we may see that $(p' + q')$ $(p'' + q'')$ $(p''' + q''')$..... ....$(p^{(m)} + q^{(m)})$, &c., is a product, every expression of which will give the chance of the events indicated by the notation mark I, II, III... ......(m), happening or failing, according as they are over p or q. Thus $p' q'' p'''$.......$q^{(m)}$ gives the chance of the first happening, the second failing, the third happening .... the mth failing. —We have spoken of repetitions of events. Suppose the second event, the third, &c., to be all the same event in reality as the first. Then, if p q be the chances for one, they will be the chances for all; and if these are m events, all the same we have $(p + q)$ $(p + q)$ $(p + q)$, ... m times $= (p + q)^m$, as the quantity expressing all the possible happenings or failings. That is, if we repeat the same event m times, p q being the chances for its happening and failing in each trial, $(p + q)^m$ will be an expression giving the total chances of happening and failing any definite number of times.—But here a question arises. In the development of $(p + q)^m$, we have such a term as $m\, p^{m-1}\, q$, that is, we have the event $p^{m-1}\, q$ corresponding to the development $(p' + q')$ $(p'' + q'')$.........$(p^{(m)} + q^{(m)})$, m times repeated. If we write down the product of this fully, and then substitute p q for p' q', &c., we shall see the meaning of this. All the results where there is only one event failing are $p'\, p''\, p'''$............$q^{(m)} + p'\, p''\, p'''$............ $q^{(m-1)}\, p^{(m)}$.......$p'\, q''\, p'''$......$p^{(m)} + q'\, p''\, p'''$, ...$p^{(m)}$. Substitute p, q, for p', q', &c., and we have $p\,p\,p$......$q + p\,p\,p$......$q\,p$...... $p\,q\,p$...... $p + q\,p\,p$.....p. The first event, then, represents the chance of the first m—1 events happening, and the m^{th} failing,—the next, of the first m—2 happening, the m—1th failing and the last happening, and so on. Each of the terms is evidently equal to $p^{m-1}\, q$, which, therefore, represents the chance of the given event happening m —1 times and failing once, *in any assigned order.* Again, since in all of them we have m—1 happenings and *one* failing, and since these are all the terms in which we have that, the sum of the chances for each will be the total chance for the whole,—that is, since there are m terms, $m \,.\, p^{m-1}\, q$, is the chance of the events happening m— 1 times and failing once, in *any order whatever.* Because this is an important distinction, we shall present it in yet another form. The chance of an event happening m—n times and failing n times, when the chances are p and q each time, *in any order whatever that may be assigned,* will evidently be p multiplied by itself m—n times, multiplied by q multiplied by itself n times—that is, $p^{m-n}\, q^n$. Hence the chance of its happening *so at all, without respect to order,* will be $p^{m-n}\, q^n$, multiplied by the number of ways in which we may have m quantities, divided into two distinct groups of m—n, and n quantities, arranged. This, as is seen in PERMUTATIONS, is

$$\frac{\angle\, m}{\angle\, n.\ \angle\, m-n.}\ *$$

Hence $\dfrac{\angle\, m}{\angle\, n\ \angle\, m-n}\, p^{m-n}\, q^n$ is this chance.

But this is the $(n + 1)^{th}$ term in the binomial development of $(p + q)^m$, and there are $m + 1$ terms; therefore the successive terms of that development, express the chances of m—n events happening and n events failing, *in any order whatever*, and, when stript of their co-efficients, of m—n happening and n failing, *in any order assigned.* We shall illustrate this by a problem celebrated in the history of the science, as being the first occasion of the researches in this direction of Pascal—its true founder. It is asked how many trials—repetitions of events—it is necessary to have, in order to have a probability ½ that we shall, at least once, have two sixes in *tric-trac*, which consists in the throwing of two dice.

* The product of all the natural numbers 1·2·3..m is written shortly thus $\angle\, m$. Where m is a large number it is not common, or indeed easy, to calculate this, but we use this formula of approximation $1\cdot 2\cdot 3..x = x^x\, \varepsilon^{-x} \sqrt{2\pi x}\left(1 + \frac{1}{12x} + \frac{1}{288}\,\frac{2}{x}\right)$ &c., where ε is the base of Naperian logarithms; and this may be further approximated to, thus $1\cdot 2\cdot 3\ldots x = x^x\, \varepsilon^{-x} \sqrt{2\pi x}$.

Clearly this is the same as asking how many are needful to give the probability $1 - \frac{1}{2}$, of failing to get two sixes out of all the throws. The expression $(p + q)^m$ represents all the possibilities. Now p, the chance of obtaining two sixes $= \frac{1}{6} \times \frac{1}{6} = \frac{1}{36}$, therefore $q = 1 - p = \frac{35}{36}$. Hence q^m, the probability of failure in all the trials $= \frac{1}{2}$,

$$m \log q = - \log 2$$

$$m = \frac{- \log 2}{\log 35 - \log 36} = \frac{\log 2}{\log 36 - \log 35}$$

$= 24 \cdot 6$. Hence in 25 throws there will be a probability rather greater than $\frac{1}{2}$, in 24 rather less.

III. The problem of chances known *à priori and constant*, might be held to be now completely solved. There are certain subsidiary questions, however, which the necessities of practical discussion, will discover to be of more real importance even than these general principles. We shall find these evolve themselves successively from the consideration of several points which at first sight seem mere matters of curiosity. Thus, we ask, which of the whole number of events—m—n successes, and n failures, is *the* most probable. The two successive general terms are,

$$\text{the } n\text{th}, \frac{\lfloor m}{\lfloor n-1 \lfloor m-n+1} p^{m-n+1} q^{n-1}$$

$$\text{the } (n+1)\text{th}, \frac{\lfloor m}{\lfloor n \lfloor m-n} p^{m-n} q^n$$

Hence the $n + 1$th will be greater than the nth of their ratio which is

$$\frac{q}{p} \cdot \frac{m-n+1}{n}, \text{ be greater than } 1$$

that is, if

$$\frac{q}{p} \left(\frac{m+1}{n} - 1 \right) \text{ be greater than } 1.$$

Now, this is a quantity which for successive values, 1, 2, 3, &c., of n will grow less and less, hence, if we can get the last quantity for which

$$\frac{q}{p} \left(\frac{m+1}{n} - 1 \right) \text{ is greater than } 1,$$

we shall have the $n + 1$th term as the greatest. Restore the original form

$$\frac{q}{p} \cdot \frac{m-n+1}{n} > 1$$

$$\frac{m-n+1}{n} > \frac{p}{q}$$

And if $\frac{m-n}{n} = \frac{p}{q}$, or if *$m$ be divided into two parts so as to be in the proportion of* p *and* q, *we shall have the equation satisfied, and the term in which we have* m — n *successes and* n *failures will be the greatest.* Hence, if the chances for p and q be equal, the greatest is the *middle* term—or the two middle terms—it being well known that where there are two middle terms in the expansion of the binomial they are equal. Suppose then $m - n = p\, m$, $n = q\, m$. Then

$$\frac{q}{p} \cdot \frac{m-n+1}{n} = \frac{q}{p} \cdot \frac{p\,m+1}{q\,m}$$

$$= \frac{m + \frac{1}{p}}{m} = 1 + \frac{1}{p\,m}$$

a quantity which is nearer *unity* the greater m is. Hence where the number of repetitions is very great, we have the terms nearest the greatest term, really nearer it than when the number of repetitions is small. We see then that for a great number of repetitions we shall have a *massing of the most probable events* round the greatest term, increasingly so as m increases. In order that we may have a clear idea of the importance of this result, we shall suppose an urn in which there are two white balls and one black. Suppose six repetitions; $(\frac{2}{3} + \frac{1}{3})^6$ gives the following, in which the upper numbers are for the white balls.

6	5	4	3	2	1	0
$\frac{64}{729}$	$\frac{192}{729}$	$\frac{240}{729}$	$\frac{160}{729}$	$\frac{60}{729}$	$\frac{12}{729}$	$\frac{1}{729}$

We see, then, that the result, four white and two black balls, for which, six is divided in the ratio of the chances, $\frac{2}{3}$ and $\frac{1}{3}$ is *the* most probable, and that it, with the two next, have a total probability of $\frac{592}{729}$ or nearly $\frac{4}{5}$. The massing near the greatest term is thus very distinct. We have taken three terms out of seven. Let nine repetitions be now made, we shall find the following results:—

9	8	7	6	5	4
$\frac{512}{19683}$	$\frac{2304}{19683}$	$\frac{4608}{19683}$	$\frac{5376}{19683}$	$\frac{4032}{19683}$	$\frac{2016}{19683}$

3	2	1	0
$\frac{672}{19683}$	$\frac{144}{19683}$	$\frac{18}{19683}$	$\frac{1}{19683}$

The greatest term again is 6, 3, where the division of the nine results is in the proportion of $\frac{2}{3}$ and $\frac{1}{3}$, and taking three out of ten—the greatest term and those which succeed and follow it, we have nearly $\frac{7}{10}$ of the whole probability in favour of one or other of the drawings 7, 6, 5. Let ninety repetitions be made. Then we write down the value of the five greatest terms

62	61	60	59	58
·081817	·087460	·088918	·086049	·079327

So that, in favour of the actual drawing being one of these five out of the whole ninety drawings we have the probability ·423571, or very nearly an even chance. We see then that the actual probability of *the* most probable result, 4, 6, 60 white balls, *decreases* as the number of repetitions increases, but that there is an *increasing* probability that the result found will be one of

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those very near that. By purely mathematical reasoning then we see how the following principles may be obtained.—1. When the occurrence of an event depends on a chance, and the number of trials is repeated, the most probable result is, that where the proportion of actual events W (*e.g.*, white balls) to that of actual events B (*e.g.*, black) is equal to the proportion of probability for A and B respectively, or as near that as possible,—the probabilities of the other events go on diminishing; and the more rapidly, the farther they pass on one side from this in arrangements according to the number of the events W, or of the events B.—2. According as the number of repetitions increases, the probability of each separate value decreases; but the more rapidly, the farther it passes from the most probable value, the more slowly, the nearer it is to that.—3. Consequently, there is an always increasing probability that the ratio of the number of events, W to that of B will not differ from the ratio of the respective probabilities of these events beyond certain definite limits; and however close these limits be taken we may increase the number of trials, so that the probability of the result being within these limits shall be as near 1, that is, *certainty*, as we may choose. These propositions—the last of which is the basis of the whole practice of probabilities—are due to James Bernouilli, and were published in his *Ars Conjectandi*, 1713. We may represent his conclusions, and the law of probability, graphically in the following way:—Suppose m trials, then there are $m + 1$ results. Take then any line and divide it into m equal parts, and at each division raise a perpendicular to the line, proportional to the chance corresponding. Thus, the curve of possibility for $(p + q)^m$ will have the first ordinate at the extremity of the line proportional to p^m, the second, at the first division, to $m\ p^{n-1}\ q$, and so on. The result will then be a series of ordinates like those in the figure. The tops may be joined and then

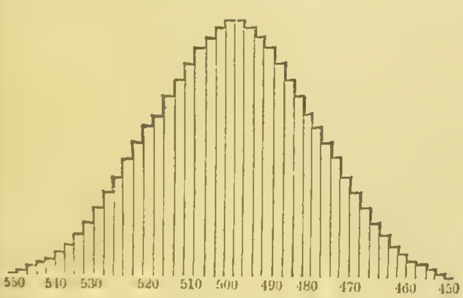


Fig. 1.

we shall have a polygon approximating to a curve, which may be drawn as a continuous line through the summits of the ordinates. Now a geometrical curve can really be found which does in this way pass through these successive summits of ordinates. The curve in the figure is for $p = q = \frac{1}{2}$, and is therefore symmetrical about the maximum ordinate. If the number of trials be increased, the values of p and q remaining fixed, the curve will slope

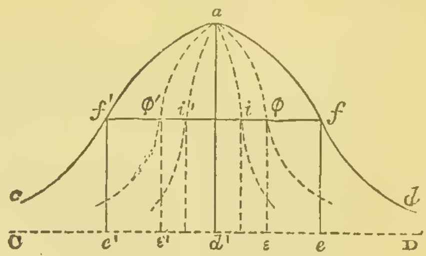


Fig. 2.

in towards the maximum ordinate—in the proportion *of the square roots of the number of trials.* Thus, if $c\ f'\ a\ f\ d$ be the curve of probabilities (not carried out for want of space to the extreme ends where it lies very close to the base line), for m trials, then $\varphi'\ a\ \varphi$ will represent it for $4m$ trials, if $e'\ d' = 2\ \varepsilon'\ d'$, wherever the lines parallel to abscissæ are drawn in the two curves. So if $i'\ a\ i$ hold the same relation to $\varphi'\ a\ \varphi$ that $\varphi'\ a\ \varphi$ does to $c\ f'\ a\ f\ d$, this will be the curve of possibility for $16m$ trials, and so on.—From this graphical representation we learn how we may represent the chances that an event will be of the character $m\ p$ of W, $m\ q$ of B, or within certain limits of closeness to that. Suppose that the division e' corresponds to $m\ (p - l)$, $m\ (q + l)$, and that e corresponds to $m\ (p + l)$, $m\ (q - l)$, then the chance that the result will fall within the limits $e'\ e$, will manifestly be represented by the ratio which space $e'\ f\ a\ f\ d$ bears to the whole space enclosed by the curve and the base line. This may be comprehended at once by the last figure, where one readily sees that the sum of the ordinates between e' and e, corresponds to the sum of the chances, or the total chance, that the number of W events will be within $m\ (p - l)$ and $m\ (p + l)$, and the sum of all the ordinates, to certainty. It is also clear since all the divisions are equal, that the sum of the ordinates between $e\ e'$ bears to the sum of all the ordinates the ratio which the area $e'\ f'\ a\ f\ e$ bears to the whole area of the curve. What is called the *probable error*, is that value of l, for which $e'\ f'\ a\ f\ e$ is half of the whole area. The maximum ordinate, in the case of $p = q = \frac{1}{2}$ is evidently according to the rules laid down, the one in the centre, and the curve is perfectly symmetrical around it. On the other hand, if p is not equal to q, the maximum ordinate will not be in the centre nor will the curve be symmetrical. Thus if $p = 50$, the curve will be that to which ·50 is attached in fig. 3, and so if it be ·60, ·70, ·80, ·90, ·95;—a series of figures from which it is graphically evident that the vertical axis will come to be an asymptote to the last curve, that is, that as the value of p approaches nearer and nearer 1·00, the height of the curve will approach infinity. It is

supposed that in all of them the same base is taken. Hence another law becomes evident gra-

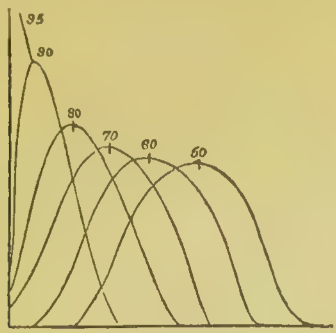


Fig. 3.

phically, that the more p becomes unequal to q, the smaller is the probable error.—We may lay down laws which fully express those facts. The complete investigation of these laws we cannot give here. If

$$t = l\sqrt{\frac{m}{2\,p\,(1-p)}},$$

then the probability that the result will be between $m\ (p - l)$ of W and $m\ (p + l)$ of W, can be expressed in the following formula:—

$$\text{P} = \frac{2}{\sqrt{\pi}}\int_0^t \varepsilon^{-t^2}\,d\,t + \frac{\varepsilon^{-t^2}}{\sqrt{2\,\pi\,p\,(1-p)\,m}}$$

or, throwing off the last term which is not so important;

$$\text{P} = \frac{2}{\sqrt{\pi}}\int_0^t \varepsilon^{-t^2}\,d\,t$$

In fact the equation of the curve of probability for large numbers m, may be expressed by the formula

$$y = \frac{\varepsilon^{-x^2}}{\sqrt{\pi}}$$

and the value of P, is the proportion of

$$\frac{2\int_0 y\,d\,x}{\int_{\infty}^{\infty} - \infty\, y\,d\,x}$$

that is, of the portion of area between $m\ (p - l)$ and $m\ (p + l)$. We shall assume these results as established. Hence, to get the probable error—that is, the exact point—within which it is an even bet that the divergence from the most probable result of all lies, we have simply to solve the equation

$$\frac{1}{2} = \frac{2}{\sqrt{\pi}}\int_0^t \varepsilon^{-t^2}\,d\,t$$

$$\therefore\ \frac{\sqrt{\pi}}{4} = \int_0^t \varepsilon^{-t^2}\,d\,t.$$

One method of integrating this result by development is very evident. Hence

$$\frac{\sqrt{\pi}}{4} = \int_0^t \left(1 - t^2 + \frac{t^4}{1\cdot 2}\ \&c.\right) d\,t.$$

$$= t - \frac{t^3}{1.3} + \frac{t^5}{1\cdot 2\cdot 5} + \frac{t^7}{1\cdot 2\cdot 3\cdot 4\cdot 7.} + \&c.$$

a series which very soon gives a very close approximation for t, if t be < 1. If $t > 1$, other methods must be taken. The value of t is found to be ·479. It is clear also that by assigning any values to t we can find what $\int_0^t \varepsilon^{-t^2}\,d\,t$, becomes, and from that the corresponding value of P. We may tabulate these corresponding values—which is done at the end of most treatises on the subject; and thus for any given probability find the corresponding value of t, or for a given value of t find the probability of the errors being within assigned limits. We find then that t depends on the value of l, m, p. In the first place, it varies directly with the limit of error l, and inversely as the square root of the number of trials m. Also $p\ (1 - p)$ is a maximum for $p = \frac{1}{2}$; therefore for given values of t, or p, and m, the value of l is less, the more unequal p is to $\frac{1}{2}$, and is maximum at $p = \frac{1}{2}$. We write down several values from the table of values of the function $\text{P} = \frac{2}{\sqrt{\pi}}\int_0^t \varepsilon^{-t^2}\,d\,t$, so as to illustrate the subject.

t	P	Differences.	t	P	Differences
·01	·01128	·01128	1·60	·976348	·000887
·10	·11246	·01118	1·70	·983790	·000638
·20	·22270	·01086	1·80	·989090	·000450
·30	·32863	·01034	1.90	·992790	·000311
·40	·42839	·00964	2.00	·9953223	·0002109
·50	·52050	·00883	2·10	·9970206	·0001401
·60	·60386	·00792	2·20	·9981371	·0000912
·70	·67780	·00696	2·30	·9988568	·0000582
·80	·74210	·00600	2·40	·9993115	·0000364
·90	·79691	·00506	2·50	·99959305	·00002234
1·00	·84270	·00429	2·60	·99976396	·00001342
1·10	·88020	·00340	2·70	·99986567	·00000791
1·20	·91031	·00270	2·80	·99992499	·00000457
1·30	·93401	·00211	2·90	·99995890	·00000258
1·40	·95228	·00161	3·00	·999977909	
1·50	·966105	·001207	4 00	·999999985	

The differences are given for differences of ·01′; and the calculation for value of t between those given in the table, may be made as with logarithms. Example 1.—Suppose then we have 1,000 trials, or repetitions of an event, and that we know the antecedent probabilities of the event W to be $\frac{2}{5}$, and those of B $\frac{3}{5}$, and suppose we seek the probability P that the event which actually occurs will be somewhere between 1,000 $(\frac{2}{5} - \frac{3}{100})$ and 1,000 $(\frac{2}{5} + \frac{3}{100})$ of W, that is between 370 and 430 of W.

$$\text{Then } t = l\sqrt{\frac{m}{2\,p\,(1-p)}} =$$

$$\frac{3}{100}\sqrt{\frac{1000}{2\,\frac{2}{5}\cdot\frac{3}{5}}} = \frac{3}{100} \times \frac{10\cdot 5.}{2} \sqrt{\frac{10}{3}}$$

$\therefore t = \frac{3}{4}\sqrt{\frac{10}{3}} = \frac{3}{4} \times 1{\cdot}8$ nearly $= 1{\cdot}36$ nearly. Hence, using the table 1·30 gives ·03401, and to this may be added as an approximation for the value ·06; 6 times the mean of the differences for 1·30 and 1·40, that is 6 × ·00186 = ·01116.—Hence P = ·93401 + ·01116 = ·94517, that is a probability of 189 in 200 nearly, that the result will lie within the limits mentioned. Example 2. Suppose, again, we want to know the limits within which it would be safe to bet 2 to 1 that the result will fall in the case supposed. Then P = ·666·, and P + ·67780, corresponds to $t =$· 70. Hence, proceeding as in logarithms,

$$\begin{array}{r} \cdot 67780 \\ \cdot 66666 \\ \hline \end{array}$$

$$\cdot 00696\overline{)01113}\cdot(2 \text{ nearly.}$$

Therefore for P = ·666·, $t =$ ·68 nearly.

Therefore $.68 = l\sqrt{\frac{m}{2p(1-p)}} =$

$$l\sqrt{\frac{1000}{2\cdot\frac{2}{5}\cdot\frac{3}{5}}} = l\frac{50}{2}\sqrt{\frac{10}{3}} = 45\, l.$$

Hence $l = \frac{.68}{45} = \frac{.075}{5} = {\cdot}0151 + \frac{1}{60}$ nearly.

Therefore a bet of 2 to 1 may be taken that the result will fall within $(\frac{2}{5} - \frac{1}{60}) \times 1000$, and $(\frac{2}{5} + \frac{1}{60}) \times 1000$, that is between $\frac{2300}{6}$ and $\frac{2500}{6}$ or 383 and 417. Since, then, all events from 0 of W, to 1000 of W are possible, we see how the greatest values are massed round the one which is *the* most probable, because between 383 and 417, that is on about $\frac{1}{30}$ of the base line of the curve of probability, $\frac{2}{3}$ of the whole area is built. The curve then must come to lie extremely close to the base line, and at the end nearly coincide with it.—Another question suggests itself. In our standing supposition of the *urn*, where the drawings are repeated, we supposed either *different urns*, or if the *same urn* was used, the balls drawn were thrown back into the urn so as to bring the new drawing to be made in the same circumstances as the preceding one. But, suppose that the balls are not thrown back. Let a, b be the number of the white and black balls respectively. Then the probability of W W B being drawn is

$$\frac{a}{a+b} \times \frac{a-1}{a+b-1} \times \frac{b}{a+b-2}.$$

Similarly the probability of W being drawn $m-n$ times, B, n times, in any order will be

$$\frac{a.\ a-1 \ldots\ldots a-(m-n)+1}{a+b.\ a+b-1 \cdot a+b-(m-n)+1}$$
$$\frac{b.\ (b-1) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots b-n+1}{a+b-(m-n) \ldots\ldots a+b-m+1}$$

Also, the chance of drawing in any one order is the same as that of drawing in any other order. Hence, since the number of combinations is $\frac{<m}{<m-n.<n}$, we have the total chance of $m-n$ W, and n B being the resultant event.

$$\frac{<m}{<m-n<n} \cdot \frac{a \,.\, \ldots\, a-(m-n)+1}{a+b \ldots\ldots\ldots\ldots} \cdot$$
$$\frac{b \ldots\ldots\ldots b-n+1}{a+b-m+1}$$
$$= \frac{<m}{<m-n.<n} \cdot \frac{<a}{<a-(m-n)} \cdot$$
$$\frac{<b}{<b-n} \cdot \frac{<a+b-m}{<a+b}$$
$$= \frac{<m}{<n.<m-n} \cdot \frac{<a+b-m}{<a-(m-n).<b-n}$$
$$\cdot \frac{<a.<b}{<a+b}$$

or putting $m-n=n'$, and therefore $m=n+n'$.

$$P = \frac{<n+n'}{<n.<n} \frac{<a+b-(n+n')}{<a<-n'<b-n)}$$
$$\frac{<a.<b}{<a+b}.$$

A remarkable theorem may be proved here, not from its relation to the theory of probabilities, but as illustrating how frequently problems pertaining to the abstract sciences of number and quantity can be solved by the temporary introduction of ideas foreign to the abstraction. Thus, the total probabilities of drawing 0 W's, 1 W, &c., up to m W's, will be certainty, or 1. Therefore, 1 =

$$\left(\frac{a \cdot a-1 \cdot a-2 \ldots\ldots a-m+1}{(a+b)(a+b-1)(a+b-2) \ldots a+b-m+1}\right.$$
$$+\frac{m}{1}\frac{\cdot a \cdot a-1 \ldots\ldots\ldots\ldots\ldots a-m+2 \cdot b.}{a+b \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots a+b-m+1}$$
$$\left.+\frac{m \cdot m-1 \ldots n+1}{1 \cdot 2 \cdot 3 \cdots m-n} \cdot \frac{a \cdot a-1 \ldots a-n+1}{}\right)$$
$$\frac{\cdot b \cdot b-1 \ldots\ldots b-(m-n)+1}{a+b \ldots\ldots\ldots\ldots a+b-m+m\,1} \ldots\ldots\ldots$$
$$+\frac{b \cdot b-1 \cdot b-2 \ldots\ldots b-m+1}{a+b \ldots\ldots\ldots\ldots a+b-m+1}.$$

Hence

$$(a+b)(a+b-1)(a+b-2)(\ldots\ldots\ldots)$$
$$(a+b-m+1) = a \cdot (a-1) \ldots (a-m+1)$$
$$+\frac{m}{1} \cdot a \cdot a-1 \ldots\ldots\ldots a-m+2 \cdot b \ldots\ldots$$
$$+\frac{m.m-1 \ldots\ldots n+1}{1 \cdot 2 \cdot 3 \ldots\ldots m-n} \cdot \frac{a \cdot a-1 \ldots a-n+1}{} \times$$
$$b \cdot b-1 \ldots\ldots b-(m-n)+1 \ldots\ldots\ldots\ldots$$
$$+\, b \cdot b-1 \ldots\ldots\ldots b-m+1.$$

The analogy of this theorem to the binomial, with the mere substitution of factorials for powers is sufficiently evident.—This case is not, however, of sufficient importance for

us to investigate it farther. When we consider observations of nature, to be like the drawing of balls from an urn, it is evident that the number of balls is infinite. Hence it makes no matter whether we do or do not return the ball to the urn, and the whole theory of balls not returned is so much simpler, and more manageable, that we may apply it solely.

IV. We have spoken of what is called the *moral* value of an expectation as distinguished from its mathematical value. Contemplate the following two cases:—A person of moderate fortune risks £500 for the sake of gaining £5, where the chances are 100 to 1 in his favour; and, again, risks £5 to gain £500, where the chances are 100 to 1 against him. It appears clear enough that his conduct would be considered in the former case reckless and foolish, although it might not in the latter. Yet, in the mathematical theory, the stake is quite equal, in both cases, to the expectation. Again, a man possesses a lottery ticket, suppose the last of two undrawn, one of which (unknown which) is a prize of £20,000. His expectation is therefore worth £10,000. Yet, to a man of moderate fortune, evidently £10,000 in hand would be much more valuable than the chance of £20,000, which is mathematically equal to it. Evidently, although the actual sum gainable or losable should be the same, the privations which loss would entail, would, in many cases, greatly outweigh the additional advantages which gain would secure. It follows, therefore, that to make the theory practical, we must take into account these *relative values* of Expectation, as distinguished from the absolute mathematical values. But how is it possible to do this satisfactorily? It is clear that we cannot state the whole data in any case—that we must take some in so far arbitrary approximation to what we may consider the effect of these moral and relative considerations. That which appears most natural and most generally applicable is, that the relative value of any very small increment or decrement of fortune is directly proportional to its absolute value, and inversely to the fortune of the person who expects it. Thus the relative value of $d\,x$, to a person whose fortune is x, is $\frac{c\,d\,x}{x}$ where c is a constant, determinable by the nature of the question. If then we suppose y the relative or moral value of the fortune which arises from little increments $d\,x$, taken together $y = \int \frac{c\,d\,x}{x} = c \log x +$ constant. Suppose that $y = o$, when $x = a$, that is, that at commencement the fortune actually possessed is a. Then $o = c \log a +$ constant, therefore $y = c\,(\log x - \log a) = c \log \frac{x}{a}$.

From this we may deduce a numerical expression for the value of a moral expectation. Suppose a the original fortune, and α, β, γ, &c., sums to be received on occurrence of certain events, E, F, G, &c. Then, if α happens, the fortune becomes $a + \alpha$, and the relative value, according to the formula, is $c \cdot \log \frac{a+\alpha}{a}$. So on for the other quantities. Suppose p, q, r, &c., to be the probabilities of each event, where $p + q + r \times$ &c., $= 1$, that is, where one or other of the events must happen. Then if Y represent the relative fortune arising from the expectation

$$\text{Y} = c \cdot \left(p \log \frac{a+\alpha}{a} + q \log \frac{a+\beta}{a} + r \log \frac{a+\gamma}{a} + \text{\&c.} \right)$$

Suppose χ to denote the absolute value of Y. Then $\text{Y} = c \log \frac{\chi}{a}$. Therefore $\log \frac{\chi}{a} = p \log \frac{a+\alpha}{a} + q \log \frac{a+\beta}{a} + r \log \frac{a+\gamma}{a} +$ &c.

$$\frac{\chi}{a} = \frac{(a+\alpha)^p \times (a+\beta)^q \times (a+\gamma)^r \text{ \&c.}}{a^p\, a_q\, a^r \text{ \&c.}}$$

$$\chi = (a+\alpha)^p\,(a+\beta)^q\,(a+\gamma)^r \text{ \&c.}$$

If α, β, &c., be very small in comparison with a, so that $\frac{\alpha}{a}$, $\frac{\beta}{a}$, &c., may be neglected for powers higher than the first, we have

$$\chi = a^{p+q+r+\text{\&c.}} \left(1 + \frac{\alpha}{a}\right)^p \left(1 + \frac{\beta}{a}\right)^q \left(1 + \frac{\gamma}{a}\right)^r \text{\&c.}$$

$$= a \left\{ 1 + \frac{p\,\alpha + q\,\beta + r\,\gamma + \text{\&c.}}{a} \right\}$$

$$\chi - a = p\,\alpha + q\,\beta + r\,\gamma + \text{\&c.}$$

This then is the value of the expectation, or the sum equivalent to the moral advantage. This is also equivalent to the mathematical advantages. Hence these two are in this case equal.

From these formulæ we shall establish certain extremely interesting conclusions. Suppose A, whose fortune is 100 crowns, to bet 50 crowns with B, on the issue of an event, the probability of which is $\frac{1}{2}$, on terms that if the event happen, A is to receive 50 crowns; if it fail, to pay 50 crowns, it is required to find the relative value of A's fortune after the bet. Employing the formula $\chi = (a+\alpha)^{\frac{1}{2}} \times (a+\beta)^{\frac{1}{2}}$ to this case we have $\chi = (100 \times 50)^{\frac{1}{2}}\,(100 - 50)^{\frac{1}{2}}$

$= \sqrt{150 + 50} = 87$, so that, morally A's fortune is less worth by 13 crowns after the bet. Of course this must be considered as simply

an estimate of the extent of the excess of the privations which loss would entail over the advantages which would accrue from gain.—We may show always, that, with a limited capital, any player—*and therefore both players*, are morally disadvantaged by making a bet on equal terms. Let the chance of an event be p, and let s be staked. Then

$$x = (a + q\,s)^p\,(a - p\,s)^q. \text{ Hence } \log\frac{x}{a} =$$

$$x = p\log\left(1 + q\,\frac{s}{a}\right) + q\cdot\log\left(1 - \frac{p\,s}{a}\right)$$

$$\therefore d\cdot\log\frac{x}{a} = p\,q\,d\,s \times$$

$$\times\left(\frac{1}{1 + q\,\frac{s}{a}} - \frac{1}{1 - \frac{p\,s}{a}}\right)$$

which is a negative quantity. Therefore $\log\frac{x}{a}$ which is the integral of $d\cdot\log\frac{x}{a}$, being made up of negative quantities, is itself negative. Hence x is less than a. We draw from this, then, the conclusion, that, in all cases of limited fortune (a finite), a bet made upon perfectly equal terms makes the better's fortune, of necessity, before the event, worth less to him than it was before the bet.

Another consequence is, that when property of any kind is exposed to risk, it is more advantageous to expose it in several parts to several independent risks, than to expose the whole at once, though the probability of loss be the same in both cases. Take this example. Suppose a merchant to have a capital of £4,000, besides goods worth £8,000, to be transported by sea. Then suppose the probability of loss of a ship to be $\frac{1}{10}$, required the worth of the expectations in the two following cases:—1st, If the goods be all embarked in one ship. 2d, If they be embarked in equal portions in two ships. In the first case

$$x_1 = (4,000 + 8000)^{\frac{9}{10}}\,(4,000)^{\frac{1}{10}} = 10,751$$

Hence the worth of his expectation is £6,751.

$$\text{Again, } x_2 = (4,000 + 8,000)^{\frac{81}{100}} \times (4,000 + 4,000)^{\frac{18}{100}}\,(4,000)^{\frac{1}{100}} = 110,33.$$

Hence the worth of his expectation is £7,033, a sum greater than the former by £282. We could show similarly that as the number of ships is increased, the moral expectation approaches its highest limit, the mathematical expectation $=$ £8,000 $\times \frac{9}{10} =$ £7,200. Of course there is a practical limit to the subdivision of risks somewhere, in the increased expense of management.—There are also important questions as to the wisdom and advantage of assuring. Of course it is only on this principle of moral expectation that assurances can be valuable to the assurer. The principles upon which these rest are shortly as follow:—The company start with a very large capital, and insure a great many risks. The former is necessary to prevent any fortuitous drain from exhausting their whole stock. In consequence of the latter—the repetition of a great many trials—they may have, according to Bernouilli's third law, any amount of probability—by continuous operations—that the actual results will approach their calculated results within given limits of error; hence they may have any amount of probability that their gains will not exceed a certain amount £A, nor be less than a certain amount £B in a given time. If £B be the actual expense of management the company may work with perhaps more perfect security than any merchant firm, by taking a very high probability that their profits will not be less than £B. Of course it is clear that to have gain and gain, instead of gain and loss as the limiting conditions, the company must bet at mathematical advantage—since mathematical equality of betting represents the maximum ordinate of the curve of probability around which the limits for error group. So much for the company. But, according to the theory of moral expectation, an assurer also may assure in certain cases, paying an amount greater indeed than the mathematical expectation, that is, fulfilling the conditions of the company's stability, and yet securing a real moral advantage for himself. There are three chief questions which suggest themselves:—1. The maximum premium which the assured may pay without disadvantage. 2. The ratio of his fortune to the value of the sum risked, which makes it advantageous to insure at a given premium. 3. The capital which the assurance company ought to have to be able to insure a given risk with probable advantage to itself, and with safety to the insured. Suppose s the value of a cargo, p the probability of the vessel's arriving safely, and a the merchant's independent capital. Then, evidently $q\,s$ is the mathematical value of the premium for insurance. If the merchant insure, then this absolute fortune is $a + s - q\,s = a + p\,s$. If he do not insure, it is $(a+s)^p\,a^q$. Hence it will be advantageous, or the opposite to insure according as $(a + s)^p\,a^q$ is less or greater than $a + p\,s$. Now $\log\,a + p\,s)$ is the integral of $\frac{p\,d\,s}{a + p\,s}$ and $\log\left((a + s)^p\;a^s\right) = p\log(a + s) + q\log a$, the integral of $\frac{p\,d\,s}{a + s}$, the former of which fractions is greater than the latter, and therefore $(a + p\,s)$ is greater than $(a + s)^p\,a^q$. Hence, generally, the former is greater than the latter, that is, it is a real advantage to insure at the premium which answers the mathematical value of the expectation. This will serve as sufficient indication of the method of solving all the three questions asked.—There is a different expression

used by Buffon for the moral expectation (the one we have given is Bernouilli's) which consists in making it proportional directly to the gain and inversely to the fortune, that fortune being estimated all through as the same as at first, and not as being every instant increased by little increments. This has seldom been employed.

V. We pass to the fifth great division of the subject, where we ask, what is the probability from the actual occurrence of certain results, that the causes of these are such and such. Thus, suppose an urn to contain four counters, which are known to be white or black—and that in four successive drawings—the ball drawn being always replaced—a white ball has been drawn three times, a black one once, what are the probabilities of the various inferences as to the contents of the urns. There are three hypotheses here possible—1. That the urn contains three white and one black ball.—2. That it contains two white and two black.—3. That it contains one white and three black; the others being excluded by the actual results of the drawing. On the first hypothesis $p = \frac{3}{4}, q = \frac{1}{4}$, and the probability is the term of $(p + q)^4$ which contains $p^3\ q$, *i.e.*, $4\ p^3\ q = 4 \cdot \frac{27}{64} \cdot \frac{1}{4} = \frac{27}{64}$. On the second hypothesis $p = \frac{1}{2},\ q = \frac{1}{2}$, and $4\ p^3\ q = 4 \cdot \frac{1}{8} \cdot \frac{1}{2} = \frac{16}{64}$. On the third $p = \frac{1}{4}, q \frac{3}{4}$, and $4\ p^3\ q = 4 \cdot \frac{1}{64} \cdot \frac{3}{4} = \frac{3}{64}$. We may now state Bayes' rule of *à posteriori* probabilities, which will give us an immediate result from these numbers. "The probabilities of causes or hypotheses are proportional to the probabilities which these causes give for the events *actually observed.* The probability of one of these causes or hypotheses is a fraction which has for numerator the probability of the event in consequence of this cause, and for denominator the sum of the probabilities relative to all the causes or hypotheses." This theorem in itself of course merely indicates the correct and prudent opinion which we may form with reference to any special subject of thought; as in the example considered,

$$\frac{27}{7 + 16 + 3} = \frac{27}{46},\ \frac{16}{46},\ \frac{3}{46},$$ are the probabilities of the different hypotheses spoken of—that is, in the ignorance in which we stand as to the actual state of the urn. If the same experiment were repeated a great number of times, we should accumulate, according to the principles of the repetition of events, any amount of probability that these did actually represent the true state of the case,—*i.e.*, in 27 out of 46 urns there would be three white balls; in 16, two; in 3 one; if a very great number of urns were taken.—From these results, then, it is clear how to find the probability of drawing a white ball at the next trial also. The probability of the first hypothesis is $\frac{27}{46}$, and the subsequent probability of a white ball on that hypothesis is $\frac{3}{4}$. Hence $\frac{27}{46} \times \frac{3}{4} + \frac{2}{4} \times \frac{16}{46} + \frac{1}{4} \times \frac{3}{46} = \frac{116}{184}$, represents the chance required; all the different values being framed in the same way as $\frac{27}{46} \times \frac{3}{4}$. We may generalize this,—suppose an observed event q, which may be attributed to any one of the n causes, $c_1\ c_2 \ldots\ldots\ c_n$, equally probable before the event has happened. Let the probabilities of the observed event on the hypothesis be p_1 p_2, and so on. Then Bayes' rule states that if $\tilde{\omega}_1\ \tilde{\omega}_2 \ldots\ldots\ \tilde{\omega}_n$ represent the different probabilities, $p_1 : p_2 : p_3 \ldots p_n : : \tilde{\omega}_1 : \tilde{\omega}_2 : \tilde{\omega}_3 \ldots : \tilde{\omega}_n$ $\tilde{\omega}_n = \frac{p^n}{\Sigma\, p}$. Also let the probabilities of any future event q'; in respect of the different hypotheses be p_1. Then Π the total probability of the event is $p_1\ \tilde{\omega}_1 + p_2\ \tilde{\omega}_2 + \ldots\ldots\ p_n\ \tilde{\omega}_n = \Sigma\, p\ \tilde{\omega}$. If again the hypotheses be not equally probable before the event, let λ_1, λ_2, &c., represent their respective probabilities, then $\tilde{\omega}_1 = \frac{\lambda_1\, p_1}{\Sigma\, \lambda\, p}$. These formulæ can only be used when the number of hypotheses is finite. By means of a theorem of differences $\Sigma \chi = \int_0^1 \chi\ \delta x$, we can establish the following conclusion as to the constitution of an event, from actual observation, where the number of hypotheses may be considered infinite. If a ball, for example, have been drawn a great number of times in succession from an urn and replaced every time—the number of balls in the urn being unknown—and if the result has been m white balls and n black balls, the probability of drawing a white ball at the next trial is $\frac{m + 1}{m + n + 2}$. So the probability of drawing, in $m' + n'$ future trials, m' white and n' black balls, can be shown to be

$$\frac{\angle m' + n^1}{\angle m' \angle n'} \times$$

$$\times \frac{\angle m + m' \angle n + n' \angle m + n + 1}{\angle m' \angle n \angle m + m' + n + n' + 1}.$$

Some light may be thrown upon the first result by showing that, if the urn be supposed originally to contain one black and one white ball, and if a black ball and a white ball be supposed added to it for every black ball and white ball drawn, then the urn which would result from that process would give quite the same probability of drawing a white or black ball. As an illustration, we may take the probability of a sunrise to-morrow morning—that is, the purely subjective probability—say on 1st January, 1837. Taking the ordinary era, there had then been 2,131,965 successive sunrises—drawn as it were from the urn every morning, and no failures in that result. Hence $n = o$ and the probability of that special sunrise was $\frac{2131966}{2131967}$, which is near enough certainty. Another question, resting upon the same principles, is solved by Bayes—

amely, what is the probability that—given a ertain number of appearances of an event—there xists a cause for them. The measure of this, here n is the number of results, is $\frac{2^{n+1}-1}{2^{n+1}}$.

hus, if an event has been observed ten times ı succession the probability that that is not an fect of chance is $\frac{2^{11}-1}{2^{11}} = \frac{2047}{2048}$ or very near ertainty.

It will be at once evident how powerfully these ethods and principles can assist in the great ractical problem of obtaining from a series of servations of a physical event *the* most probable sult and the probable limits of error. It is ıpossible for us to enter here into any detail ı a subject treated besides under SQUARES, ETHOD OF.

VI. We shall now try to illustrate the prin- ples of probabilities in two very important ap- ications. The first is that to the question of *nefits depending on the probable duration of man life.* The fundamental element or datum the problem is the probability, which observa- ons must determine, that an individual of any ven age will live over a fixed time, say one ar. Thus if the probabilities for a person aged living over 1, 2, 3, ... n years, be $p_1\, p_2\, p_3$ ...p_n, and if q_1 ...... q_n, r_1 ...... r_n, &c., note the same probabilities for $y+1$, $y+2$, ., then $p_2 - p_1 = q_1 p_2 - p_1\, q_1 = r_1$, &c., so that the values p_n are given when the set $p_1\, q_1$ &c., are determined by observation. But s is actually accomplished by careful statis- al work, viz.,—the accurate determination, in en circumstances, of the values of $p_1\, q_1\, r_1$ &c. w for annuities. Let r be the interest of £1 for a ar. But v = present value of £1 to be received er a year is $= \frac{£1}{1+r}$. The present value the first yearly payment to an annuitant is but the probability of payment is p. Hence 1 is the value of his expectation. So v^x is the ue of £1 certainly to be paid after x years, and is the chance of its being paid. Hence, also $v^x p_x$ is the value of an annuity of £a from $= o$, to $x =$ such a number that $p_x = o$. e value of the annuity $\Sigma v^x\, p_x$ may be separ- d into two parts $\Sigma\, v^n\, p_n + \Sigma\, v^x\, p^x$ where s taken on from $n+1$ to the number for ich p_x vanishes. The first part is evidently value of a *temporary* annuity to end after n rs, the second of the *deferred* annuity, only to in then if the person be then alive. This er may also be found by the regular formula total annuities. Let A_n be the value of a annuity for a person aged $y + n$ years. en clearly $v^n\, p_n\, A_n$ is the value of this *deferred* uity, the terms $v^n\, p_n$ being introduced, because, *first*, the amount A_n is not receivable till after n years; *second*, because it is payable only on the contingent event of the annuitant being alive. Hence the *temporary* annuity $= A - v^n\, p_n\, A_n$. The same principles are carried into assurances of lives, temporary or permanent. We refer the reader to Baily's *Doctrine of Life Annuities and Assurances;* and Milne's *Treatise on Annuities and Insurances on Lives and Survivorships.*—Our other illustration refers to the decisions of juries or tribunals. The case of a witness making an assertion may be compared with the illustration of an urn which contains balls of two colours, and from which certain *drawings* show m of the one and n of the other. Then $\frac{m+1}{m+n+2}$ is the probability of the former result recurring. Let the colours of the balls represent true and false. Then $\frac{m+1}{m+n+2} = v =$ the veracity of the witness; $\frac{n+1}{m+n+2} = w = 1 - v =$ chance of his telling a really untrue story. Now, suppose a witness to testify that an event whose *à priori* probability is p, has taken place. Suppose v and w, which in this case must be roughly estimated, under the condition $v + w = 1$, to represent the chance of correctness or incorrectness in his evidence, then by the principles already established, if $\tilde{\omega}_1$ be the probability of truth and $\tilde{\omega}_2$ that of falsehood in the testimony $\tilde{\omega}_1 = \frac{v\,p}{v\,p + w\,q}$. Hence $\tilde{\omega}_1 - p = \frac{p\,(2\,v-1)\,q}{v\,p + w\,q}$. From this it appears that the event is made more probable or less probable by the testimony, according as $2\,v - 1$ is $>$ or <0, that is as $v > \frac{1}{2}$, or $< \frac{1}{2}$. If $v = \frac{1}{2}$, $p = \tilde{\omega}_1$, that is, the probability is unaltered. When a witness's character is entirely unknown, we may suppose v to have all possible values between 0 and 1. Then $\tilde{\omega}_1$ will be found by integrating $\int_0^1 \tilde{\omega}_1\, \delta\, v$. Substituting for $\tilde{\omega}_1$ we have $\int_0^1 \tilde{\omega}_1\, \delta\, v = \int_0^1 \frac{p\,v\,d\,v}{v\,p + w\,q}$, and substituting for $w\,q$ we have

$$\int_0^1 \tilde{\omega}_1\, d\,v = \int_0^1 \frac{p\,v\,d\,v}{1 - p + (2\,p-1)\,v}$$

$$= \frac{p}{2\,p-1}\left(1 - \frac{1-p}{2\,p-1}\,\log\,\frac{p}{1-p}\right).$$

It is easy to see how by combining those elementary principles, it is possible to determine the probability given by the concurrent testimony of

several witnesses,—the concurrent testimony of m and the opposing testimony of n, and so on. The case of jurors and tribunals is exactly analogous to this. They are considered as witnesses to certain of the circumstances connected with the fact, and so forming an opinion. It is unnecessary and impossible here to go into details. All of them assume a previous establishment of the mean values of these two data, the probability of a juror's giving a verdict correct as to the facts—and that of the guilt or innocence of the prisoner *à priori.* The probable values of these two essential elements were found by Poisson, from statistical results, to be as follows:—the probability of a juror giving a correct verdict, was a little more than $\frac{2}{3}$ with respect to crimes against the person, and to $\frac{13}{17}$ for crimes against property; and—making no distinction of crimes—to be very little below $\frac{3}{4}$. The probability as to the guilt of accused before the trial, was found to be about ·53 or ·54 with respect to crimes against the person, and more than $\frac{2}{3}$ for those against property. For crimes without distinction it was very nearly ·64. Using those data, the results of the formulæ of which we have spoken, are, that in a hundred trials it will happen only seven times that the accused will be pronounced guilty by seven against five. The probability of a unanimous verdict of not guilty is ·0114. The probability of the correctness of the verdict of seven against five, is $\frac{16}{17}$, and that of a verdict pronounced by seven against five *at least*, is $\frac{118}{119}$, so that in the former case we may expect one of seventeen to be innocent, in the latter one of 119. —We refer the reader for an exposition of the physical bearings of the theory of probabilities to SQUARES LEAST, METHOD OF. The non-scientific reader will find an admirable summing up of the subject in Quetelet's *Lettres sur la Théorie des Probabilities;* and of treatises which do not employ the resources of the highest mathematical analysis, Cournot's *Exposition de la Théorie des Chances*, and De Morgan's Treatise in *Lardner's Cyclopædia* are the best known and perhaps the most valuable.

Procyon. A fixed star of the second magnitude in the constellation Canis Minor.

Projectiles. The problem before us—to determine the circumstances of a projectile's path—must, it is evident, be complicated considerably by the resistance of the air to such motion. The forces which operate upon the body are, the primary force of the original projection—the continuous and uniform force of gravity, and the force which the air exerts in opposition to motion. We shall not enter into details as to this latter force, and it is clear that its removal greatly simplifies the problem. It is usual first to consider projectiles as acting "*in vacuo.*" Indeed, no experimental determinations of the resistance of the air have been yet made so accurately, as to warrant our introducing them as permanent data in the problem of projectiles.—Suppose, then, we have a body projected from A, in the direction A T, and let t be the time in which the body would have reached T, with the initial velocity v, if gravity had not been acting at all. Then if T P be the space through which it would have fallen by virtue of gravity, during the time t, P will be its position at the end of that time (COMPOSITION OF MOTION).

$$\text{But } \mathrm{AT} = v\,t,\ \mathrm{PT} = g\,\frac{t^2}{2}.$$

$$\text{Hence } \mathrm{AT}^2 = v^2\,t^2;\ \frac{2\,\mathrm{PT}}{g} = t^2$$

$\therefore \mathrm{AT}^2 = \dfrac{2\,v^2}{g}\,\mathrm{PT}.$ Now let h be the height from which a body must fall in order to acquire the velocity v, then $h = \dfrac{v^2}{2\,g}$

$$\therefore \mathrm{AT}^2 = 4\,h\ \mathrm{PT}.$$

Drawing A V parallel to P T, and taking A V A T, as oblique axes of x and y respectively, this equation assumes the form $y^2 = 4\,ax$, the equation of a parabola whose axis is parallel to A V, and therefore vertical. This investigation is replaced to the more advanced student by the following:—Taking the axis of co-ordinates as vertical or upwards, and horizontal—the origin being at the point A, the equations of motion

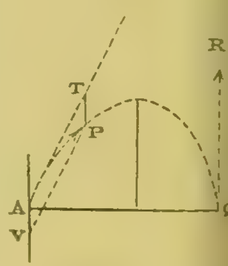


Fig. 1.

become $\dfrac{d^2\,x}{d\,t^2} = o$, $\dfrac{d^2\,y}{d\,t^2} = -\,g.$ By integration $\dfrac{d\,x}{d\,t} = c$ and $\dfrac{d\,y}{d\,t} = -\,g\,t + c'$,

where the constants c, c' will depend on the special circumstances of the projection. If the velocity of projection be v, and the angle which the line of projection makes with the horizontal axis be α,

we obtain $v \cos \alpha = \dfrac{d\,x}{d\,t}$ at the time $t = o$, and

as $\dfrac{d\,x}{d\,t} = c$, we have $\dfrac{d\,x}{d\,t} = v \cos \alpha.$ Also

$$\frac{d\,y}{d\,t} = v \sin \alpha = c' \text{ when } t = o,$$

therefore $\dfrac{d\,y}{d\,t} = -\,g\,t + v \sin \alpha.$ Hence the differential equations from which we obtain final results, are

$\frac{d\,x}{d\,t} = v \cos \alpha. \quad \frac{d\,y}{d\,t} = -g\,t + v \sin \alpha.$

ıtegrating these, we have $x = v\,t \cos \alpha + \text{c}$

$$y = -\frac{g\,t^2}{2} + v\,t \sin \alpha + \text{c}'.$$

ɔw as A is the origin, we have for $t = o$, $x =$ ınd $y = o$. Hence, substituting, we find c d c′ each $= o$. ∴ Finally,

$$= v\,t \cos \alpha. \quad y = -\frac{g\,t^2}{2} + v\,t \sin \alpha.$$

ıese formulæ contain an independent variable t. iminating this by means of the first of these have $y = x \tan \alpha - \frac{g\,x^2}{2\,v^2 \cos^2 \alpha}$, which ı be at once put in form of the equation to a ·abola whose axis is vertical and stretches vnwards. We shall take several simple illus- tions of these principles, in the usual problems projectiles. It is required, for instance, to find *range* and the *time of flight* of a projectile ose initial velocity is v. The range is the gth of a line between the point of projection A and the point Q, where the horizontal axis ıgain cut by the trajectory of the projectile; *time of flight* is that occupied in passing from ɔ Q. Since A R $= v\,t$, and R Q $= \frac{g\,t^2}{2}$, we

e R Q = A R $\sin \alpha = v\,t \sin \alpha = \frac{g\,t^2}{2}$.

refore $2\,v \sin \alpha = g\,t$, and $t = \frac{2\,v \sin \alpha}{g}$.

o A Q = A R $\cos \alpha = t\,v \cos \alpha = \frac{2\,v \sin \alpha}{g} \times$

$\cos \alpha = \frac{2\,v^2 \sin \alpha \cos \alpha}{g}$.

l since $2 \sin \alpha \cos \alpha = \sin 2\,\alpha$,

$$\text{A Q} = r \text{ (or range)} = \frac{v^2 \sin 2\,\alpha}{g}.$$

n these formulæ it is only necessary to sub- ıte for v, α (which depend upon the special olem), and g (32·2 feet), their special values, rder to obtain the range and the time of flight. *greatest* range for a given initial velocity will btained when—v being considered constant— $\frac{\sin 2\,\alpha}{g}$ becomes the greatest possible, as e case when $\sin 2\,\alpha = 1$, and when, there- $2\,\alpha = 90°$ or $\alpha = 45°$. Hence a shot in o would have greatest range if fired at an e of 45° to the horizon. In that case the e $= \frac{v^2}{g}$. Now, if h be the height due to a city v, we have $2\,g\,h = v^2$, $2\,h = \frac{v^2}{g}$, and

therefore the greatest range $2\,h$.—These problems are solved more readily by means of the second equation given above, viz.,

$$y = x \tan \alpha - \frac{g\,x^2}{2\,v^2 \cos^2 \alpha}.$$

To find the range we have $y = o$: therefore $x \tan \alpha - \frac{g\,x^2}{2\,v^2 \cos^2 \alpha} = o$, which gives two values of x, $x = o$ (answering to the point A), and, from $\tan \alpha - \frac{g\,x}{2\,v^2 \cos^2 \alpha} = o$,

$x = \frac{2\,v^2 \cos^2 \alpha \tan \alpha}{g}$, or $x = \frac{v^2}{g} \sin 2\,\alpha$ as before.—If, again, the body meet an inclined plane (inclined to the horizon at an angle i), the value of x for the point where it meets it, is called the range on the inclined plane. The student may exercise himself finding this without the use of the formulæ, just as we found the range. Employing them, $x \tan i = x \tan \alpha - \frac{g\,x^2}{v^2 \cos^2 \alpha}$, which gives $x = o$ (answering to A) and x

$$= \frac{2\,v^2}{g} \cos^2 \alpha\,(\tan \alpha - \tan i) = \frac{2\,v^2}{g} \cos^2 \alpha \times$$

$$\times \left(\frac{\sin \alpha \cos i - \cos \alpha \sin i}{\cos \alpha \cos i} \right)$$

$= \frac{2\,v^2}{g} \cdot \frac{\cos \alpha}{\cos i} \cdot \sin \alpha - i)$ where $\alpha - i$ is the inclination of the original projection to this plane. Again, to find t, we have $x = v\,t \cos \alpha$, by the formula

$$\therefore t = \frac{x}{v \cos \alpha} = \frac{2\,v^2}{g} \cdot \frac{\cos \alpha}{\cos i}.$$

$$\sin (\alpha = i) \cdot \frac{i}{v \cos \alpha} = \frac{2\,v}{g} \cdot \frac{\sin (\alpha - i)}{\cos i}\ v$$

which, if $i = o$, becomes as it ought $= \frac{2\,v}{g} \cdot \sin \alpha$.

When the body reaches its greatest height—we have $\frac{d\,y}{d\,x} = o = \tan \alpha - \frac{2\,g\,x}{2\,\alpha \cos^2 \alpha}$

$$\therefore x = \frac{2\,v^2}{2\,g} \cos^2 \alpha \tan \alpha =$$

$$\frac{v^2}{2\,g} \cdot 2 \sin \alpha \cos \alpha = \frac{v^2}{2\,g} \cdot \sin 2\,\alpha$$

Also, substituting in the formula for x, we get

$$y = \frac{v^2}{g} (\sin^2 \alpha - \tfrac{1}{2} \sin^2 \alpha) = \frac{v^2}{2\,g} \cdot \sin^2 \alpha.$$

From this we see that the greatest height is reached just above the middle of the line A Q, and is equal to $\frac{v^2}{2\,g} \sin^2 \alpha$.

If we desire to secure that the curve shall pass through the point whose co-ordinates are y', x', we must have such value of α as will satisfy the equation

$$y' = x' \tan \alpha - \frac{g\,x^2}{2\,v^2 \cos^2 \alpha},$$

for a given velocity v (practically for a given charge of powder), an equation which gives the following result,

$$\tan \alpha = \frac{v \pm \frac{1}{2}\sqrt{v^4 - 2\,v^2\,g\,y' - g^2\,x'^2}}{g\,x'},$$

So that if $v^4 > 2\,v^2\,g\,y' + g^2\,x'^2$, we have two values for $\tan \alpha$, if $v^4 = 2\,v^2\,g\,y' + g^2\,x'^2$, we have only one, and if $v^4 < 2\,v^2\,g\,y' + g^2\,x'^2$, no value is possible. It is at once clear that only the value of α need be taken, which is below 90°. —The equation $v^4 = 2\,v^2\,g\,y' + g^2\,x'^2$ expresses the parabola which is called the envelope of all those which correspond to different values of α, that is, which touches them all. Hence if the point x', y', lie without this curve, it will be impossible, as is otherwise clear, that any shot fired with velocity v should pass through it.—Again, if we are to find v for a given value of α, so that the curve may pass through x', y', *e.g.*, having given the elevation of cannon or mortars, required practically the charge, which will enable them to strike a given object,

$$y' = x' \tan \alpha - \frac{g\,x'^2}{2\,v^2 \cos^2 \alpha}$$

$$\frac{g\,x'^2}{2\,v^2 \cos^2 \alpha} = y' - x' \tan \alpha$$

$$2\,v^2 \cos^2 \alpha = \frac{g\,x'^2}{y' - x' \tan \alpha}$$

$$v^2 = \frac{g\,x'^2}{2 \cos^2 \alpha\,(y' - x' \tan \alpha)}.$$

An equation which is impossible in the single case $y' < x' \tan \alpha$, and which, if $y' = x' \tan \alpha$, requires v, and therefore the charge, to be infinite. The meaning of this condition is evidently that y', x', must not be higher than the line which passes the origin, at an angle α to the axis of x; not higher, that is, than the course of a projectile shot off at an elevation α, if gravity did not exist. —The equations of motion for the case of bodies in air are much more complicated. We require to make hypotheses of laws of fluid motion, and to assume experimental data, not yet clearly determined. We refer the reader to M. Duhamel's *Mecanique* for the full investigations. We shall only indicate how very great is the importance of the most accurate determination of the points yet in dispute, by one illustration. For a velocity of 1,000 feet per second, the resistance of air to a leaden ball of an inch in diameter and spherical in shape would be equal to 52 oz. But its weight is only about $3\frac{1}{2}$ oz. Hence the resistance has come to have about fifteen times the effect which gravity would have upon the motion of the ball. In such a case the investigations which precede would be of almost no practical use.—Upon the hypothesis that the resistance of the air varies as g and as v^2, we may express it as $\frac{g\,v^2}{k^2}$, k being a constant experimentally determined. Calling s the arc of the trajectory from the point of starting, we obtain the following equation of the trajectory, if $p = \tan \alpha$.

$$p\sqrt{1+p^2} + \log\left(p + \sqrt{1+p^2}\right) = - \frac{k^2}{a^2 \cos^2 \theta}\, e^{\frac{2gs}{k^2}} + y,$$

where a and θ are the initial values of v and α; y being determined by making $s = o$, $p = \tan \theta$, which gives

$$y = \tan \theta \sec \theta + \log\left(\tan \theta + \sqrt{1 + \tan^2 \theta}\right) + \frac{k^2}{a^2 \cos^2 \theta}$$

$$= \tan \theta \sec \theta + \log(\tan \theta + \sec \theta) + \frac{k^2}{a^2} \sec^2 \theta.$$

This equation of the trajectory contains s, which is by no means a convenient element. The differential formulæ in which the curve may be expressed are given fully in Duhamel, and numerous interesting practical cases in which the very complex original equation becomes so simplified as to admit of easy solution are fully considered.—It has not been thought necessary to remark that, *in vacuo* at least, and where the resistance of the air is in a line perpendicular to the path, all the motion would take place in one plane. This would not be the case, were the projectile passing through a medium itself agitated in arbitrary directions. The effect, however, of high winds or the like, upon the motion of projectiles probably can never be accurately estimated. As it is clear, finally, that the resistance will vary with the density of the medium, experimental determinations of k must be made for the actual circumstances of every special case.—We append to this general sketch of the theory of projectiles a very beautiful geometrical method, by Mr. Galbraith of Dublin, for determining the complete circumstances of projectile motion in vacuo.—Let B T be the direction of the projectile, and B the initial position. Draw the vertical B H = 4 h, h being the height due to v. Suppose the projectile to strike upon the plane P′ P M R at R, and draw R T meeting B T, and T M; P′ B H′ perpendicular to the plane; also H H′ perpendicular to B H, through H. Bisect B H′ in O. Join O T, and draw T N parallel to the plane. Then

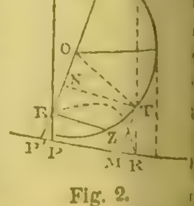


Fig. 2.

$$\mathrm{BT} = vt,\ \mathrm{BT}^2 = v^2 t^2.$$

3ut $$\frac{\mathrm{BH}}{4} = \frac{v^2}{2g},\ \mathrm{BH} = \frac{2v^2}{g}$$

$$\therefore \mathrm{BT}^2 = \mathrm{BH} \cdot \frac{gt}{2}.$$

3ut $$\mathrm{TR} = \frac{gt^2}{2},\ \therefore \mathrm{BT}^2 = \mathrm{BH} \cdot \mathrm{TR}.$$

n the similarity of the triangles B H H′, and R, we have $\mathrm{BH} \cdot \mathrm{TR} = \mathrm{BH}' \cdot \mathrm{TM}. = 2\,\mathrm{OB}'$ P′. Again, $\mathrm{BT}^2 = \mathrm{BO}^2 + \mathrm{OT}^2 - 2\,\mathrm{BO} \cdot \mathrm{ON}$

$$\therefore \mathrm{OT}^2 = \mathrm{BT}^2 - \mathrm{BO}^2 + 2\,\mathrm{BO} \cdot \mathrm{ON}$$
$$= \mathrm{BT}^2 - 2\,\mathrm{OB}^2 + 2\,\mathrm{BO} \cdot \mathrm{ON} + \mathrm{BO}^2$$
$$\mathrm{OB} \cdot \mathrm{NP}' - 2\,\mathrm{OB}^2 + 2\,\mathrm{BO} \cdot \mathrm{ON} + \mathrm{BO}^2$$
$$= 2\,\mathrm{OB}\,(\mathrm{NP}' - \mathrm{OB} + \mathrm{ON}) + \mathrm{BO}^2$$
$$= \mathrm{BH}' \cdot \mathrm{BP}' + \mathrm{BO}^2.$$

r all the quantities B H′, B P′, B O, are deter-ed quite independently of the direction B T, g which the projectile moves, and therefore ave, whatever that angle be, O T, an invari- quantity. Hence we can readily construct circle, by drawing B Z perpendicular to H′ P′ a mean proportional between B H′ and B P′, by describing it from O as centre with O Z adius.—Suppose we wish to find the farthest e along the plane P′ R. We have only to r the vertical line which is tangent to the e; and this may be done by drawing the hori-al radius, and the tangent at its extremity. point where the vertical cuts the plane will ently be that required; and the line of pro-on will be that from B to the extremity of adius. The usual proposition that this line ts the angle between the plane and the ver- can be readily proved. As to all points on lane beyond this point of maximum range, rojectile shot off with the velocity v can through them anyhow; and for all points in it, it is clear that—if R represent one nem—two directions, B T and B T′ can be d for the projectile, by drawing the vertical R T T′. The most important theorems of ctiles in vacuo can thus be shown to be true is geometrical method.—We subjoin certain rimental results relative to the comparative ts of charges of powder in communicating ity to projectiles. We have hitherto, it will een, simply considered a certain velocity as lly given. The present inquiry shows us to give it, and that most economically, neans of powder. The results were ob-d by the use of Robin's ballistic pendu- The experiments and results are fully led in the *Philosophical Magazine*, June, , in a paper by Mr. Haughton. We can only cribe an abridged account of these results. e experiments were used—1. A two-grooved length 31½ inches, diameter ·66 inch with urn in four feet.—2. The regulation minie length 39 inches, diameter ·69 inch.—3. e carbine; length 28¼ inches, diameter ·66 inch. With these guns were used the following:—With the first—1. A minie bullet with two projections corresponding to the grooves, without "culot," weight 697 grs.—2. A sugar loaf bullet, fired point foremost, weight 669¾ grs.—3. A belted spherical bullet, weight 482 grs. With the minie—the regulation minie bullet with "culot," weight 744 grs. With the carbine—a spherical bullet, weight 391 grains. We have the following final results of experiment:—1. That the quantity of motion communicated by a given quantity of powder to the minie bullet, discharged from the regulation rifle, is greater than that possessed by any of the other bullets; the result due partly to the greater weight of the bullet, partly to the greater length of the rifle.—2. That the quantity of motion communicated to the belted bullet discharged from the two-groove or Brunswick riflé, is less than that possessed by the other rifle bullets; the result being due to the greater weight of the belted bullet.—3. That from the greater friction in the rifle barrel, the quantities of motion for the carbine and belted rifle bullet are equal.—4. That in passing through eighty feet of still air, the quantity of motion in the minie bullet was diminished by $\frac{1}{7\cdot6}$; in the sugar loaf bullet by $\frac{1}{7\cdot5}$; and in the belted bullet by $\frac{1}{8\cdot6}$; this remarkable inferiority in the latter being due chiefly to its shape.—5. That the adaptation of bullets of the proper weight, shaped like the minie, with two side projections to fit the grooves, would make the Brunswick two-groove rifles in possession of the British rifle service, as effective as the regulation minie rifles. The weight, calculated theoretically, of these bullets would have to be 967 grains, or 7¼ to the pound. It results in general that the velocity with which a bullet is projected from a rifle by means of a given charge of powder, mainly depends on the weight of the bullet and length of the barrel—varying inversely as the square root of the former, and directly as the square root of the latter.

Projection. The projection of a figure upon any surface is obtained by drawing from a given point of sight, or central point, lines to all points of the figure, and producing them till they meet the surface. The total figure made of all these points of meeting, is the projection, for the data given.—Several principles of projection become at once evident. 1. The projection of a straight line on a plane is a straight line,—for evidently it is the line of intersection of the given plane with the plane which passes through the point of sight and the given line; hence, points which lie on a straight line in the original figure will be projected upon a straight line: lines which meet in a point in the original figure will be projected into lines meeting in a point: tangents to curves—as the limits of chords—in the original figure, will be projected into corresponding tangents, and so on.

I. We shall begin this article with a brief

account of the practice of projection in common use. —In map drawing, for instance, we have to solve the problem how to represent upon a plane surface the solid body of the earth, so as accurately to suggest its forms. The methods of approximating to such a solution, we propose to exhibit.

(1.) *Orthographic Projection.*—The point of sight spoken of, is here considered as at an infinite distance and in the axis of the plane. Evidently, therefore, all points on the spherical surface will be projected by drawing perpendiculars on the plane. Now, for surfaces near the centre or top of the hemisphere, this will be nearly an accurate representation. In fact, at these points, the sphere is very nearly a plane parallel to that of projection, and the picture of such a plane would, it is evident, be given with complete truth. But for points near the edge, the surface of the sphere on the contrary, approaches to a plane perpendicular to that of projection. Hence this projection on the plane of the equator answers very well for points near the pole; and on the plane of any great circle for points near its pole; but it becomes utterly useless as we move down towards the plane of projection. In making the projection, only one-half of the sphere is projected upon the one plane. Suppose that to consist of an upper and lower surface, separable, but connected at one extremity of a diameter. Suppose now the lower side to be moved around this hinge 180°, the lower side will come to be seen by an eye looking at the upper. We see this projection in the pair of maps of the two hemispheres usually found in atlases. It is fortunate—for the truth of the projection—that near the edges of the plane of projection, sea and not land is most copiously found, so that the picture does not so manifestly suffer. The orthographic projection is made usually upon the plane of the equator or of a meridian. In the former case it is clear that the meridians—great circles all through the pole—will be projected into straight lines, all through the point which is the projection of the pole. In the latter, the circles of latitude will be projected also into straight lines, perpendicular to the axis of the sphere.

(2.) *Stereographic Projection.*—In this the

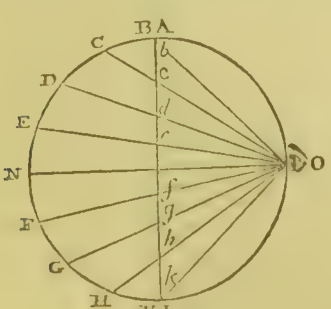


Fig. 1.

point of sight is considered to be at the pole of the plane of projection, remote from the hemisphere to be projected. Every circle is here projected into a circle. As planes of reference, we have either the plane of the meridian, the plane of the equator, or an oblique plane,—the latter however used less frequently. It is clear from the figure that there will be less crowding and distortion of parts in general than in the orthographic projection—though still too considerable an effect. Here, too, a little consideration will show that the parts near the centre will be more crowded than those more removed, but the projections of circles intersect under the same angle as the circles themselves, so that the relative positions are in so far represented.

(3.) *Globular Projection.*—The point of sight is here so chosen that if A D C be bi-sected, and each part projected into O P, P C, O P and P C will be equal. This is, in fact, something equivalent to making two centres, P, and the corresponding point P′, of closely approximate correctness. It is easy to see by simple trigonometry that S H : A O : : sin 45° : 1.—A modification of this projection, called the equi-distant projection, is sometimes used. Draw a circle and within it two diameters at right angles. Assume the vertical one as the axis of the sphere, and divide the horizontal into equal parts. Through the poles, and each of the points of division describe a semicircle. These semicircles all represent projections of the meridians. Divide each quadrant into equal parts, and number them from the equator to each pole. Divide the vertical semi-diameters into the same number of equal parts, and number these from the equator. Through corresponding points of division on the same hemisphere draw arcs of circles: these will represent the projections of circles of latitude.—The circles of the sphere may be projected on the plane of the equator, as follows:—Draw a circle and divide it into equal sections by diameters; these will represent the projections of meridians. Divide any radius into equal parts, and through the points of division draw circles concentric with the assumed circle; they will represent the projection of equi-distant circles of latitude. This equi-distant projection differs from the globular chiefly in this, that, in the former, all circles of the sphere are projected into ellipses with small eccentricities, whereas in the latter they are taken to be perfect arcs of circles. In both equal distances and spaces on the surface of the sphere are represented by equal or nearly equal spaces of projection; and, consequently, the relative dimensions are better preserved than in either of the preceding. But the projections of circles do not intersect under the same angle as

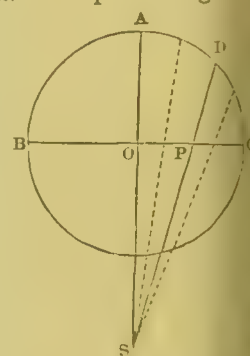


Fig. 2.

e circles themselves, so that the *forms* of parts the surface are considerably distorted—the ore so, the more distant they are from the ntre of projection. On the whole, therefore, the reographic projection appears to unite most ad- ntage and smallest disadvantage, so that it is nmonly adopted.—These are the only projec- ns in much use for the whole sphere. The lowing are employed for parts of it.

(4.) The *Gnomonic Projection.*—In it the eye aken to be at the centre of the sphere, and the rence plane is tangent to the surface at same nt, called for the time the *principal point.* e meridian through the principal point is led the *principal meridian*—the circle of lati- e through it the *principal parallel,* and its ar distance, the *principal polar distance.*— ıen the principal point is at the pole of the ere the meridians will evidently be projected straight lines, passing through it and mak- angles equal to those between the meridians nselves. The circles of latitude will evidently projected into circles, with the principal point centre, and with radii equal to the tangents of e polar distances.—Next when the principal it is on the equator. In this case the meridians projected into parallel straight lines, sym- rically arranged round the line of projection he principal meridian. The circles of lati- are projected into arcs of hyperbolas, whose res are at the principal point, and whose sverse axis is the projection of the principal idian. —*Third,* when the principal point is on irc of any circle of latitude—that is, anywhere on the sphere. The meridians are, in this , projected into straight lines passing through projection of the pole, which is on the line of ection of the principal meridian, and distant line equal to the product of the radius by angent of the principal polar distance. The es of latitude whose polar distances are less the inclination of the polar radius to the is of the principal point, are projected into ses. That which has its polar distance just l to this—into a parabola, and all others into rbolas. The principal axis of all these es is the projection of the principal meridian. projection is not much used, and only for a l space near the principal point; since the ents of very small arcs are very nearly equal e arcs themselves. It seldom happens that principal point but the pole is practically . For this position, however, it apparently lies a defect in Mercator's projection.

.) The *Polar Projection.*—In this case the eye o at the centre of the sphere, and the prin- plane is the plane of one of the polar circles. meridians are projected into straight lines, ecting each other at the point in which the itive plane cuts the axis of the sphere, ng angles with each other equal to the s made by the meridians. The circles of ıde are projected into concentric circles, and so on. The projection only serves for a zone within a few degrees on each side of the polar circle, and may be used like the preceding in supplement to Mercator's projection.

(6.) The *Conic Projection.*—In both varieties of this, the eye is supposed to be at the centre of the sphere and the characteristic of the pro- jection is that the sphere is projected upon a cone, which is afterwards rolled or developed upon a plane tangent to it, along one of its ele- ments. The first variety of this projection is when the cone is a tangent cone. Suppose it to be tangent to the surface of the sphere along the middle circle of latitude of the zone which is to be projected. Evidently the meridians will be projected into right lines intersecting at the vertex of the cone, and developed on the tangent plane into straight lines also. The circles of latitude will be projected into circles of the conic surface, and are developed into arcs of circles whose common centre is the development of the conical vertex. The second description of cone is a secant cone. It is passed through circles of latitude equi-distant from each other and from the extreme circles. In fact the tangent cone is just the limit of this secant cone, when the two circles of latitude through which it passes come to coincide. The method of construction and the whole properties of this projection are ex- tremely similar to those of the other case.

(7.) There is further the *Cylindrical Projection.* —Here the projection is made on a cylinder taken as tangent to the sphere, intersecting it in the equa- tor plane, and the projection upon this surface is then unrolled as in the case of the tangent and secant cones. A modification of this projection, called Mercator's, is very extensively used in projecting sailing charts. Both meridians and circles of latitude are projected into straight lines. The meridians are represented by parallel straight lines, at equal distances from each other. The parallels of latitude are represented by straight lines, whose distances apart increase nearly as the length of a degree of longitude decreases. The loxodromic curve (RHUMB) is represented by a straight line, and this is per- haps the most important advantage possessed by this kind of projection, because it corresponds with the most important apparent fact in the mind of the seaman. We may understand how the chart is constructed by considering, *à priori,* how this all-important condition is to be ful- filled. Assuming the projections of the me- ridians as equi-distant straight lines, it is clear that, as we recede from the equator, the scale upon which a degree of longitude is represented must continually increase. Hence, that the chart may fulfil the required condition, the scale of latitude is made to increase in the same pro- portion. Now the length of a degree of longitude in any latitude is equal to the length of a degree of longitude at the equator, multiplied by the cosine of the latitude;—hence, since the degree of

longitude is represented on the chart by one uniform distance, it is evident that the scale increases inversely as the cosine of the latitude—that is, directly as the secant of the latitude. The scale of latitudes must therefore increase in this proportion. It is sufficiently evident that it would be absurd to represent the regions near the poles upon such a scale, because on account of the largeness of the secant for such high latitudes, the scale of representation would be almost without limit.

(8.) The *Homolographic Projection.*—An entirely new idea as to the construction of Geographical Maps,—approximated to by Flamsteed. We owe, however, to M. Babinet the starting and resolution of the problem—how shall a map of the earth be laid down on a plane surface, so that the surfaces as represented shall have the actual ratios of the real surfaces? So that Africa, for instance, measured upon the map by a Planimeter, and Europe measured by a Planimeter, shall have the ratio of the actual surfaces of these continents? And, further, so that the relative directions shall be preserved? No one can doubt the importance of maps constructed on such a principle; although it is an error to term it a *projection*, inasmuch as it has no relation to any special point of sight. The scientific part of the problem was resolved by aid of M. Cauchy, who reduced it to the solution of a transcendental Equator. M. Babinet is at present engaged in publishing an Atlas constructed on this principle; and assuredly his enterprise merits full success. A series of maps so constructed must be of greatest value to the statist, as well as to the inquirer into the physical attributes of the earth. Our next Great Physical Atlas must be constructed on the Homolographic method.

II. But the foregoing are only practical uses, and these of a limited kind, of the now fertile and important method of *Projection.* In its true significance, it is a powerful Geometric Method, brought into use in recent times, by whose aid propositions demonstrated to be true in a single instance, or of a single *form*, may at once be inferred to be true with regard to *all forms* into which that primal form may be projected. The Theory of Projection, in this largest sense, consists in scrutinizing and laying down the special circumstances under which so large an inference is justified: perhaps it is not too much to say that its fertility is already such, as to enable the purer and more satisfactory methods of ancient Geometry to cope in the attribute of generality, with the analytical scheme founded by Des Cartes. A few illustrations of its elements and progress is all that can be offered here.—It is clear that to any point in the one figure will always correspond one in the other. Suppose the case limited to projections upon a plane, then a right line will always be projected upon a plane into a right line. Again, any curve will be projected into another curve of the same degree. For the degree of a curve, as is well known, depends upon the number of points in which any general straight line will intersect it. Now, if this general straight line be projected into another line cutting the curve of projection, we shall have here also a straight line cutting that curve in the same number of points; and therefore the projected curve will be of the same degree. Hence the only curves into which it is possible to project a circle upon a plane are—a parabola, an ellipse, or a hyperbola. We have already shown that a tangent to the original curve will be projected into a tangent to the projected curve. It is easy to see, besides, that if two curves touch in any points, their projections will touch in corresponding points. Also, if a plane through the vertex, parallel to the plane of projection, meet the original plane in a line A B, then any pencil of lines diverging from A B will be projected on the plane of projection into a system of parallel lines. For manifestly they are projected into right lines, and the point A, the projection of which is common to all, is projected to an infinite distance. Conversely any system of parallel lines in the original plane is projected into a system of lines meeting on a point in D F, where a plane through the vertex, parallel to the original plane is cut by the plane of projection. We see now, from these propositions, that if any property of a given curve involves not the *magnitude* but only the *positions* of lines or angles, this property will be true for any curve into which the first can be projected. We may thus generalize immediately our conclusions respecting the circle in many cases. For instance;—"if through any point in the plane of a circle a chord be drawn, the tangents at its extremities meet on a fixed line." But the circle may be projected into any conic; therefore, if through a point in the plane of any conic a chord be drawn, the tangents at its extremities meet in a point. Again, Pascal's and Brianchon's theorems (see POLARS) may be first most conveniently proved, the first by geometry, the second by means of polars with respect to the circle. But these are evidently theorems of position, and hence they must hold true also for all conics. Such properties as these are called *projective properties.* There are many projective properties which involve the *magnitudes* of lines also. For example, the anharmonic ratio of any four points, A, B, C, D, in a right line—being measured by the ratios of lines of angles from O, the vertex of the pencil to A B C D—will be unchanged for $a\ b\ c\ d$, where the four lines are cut by any transversal. Further, if there be an equation between any sets of points on a right line, such as

$$\mathrm{K.AB.CD.EF} + \mathrm{K.AC.BE.DF} + \mathrm{M.AD.CE.BF}, \&c., = 0,$$

where in each equation we have the same points mentioned, though in different orders, we should have, by drawing a perpendicular on the transversal from O, and substituting for A B, its value

$$\frac{OA \,.\, OB \,.\, \sin AOB}{OP} \quad \&c.,$$

ın equation in which we shall have in each term ıf the numerator $OA.OB.OC.OD.OE.OF$, ınd in each of the denominators OP^3. Dividing, hen, the whole equation by these, we have a re- ;ulting equation which depends only upon the ıngles at the vertex; so that the property will ıold for all transversals as well as A B, C D, E F. Nor need the points A, B, C, &c., be all on the same ine, if care be taken that the same product of ›erpendiculars $OP' . OP'' . OP'''$, &c., be found in ·very denominator. Hence, if we can prove a ›roperty for the *simplest transversal case*, we have t proved for all cases. Thus, to prove that if lines .rawn through a point, and through the vertices f A B C, meet the sides of A B C in $a\ b\ c$, then $.b . BC . Ca = AC . Ba . Cb$, we may suppose C ›rojected to an infinite distance, so as to have · C, B C, c C parallel. The relation then becomes $b\,BC = A'c . Ba$, which we see at once to be :ue. Hence the general proposition is true. 'hus, again, to investigate the harmonic pro- erties of a complete quadrilateral, we may join ll the points of the figure to a centre O, and ut the figure by a plane through O, and through ıe third diagonal. This will give us a parallel- gram as the projection, and the diagonals are ·isected and intersected at infinity, — that is, ıch of them is cut harmonically.—The fore- oing, however brief and inadequate, will give ›me idea of the power of the method of Pro- ·ctions.—We have spoken of the projection ' a circle upon a plane being a conic. Let us :e more accurately how this is. We shall only ıke the usual case of a right cone, in which the xis O C is perpendicular to the circular base. hat of an oblique cone, in which O C is not a :rpendicular, is quite similarly proved.—Let a

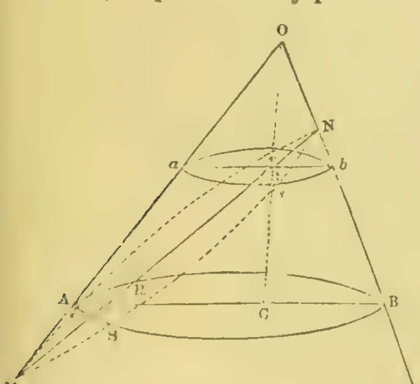


Fig. 3.

ane O A B pass through O C, and let $MSsN =$ S B be two sections both perpendicular to it. First, suppose M N to meet the two sides of the ·ne on the same side of O, then let any other ane $a\,b\,s$ be drawn parallel to the base. Draw .e figure as above.—Then, R S, the intersection of two planes perpendicular to O A B, is also perpendicular to it. Hence $RS^2 = AR . RB$. Similarly $rs^2 = ar . rb$. Now the triangles A R M, $a\,r$ M, N $r\,b$, N R B are similar. Hence $MR . RN : Mr . Nr :: RS^2 : rs^2$. Which is the property of a conic section. The figure is clearly a terminated one, that is, an ellipse.—If, on the other hand, the section M N s S be inclined so as to meet the actual lines A O, B O only once, but the produced line A O′ again, we should evidently have two figures in the cone A O B, and the inverse cone A′ O′ B′, each terminated at N and the corresponding point, but extending infinitely in the other direction. We can prove also the proposition analogous to that just proved with respect to the ellipse, showing that the curve is a conic. Again, if the section N M comes to be parallel to A O, we should have only one branch in the cone A O B, terminated at N, but, in the other direction, going to infinity.—We must content ourselves with this bare exhibition of principles, and with referring our reader to Mulcahy's *Modern Geometry*, to Salmon's *Conic Sections*, to Monge's *Geometrie Descriptive*, and, above all, to Poncelet's *Propriétés des Figures Projectives*, which is still the classical work upon the subject.

Prolate. See SPHEROID.

Property of Matter. A quality by which matter is distinguished from other substances, and which is possessed by all matter, is called a property. There is a division of properties—sufficiently arbitrary—into primary and secondary. The former are those without which we cannot conceive matter's existence,—the latter, those which belong to it, though not, as it is conceived, necessarily. Extension, Impenetrability, and Inertia, are the three which are most usually assigned as necessary and primary properties. No absolute line of distinction can be drawn.

Proportion Harmonic. See RATIO.

Proserpine. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Psychrometry. See HYGROMETRY.

Ptolemaic System. The system or scheme of the celestial motions universally adopted previous to the reformation by Copernicus. It rested on two assertions, supposed to have the force of axioms, viz., *that the earth is at rest and in the centre of the universe;* and *that all motion in the heavens is circular and uniform.* The methods by which the irregular paths and velocities of the planets were seemingly reconciled to these postulates was most ingenious, and has been already explained under ECCENTRIC and EPICYCLE; nor was the system at all deficient in its internal logic. So soon as Copernicus doubted the truth of these principles, its fabric tottered, and swiftly fell. The system, in its earliest and best form, is exposed in *Ptolemy's Almagest;* but it received what was termed its last perfection at the hands of Purbach.

Pulley. See MECHANICAL POWERS.

Pyrheliometer. See ACTINOMETER.

Pyrometer. An instrument intended to measure those higher degrees of heat to which the thermometers of mercury, air, and alcohol are insufficient. It is usual to employ for this the expansion of bars of metal—the amount of that expansion being known—for the different degrees of heat. The principles of the pyrometer will be found stated under EXPANSION. The pyrometer of Wedgwood, which is perhaps as good as any other, is, like all others, very defective. It consists of two slightly convergent pieces of copper, between which a small cylinder of clay is set. This latter contracts by heat, and the convergence is increased in consequence. Its amount being measured, the heat to which the cylinder has been exposed, can be calculated.—It has recently been proposed to construct a pyrometer accurate, to any extent of scale, by aid of the different fusibilities of the metals. The metal last fused will of course, by its degree of fusibility, indicate the heat of the furnace.

Pyrophorus. Any body which kindles of itself on exposure to the air is so called.

Pyxis Nautica; or the Mariner's Compass. One of Lacaille's Southern Constellations.

Pulley. One of the elementary mechanical powers.—See Appendix.

Pump. A well known machine for raising water. Its power is drawn from the pressure or weight of the atmosphere in common cases, and from the elasticity of compressed air in those forms of it that are termed *forcing pumps.* The principles are so simple, and the mechanism so well known, that we shall not occupy our narrow space with special descriptions.—The reader will find all information as to details in the first book on practical mechanics on which he may happen to lay his hands.

Q

Quadrant. The fourth part of a circle. Also a name given to valuable reflecting instruments. See SEXTANT. The ancient form of astronomical instruments for the determination of altitudes was the quadrant; but that has now been wholly superseded by the CIRCLE (*q. v.*)

Quadrature. The term given to modes of determining rectilineal figures that shall be equivalent in area to curvilineal ones. The whole subject belongs to the integral calculus, the general formula being

$$\int \mathrm{X}\, d\, x$$

where X represents that function of x, which is equal to the ordinate y of the proposed curve.

Quaternions. The Editor had the good fortune to expect an article on QUATERNIONS from Sir William Rowan Hamilton. He has obtained the three following most lucid and remarkable letters:—

LETTER I.—*To a Lady.*

DEAR MRS. S——, I am flattered, or at least I am gratified, by your wish to know something about the Quaternions. Really I never hold other people's buttons to talk to them on that subject; but my *own* button is occasionally held, especially by a learned librarian in Dublin, who stops me in the streets, and waylays me in booksellers' shops, to ask for a plain answer to a simple question,—which *you* have not put in *exactly* the same terms,—to wit, "What the deuce *are* the Quaternions?" Well, I must try to do what I can, to satisfy your friendly curiosity.—It would be very easy for me to give a *definition* of a *quaternion;* I mean, of *what I have so called, in mathematics:* for you know that the *term itself* is a good *old English word.* It occurs, for example, in our version of the Bible, where the apostle Peter is described as having been delivered by Herod to the charge of four *quaternions* of soldiers. In the *Paradise Lost*, Adam is represented by Milton as uttering the invocation:—

> "Air, and ye Elements, the eldest birth
> Of Nature's womb, that in *quaternion* run
> Perpetual circle manifold, and mix
> And nourish all things,—
> Vary to your great Maker still new praise."

And to take a lighter and more modern instance from the pages of *Guy Mannering*, Scott represents Sir Robert Hazelwood of Hazelwood as loading his long sentences with "triads and *quaternions.*" In all these cases, of course, the word signifies (as its etymology implies) a *set of Four* (persons, or things, or words); and, on the same plan, Dr. Latham has written a paper on "Phonetic Quaternions," meaning thereby certain *sounds*, which he conceives to group themselves, *four by four;* and I have recently noticed the term "QUATERNIONEN," applied by a very able German author, Moebius, to certain things depending upon *systems of four points.* In fact, the Latin word Quaternio, like the Greek term τετρακτύς, of which it is a translation, seems to mean simply (as above) a *set of four;* or the "NUMBER FOUR," used as a *substantive*, and *not* as a numerical *adjective.* So that, instead of saying that Pythagoras attached a mysterious and hitherto unexplained importance to the Tetractys, we might say that Pythagoras did so to the "QUATERNION:" whatever conception, precise or vague, that wonderful philosopher of antiquity may have associated with the thought of FOUR. (I sometimes fancy that I can conceive an unrecorded *Tetrachotomy* of Pythagoras, analogous to that of Kant, but more mathematically connected with the Pyramid.)—But whatever license, or vagueness, may have heretofore existed, in the employment of the word "Quaternion," it would seem that the courtesy of my contemporaries, at least in these countries, has of late years established an usage—you know Horace's "Si volet

usus, quem penes arbitrium est, et jus, et norma loquendi,"—which for the present, almost or altogether restricts the word, *as a mathematical term*, to the sense in which *I* have employed it. The question then arises:—"What does the *Author* of the Lectures on Quaternions *mean* by the latter word, when employed (as he employs it) *in mathematics?*" And here I repeat that it would be easy for me to embody my meaning in a *Definition*, if *that* could be supposed to be of the slightest earthly use, to persons unacquainted with the subject. I might, for example, in all due form, lay down the following statement:—DEFINITION. "A QUATERNION is the *quotient of two vectors*, or of two directed right lines in space, considered as depending on a system of *Four Geometrical Elements;* and as expressible by an algebraical symbol of *Quadrinomial Form.*" And I might go on to add, as a sort of second *Definition* (or at least *Description*):—"The *Science* or *Calculus*, of Quaternions, is a new *Mathematical Method*, wherein the foregoing *conception* of a quaternion is unfolded, and symbolically *expressed:* and is *applied* to various classes of algebraical, geometrical, and physical questions, so as to discover many new theorems, and to arrive at the solution of many difficult problems."—But though I believe the foregoing statements to be *correct*, and even think that they may be *useful*, as a sort of *recapitulation*, or *résumé*, for those who *already know* a great deal about the matter, what human being, *at first starting*, could be expected to be one bit the *wiser* for them? And, indeed, *what science can be defined*, so as to convey to a person who is only about to *begin* the study of it, anything like a clear and adequate notion of its extent, or even of its nature? "Mais, tenter de définir une science c'est consentir à être inintelligible," says Pouillet, in the second page of the seventh edition of his very lucid and valuable work, *Eléments de Physique Expérimentale, et de Météorologie*, (Paris, 1856,) into which I have been lately dipping.—You have been pleased to allude to Newton. Putting out of sight, for a moment, the enormous difference of *power* between him and myself, let me avail myself of an *illustration* which you have *suggested*, by speaking of his works. Do you suppose that *any one*, however intelligent,—lady or gentleman, it matters not at all,—*could* be made to understand, *by a definition*, "*what* FLUXIONS *are?*" Conceptions, which it has required years of patient thought to mature, are not to be so easily transplanted. They can indeed be made to pass from mind to mind, but not by so short a process; still less can the methods to which they lead be easily or rapidly communicated. "Considerando igitur" (says Newton, in the introduction to his *Treatise on the Quadrature of Curves*) "quod quantitates aequalibus temporibus crescentes et crescendo genitae, pro velocitate majori vel minori qua crescunt ac generantur evadunt majores vel minores; methodum quaerebam determinandi quantitates ex velocitatibus motuum vel incrementorum quibus generantur; et has motuum vel incrementorum velocitates nominando *Fluxiones*, et quantitates genitas nominando *Fluentes*, incidi paulatim annis 1665 et 1666 in Methodum Fluxionum qua hic usus sum in Quadratura Curvarum." The *conception of growing quantity*, though very refined and subtle, and even the additional and still more abstract conception of a *rate of speed* of such *growth*, may (I suppose) be seized by any sufficiently intelligent person, who has had no technical training; but to pass thence to *any degree* of *distinct understanding* of the *mathematical method*, to which its *author*, *after fully possessing* those conceptions, could only *gradually attain* ("incidi paulatim"), requires no inconsiderable amount of formal and scientific preparation, although now pretty widely diffused. The METHOD *cannot* be *explained* to a person who is *not a mathematician;* but (as above admitted) the CONCEPTION *may be communicated:* though not (perhaps) without some patience being needful.—And such—to pass from Fluxions to Quaternions—I believe to be the case with the latter study also. Undoubtedly, I do not pretend to teach any one to *use* my *calculus*, who has not been *already trained* to some extent in *algebra*, and in some rather advanced parts of *geometry*, such as are treated in the Eleventh and Twelfth Books of Euclid. But I have met with at least one lady, who, without having ever *opened* (as I suppose) a single book on algebra or on geometry in her life, was able, after some illustrations on my part, and patience on hers, to *understand perfectly* the *conception* of the *quaternion*, considered *geometrically:*—for I put the *algebraical* view aside for the present, as being much more connected with technicalities and with calculation; and I abstain from introducing here that *metaphysical notion* alluded to, according to which the IDEA of the Quaternion is generated by a certain *Synthesis* of the thoughts of *Time* and *Space*. What *she* did, I am very sure that *you* can do also.—Take, then, an equilateral triangle, A B C, constructed (let us say) on a card, and bisect it by the dotted line C D, drawn (as in fig. 1) from the vertex C, to the middle point D of the base A B. Cut away the half-triangle D B C, and insert, between the legs of the angle D A C, a *curved arrow*, to indicate a conceived *rotation*, from the leg A D to the leg A C, of that *angle*.

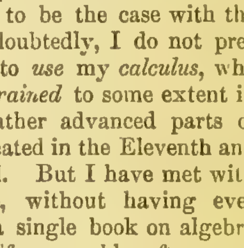


Fig. 1.

Lay the half-triangle A D C, which has been thus cut out and marked, on a square table E F G H, in the way represented in figure 2, so that the line A D may be parallel to the base E F of the square. This being done,

it is clear that the *length* of the side A C is exactly *twice* the length of the half-side A D, of the original and equilateral triangle A B C; and I need not tell *you*, that the *angle* D A C (or the *rotation* represented by the curved *arrow*,) contains exactly *sixty degrees*. Here, then, are already *two numbers* to be considered, in the

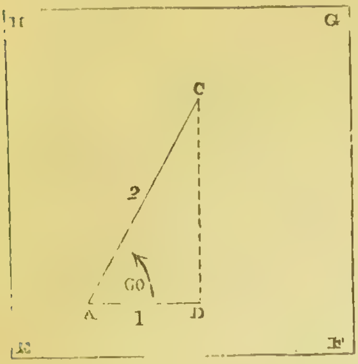


Fig. 2.

comparison of two lines (A C being compared with A D); namely, the number 2, which expresses the *ratio* of the *lengths* of the two lines; and the number 60, which expresses (in degrees) the *angle* between their *directions*. Accordingly, before the introduction of the quaternions, there had existed a branch of mathematical science, invented (I believe) by Argand, in France about fifty years ago, and to which Professor de Morgan of London, has lately

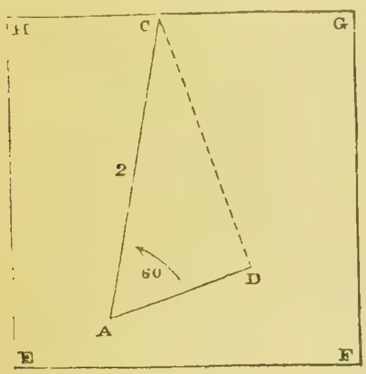


Fig. 3.

given the name of "Double Algebra;" because in it, by the *joint consideration of ratio and angle, two numbers at a time* were treated of, as the subject of every calculation. It should be added that if, after placing the half-triangle A D C as before, we merely *shift its position*, or *turn it about*, on the *fixed table* E F G H, (without turning it upside down,) so as to bring it to the state represented in figure 3, we do not alter thereby *either* of the two foregoing numbers, 2 and 60; nor do we change the *direction of the rotation* (indicated by the arrow), from the line A D to the line A C; and, therefore, the *complex relation* of the latter line to the former (*including a relation of lengths*, and *also* a *relation* of *directions*) is not conceived as having undergone *any change;* nor would any alteration in the *twofold relation*, above considered, be occasioned by the substitution of a *smaller* (or *larger*) but *similar* and *similarly placed triangle*, in any of the foregoing constructions. Such seem to me to be the most, or among the most, essential elements, of the *geometrical conception* of DOUBLE ALGEBRA: but it remains to be shown by what sort of *transition* I pass from thence to an algebra of a QUADRUPLE character, or from COUPLES to QUATERNIONS in Geometry.—And this transition *when once thought of*, appears *so easy*, that it may seem *wonderful* how it had *failed to occur* to others before myself. I simply use a *desk* instead of a *table*. I *elevate* the old square E F G H, (not that there is any magic in its being precisely of a *square form*,) so that while its *base* E F still rests, *as a ledge*, on a fixed horizontal plane, though perhaps in a *new position* thereon, its *top* G H may be at a certain *height** above that plane; there being then a certain *angle of inclination*, K E H in figure 4, suppose an angle of *ten degrees*, whereby the *desk*, E F G H,

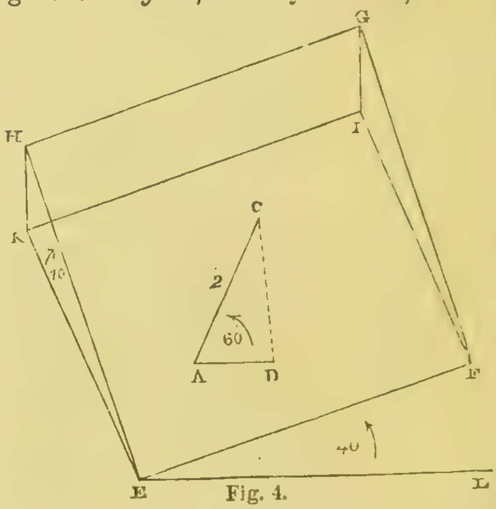


Fig. 4.

is *raised above the table*, E F I K L; which *angle* K E H thus furnishes a new or *third number*, namely, here the number 10, to express or measure the *slope of the desk*. But even when this *slope* has been determined, the *aspect of the desk* is not yet completely known, until we have fixed the *direction of its ledge*, E F; or the *angle* L E F, suppose *forty degrees*, which this ledge makes *with some fixed line*, E L, traced on the horizontal plane, or *table*: because we may conceive the moveable desk to be *turned about* thereon, for the sake (as we may imagine) of getting a *better light*. Here, then, is a FOURTH NUMBER, namely, in this case the number 40, which (in my view of the subject) enters *essentially* into the *complex conception of the relation of one directed line in space*, A C, *to another directed line*, A D; and, therefore, into the conception of what I call the "QUOTIENT" of one such line *divided* by another (compare *here* my *Definition* of a Quaternion): namely, the *complex result*, obtained by the *comparison* of those two lines: or more fully, by the *examination, how one line is related to the other, in length and in direction;* the PLANE of the two lines being (according to me) a thing to be *specially studied*, in conducting such an examination.—You see then, clearly, how the *comparison of one line with another*, conducted as above, (namely, the comparison of A C with A D

* The reader will have the goodness to fancy the line G H thus *elevated*, although the figure, as drawn, does not, perhaps, suggest it.

in the foregoing figures,) leads to the consideration of a SYSTEM OF FOUR NUMBERS, in our example

2, 60, 10, and 40;

which may be said to represent or measure, respectively,

"RATIO, ANGLE, SLOPE, and LEDGE;"

though, in fact, the third and fourth numbers, *as well* as the second, represent *angles;* namely, those which answer to the *inclination of a planet's orbit*, and the *longitude of its ascending node*, in astronomy. The systematic introduction of *these two latter angles* into *geometry*, by the assistance of my *peculiar symbols* (i, j, k), or the doing something *equivalent* thereto, so as in some way *to take account of the aspect of the desk* (or plane), whereon any proposed *angle* (as here the original angle D A C of sixty degrees) is *traced*, may be said to be the *chief geometrical characteristic* of the CALCULUS OF QUATERNIONS, and what mainly serves to *distinguish* it from its predecessor, the *Calculus of Couples:* or (with reference to geometrical application) to distinguish *Quadruple* from *Double Algebra.* But, of course, without *some preparation* (such as perhaps you possess), by the study of *common algebra*, it is impossible to understand *how* this *conception* is *worked out* into a system of *calculation.* Meanwhile, I remain, dear Mrs. S——, very truly yours, WILLIAM ROWAN HAMILTON.

LETTER II.

OBSERVATORY, ——, 1856.

MY DEAR SIR,—You say that you have read a certain letter of mine to a lady, on the subject of the conception of a *Quaternion in Geometry*, as the *Quotient of two Vectors*, or of two directed right lines in tridimensional space: and that you wish to see how this *conception* can be so developed, as to be shown to be the basis of a *Calculus.*

I. Allow me, in the first place, to remind you, that the "*quotient of two co-initial vectors*," or of two right lines in space which are drawn from one common point, has been conceived by me as being the complex *result* of the *comparison* of one such vector with another, this comparison being performed in each of *two* fundamental respects: namely, *first*, as regards the *relative length* of the two lines; and, *secondly*, as regards their *relative direction.* The former part of the comparison conducts to the consideration of a RATIO between the two lengths, which may be supposed to be accurately or approximately expressed by a *number* (whether integral or fractional, and whether commensurable or incommensurable); the latter part of the same comparison conducts to the consideration of an ANGLE between the two directions, or of a certain *elongation* of the one from the other; and also to the consideration of a PLANE, wherein the two lines are jointly contained. The *angle* may be expressed by a *second number*, of degrees and parts of a degree, or of right angles and parts of a right angle; and the *plane* has an ASPECT, which depends upon a certain *pair* of *other angular elements*, such as those which in astronomy are called the *inclination* of a planet's or comet's orbit, and the longitude of its ascending *node:* and which, in my former letter, were illustrated by the *slope* of the *desk*, whereon the angle between the two lines was supposed to be traced, and by the direction of the *ledge*, wherein that desk was met by the *table.* There enter thus, upon the whole, *three angular elements*, such as those which have above been called *elongation*, *inclination*, and *node*, (or, more briefly, *angle*, *slope*, and *ledge*,) into the complex conception, as above unfolded, of the *relative direction of two lines in space.* And when these are combined with the former element of *ratio*, (or of the *relative length* of the two lines,) they make up jointly a SYSTEM OF FOUR ELEMENTS, expressible by a system of *four numbers*, which enter (in the view here adopted) into the *complete conception* of the QUOTIENT *of two Vectors*, and constitute that *quotient* a QUATERNION.

II. So much having been premised, or repeated, as regards the *conception* of a geometrical quotient, or quaternion, of the kind which it is proposed to consider, we are next to fix what is to be understood by *equations* between such quotients, and by *operations* upon them. Adopting the *notation of fractions*, for each such quotient in particular, and supposing that the symbols

$$\mathrm{OA},\ \mathrm{OB},\ \mathrm{OC},\ \mathrm{OD}, \qquad (1)$$

or more concisely

$$\alpha,\ \beta,\ \gamma,\ \delta, \qquad (2)$$

denote some *four co-initial vectors*, or right lines drawn in space from some one common origin O, we must proceed to *compare* and to *combine* the *two quotients*, or *geometrical fractions*,

$$\frac{\mathrm{OB}}{\mathrm{OA}} \text{ and } \frac{\mathrm{OD}}{\mathrm{OC}}, \text{ or } \frac{\beta}{\alpha} \text{ and } \frac{\delta}{\gamma}. \qquad (3)$$

More fully, we must determine, without any ambiguity, what shall be *meant*, in the applications of quaternions, by an *equation* of the form,

$$\frac{\mathrm{OD}}{\mathrm{OC}} = \frac{\mathrm{OB}}{\mathrm{OA}}, \text{ or } \frac{\delta}{\gamma} = \frac{\beta}{\alpha}; \qquad (4)$$

and must fix the geometrical interpretation of each of the four elementary *combinations*,

$$\frac{\mathrm{OD}}{\mathrm{OC}} + \frac{\mathrm{OB}}{\mathrm{OA}}, \text{ or } \frac{\delta}{\gamma} + \frac{\beta}{\alpha}; \qquad (5)$$

$$\frac{\mathrm{OD}}{\mathrm{OC}} - \frac{\mathrm{OB}}{\mathrm{OA}}, \text{ or } \frac{\delta}{\gamma} - \frac{\beta}{\alpha}; \qquad (6)$$

$$\frac{\mathrm{OD}}{\mathrm{OC}} \times \frac{\mathrm{OB}}{\mathrm{OA}}, \text{ or } \frac{\delta}{\gamma} \times \frac{\beta}{\alpha}; \qquad (7)$$

$$\frac{\mathrm{OD}}{\mathrm{OC}} \div \frac{\mathrm{OB}}{\mathrm{OA}}, \text{ or } \frac{\delta}{\gamma} \div \frac{\alpha}{\gamma} \qquad (8)$$

in such a manner as to show that each represents in general a certain *definite* fraction, or quotient,

or quaternion, when the two *separate* fractions (3) are *themselves* definitely known; each by its own system of four elements, of the kind already described.

III. With regard to the EQUATION (4), it is natural to consider this as expressing, as a *part* of its signification, that a geometrical *proportion*, of the kind considered by Euclid, exists between the *lengths* of the four lines (1) or (2); so that

$$\text{OD} : \text{OC} :: \text{OB} : \text{OA}, \qquad (9)$$

or,

$$\delta : \gamma :: \beta : \alpha, \qquad (10)$$

when these lengths alone are attended to. For example, if the line β be double of α in length, then δ must at the same time be twice as long as γ. But *besides* this proportion of lengths, there must exist also a certain *proportion of directions*, or some relation which may be spoken of as such, in order to *complete* the conception of the supposed *equality of quotients*, for the two pairs of *vectors*. It is therefore natural to *define*, that whenever the equation (4) is given, we are to understand, as *another part of its signification*, (besides the proportion (9) or (10),) the following *equation between angles*,

$$\text{C}\hat{\text{O}}\text{D} = \text{A}\hat{\text{O}}\text{B}; \qquad (11)$$

or,

$$\hat{\gamma\delta} = \hat{\alpha\beta}. \qquad (12)$$

In short, to *combine* geometrically the conditions hitherto stated, we are to conceive that if the two points A, B be joined by one right line, and the two points C, D by another, as in the figure herewith annexed, the *two triangles*

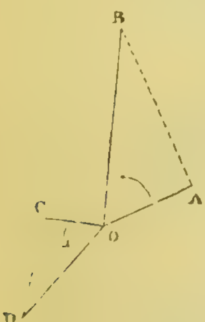


Fig. 1.

$$\text{A O B and C O D}, \qquad (13)$$

shall be *similar*: to which, however, I add this other condition, that these two triangles shall be situated *in one common plane*, and shall be *similarly turned* therein, as indicated by the *curved arrows* in the figure.

IV. And such, in fact, is the fundamental conception, which appears to have been first proposed by Argand, in an Essay published in Paris in 1806; for although his work of that date seems to be now lost, yet a sufficient sketch of it has been preserved, in Gergonne's *Annales de Mathématiques*, (tome iv., published in 1813,) to enable us to know its nature, and its chief results. And in speaking of his extended view of *proportion in geometry*, as applied to *directed lines in one fixed plane*, Argand expresses himself thus (in pages 136, 137, of the above cited volume):—"Si (fig. 2) *Ang.* A K B = *Ang.* A′ K′ B,′ on a, abstraction faite des grandeurs absolues,

$$\text{KA} : \text{KB} :: \text{K}'\text{A}' : \text{K}'\text{B}'.$$

C'est là le principe fondamentel de la théorie dont nous avons essayé de poser les premières bases, dans l'écrit dont nous donnons ici un extrait." (This refers to the printed Essay of 1806, which had been exhibited to Gergonne, and through him to Argand's generous rival, Français.) Argand continues thus:—"Ce principe n'a rien au fond de plus étrange que celui sur lequel est fondée la conception du rapport géometrique entre deux lignes de signes différents, et il n'en est proprement qu'une généralisation." [See the notes to the Preface to my published Lectures on Quaternions.] It ought to be added that Argand's *angles* A K B, A′ K′ B′, were supposed to be not merely *equal* in *amount*, but also *similarly* (not *oppositely*) *measured*, as regards the *directions* of the *rotations* corresponding; just like the angles A O B, C O D, in the fig. of the present letter, as it has been remarked that the *curved arrows* indicate. This fundamental *view* of Argand, which appears (as has been said) to have been published exactly fifty years ago, has since occurred independently to several other writers, (for example, to the illustrious Gauss,) or has been adopted by them, as the basis of a theory of *proportions of lines within one plane;* and it is one of the most essential elements of that modern branch of mathematical science, for which my valued friend, Professor De Morgan of London, has proposed the name of "DOUBLE ALGEBRA."

V. The same view of Argand, so far as it has just now been described, respecting such *proportions of directed lines*, is (as you have seen) adopted by myself, in my *transition* from *Double* to QUADRUPLE *Algebra*, or from *Couples* to QUATERNIONS in Geometry. But because I consider *my angles* as traced upon a *variable plane*, or *desk*, which may take all possible positions, or *aspects* in space, and *not* as traced on one fixed *table*. I am obliged to *sacrifice* a subsequent *simplification* which Argand, *in his less extensive theory*, was able to introduce, and in which he has been followed by many other authors since. For I am unable to assume, as he and they have done, *any one direction* as a *fixed standard*, with which all others are to be compared, and which they treat as the *direction of positive unity*. And my *reason* for abandoning any attempt at *such* a standard, is simply (as you see) that *no one line* (even through a fixed point O) is *common to all the planes* which can be drawn (even through the same fixed point). If this appears to be an *inconvenience*, at first setting out, and if, in fact, it compels me to assume *rules for multiplication* in geometry, which are altogether *unlike old rules*, in *one* essential respect, ($ij = -ji$,) I must plead that it is *not my fault*, if *tridimensional space* be a somewhat *more complex conception* than space of only *two dimensions*. And, on the other hand, I claim that an *advantage*, and a *power*, are *gained*, by the *changes* which are thus necessitated. For, precisely because the *aspect of a plane* has been made by me to enter, essentially, into the *conception* of the geometrical quotient of two directed lines, I find myself obliged to

interpret the EQUATION (4) as *including* the assertion, that the *plane of the pair* γ, δ, *coincides* with the plane of the *other* pair of lines α, β, (if all the four lines be still supposed to be co-initial); or that the *five points*, O, A, B, C, D, are all situated in *one common plane.* Thus the one pair of lines, *considered as belonging to a plane*, must have the *same pair of angular elements* (inclination and node), as the other pair of lines compared. And here you see another justification of the use of the *word* "QUATERNION," in this whole theory: for you perceive that a *single formula*, such as either of those above marked as (4), does not merely include *two equations* of the *ordinary kind*, as is the case in Double Algebra; but that it includes *four such equations.* For it expresses, according to my interpretation, *not only* that a *proportion of lengths*, and an *equality of elongations*, are supposed to exist, but *also* that a certain PAIR OF COPLANARITIES is conceived to hold good: namely, that the third line, γ, and that the fourth line, δ, are *both* coplanar with the first and second lines, α and β.—When I say here "coplanar," or speak of "coplanarity," (and perhaps to say *complanar* might be more elegant,) you will easily see that it is *not necessary* to *insist* on the two equal angles,

$$\alpha \hat{} \beta \text{ and } \gamma \hat{} \delta,$$

being both in *one identical plane.* It is quite enough, if they be situated in *parallel planes;* which may happen by our taking a *new origin*, O′, for the *new pair* of vectors, γ, δ; or by our comparing two angles, or two similar triangles,

$$\text{A O B and A}' \text{ O}' \text{ B}', \qquad (14)$$

in planes which shall be parallel to each other.

VI. Retaining, however, for simplicity, the conception of the *common origin* O, you see clearly how, on the foregoing principles, it is possible to find, without any ambiguity, the *fourth proportional*, O D, to *any three directed lines* from O, such as O A, O B, O C, which are in *any one plane;* whether that plane be *fixed*, as in Double Algebra, or *variable* as in Quaternions. Suppose, then, in the next place, that some two pairs of lines, O A, O B, and O C, O D, are given, which are *not* thus situated in any *common* plane; and let O E be an arbitrarily assumed portion of the *line of intersection of the two planes*, A O B and C O D; so that the line O E shall be at once coplanar with the pair O A, O B, and *also* with the pair O C, O D, although these two pairs are *not now* coplanar *with each other.* Then, by our principles, *two lines*, O F and O G, can be determined, one in the plane A O B, and the other in the plane C O D, in such a manner as to satisfy the two equations,

$$\frac{\text{O F}}{\text{O E}} = \frac{\text{O B}}{\text{O A}}, \quad \frac{\text{O G}}{\text{O E}} = \frac{\text{O D}}{\text{O C}}; \qquad (15)$$

and thus, to borrow a phrase from *arithmetic*, though transferring it here to *geometry*, *any two* proposed *geometrical fractions* considered as being each a *quotient of two lines*, can be *reduced to a common denominator;* or can be equated respectively to two fractions, or quotients which shall have one common divisor line, O E. (If the two planes, A O B and C O D, should happen to coincide, *any line* from O in this common plane might be taken for this line O E; but in *general* it must be, as above, a part of the line of *intersection* of the planes.) It is also evidently possible, on the same plan, to determine another line O H, in the plane A O B, which shall satisfy this other equation,

$$\frac{\text{O H}}{\text{O E}} = \frac{\text{O A}}{\text{O B}}, \text{ or } \frac{\text{O E}}{\text{O H}} = \frac{\text{O B}}{\text{O A}}. \qquad (16)$$

And when the lines, O E, O F, O G, O H, have been so chosen as to satisfy the foregoing equations, (15) and (16), all necessary *preparations* are completed, for assigning, in what seems to me the simplest possible way, and in what is at all events the one adopted in the doctrine of Quaternions, the *interpretations* of all the *four fundamental combinations*, which have been proposed for consideration in paragraph (II.) of this letter.

VII. For this purpose we have only to admit, as relations transferred from *Algebra*, or rather from the *arithmetic* of vulgar fractions, to *geometry*, that the following formulæ shall *still* be considered to hold good, *by definition*, in the *new* theory, as they do in older ones:

$$\frac{\gamma}{\alpha} + \frac{\beta}{\alpha} = \frac{\gamma + \beta}{\alpha}; \quad \frac{\gamma}{\alpha} - \frac{\beta}{\alpha} = \frac{\gamma - \beta}{\alpha}; \qquad (17)$$

$$\frac{\gamma}{\beta} \times \frac{\beta}{\alpha} = \frac{\gamma}{\alpha}; \quad \frac{\gamma}{\alpha} \div \frac{\beta}{\alpha} = \frac{\gamma}{\beta}; \qquad (18)$$

the symbols α, β, γ being here used to denote *any three vectors*, or directed right lines in space; and the *addition* and *subtraction* of such *lines* being *interpreted* as they are in a great number of modern systems, including again the theory of Argand. According to this modern theory, which was perhaps in *that* point anticipated (though somewhat dimly and vaguely, as I think) by Buée, (see again the notes to the printed Preface to my Lectures,) *lines* are added and subtracted, according to the *rules* of *composition* and *decomposition* of *motions* (or of *forces*). Complete, therefore, the two *parallelograms* F O G I, G F O K; and draw the diagonal O I; (fig. 2) the letters F

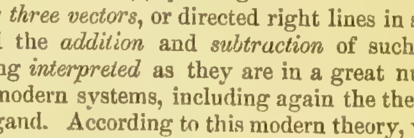


Fig. 2

and G, as well as E and H, denoting still the same points, as in the equations (15) and (16). We shall then have the FOUR following *fundamental formulæ*, for the *four operations* of the *addition*, *subtraction*, *multiplication*, and *division* of ANY TWO QUATERNIONS, or geometrical quotients, or fractions, of the kind already mentioned:

$$\frac{\mathrm{O\,D}}{\mathrm{O\,C}} + \frac{\mathrm{O\,B}}{\mathrm{O\,A}} = \frac{\mathrm{O\,I}}{\mathrm{O\,E}}; \qquad (19)$$

$$\frac{\mathrm{O\,D}}{\mathrm{O\,C}} - \frac{\mathrm{O\,B}}{\mathrm{O\,A}} = \frac{\mathrm{O\,K}}{\mathrm{O\,E}}; \qquad (20)$$

$$\frac{\mathrm{O\,D}}{\mathrm{O\,C}} \times \frac{\mathrm{O\,B}}{\mathrm{O\,A}} = \frac{\mathrm{O\,G}}{\mathrm{O\,H}}; \qquad (21)$$

$$\frac{\mathrm{O\,D}}{\mathrm{O\,C}} \div \frac{\mathrm{O\,B}}{\mathrm{O\,A}} = \frac{\mathrm{O\,G}}{\mathrm{O\,F}}. \qquad (22)$$

And it may not be too much to say, that *no other fundamental principle* is requisite to be assumed, for the establishment of the *doctrine of quaternions*, regarded as a part of *geometrical science*, wherein *rules of calculation* are employed, which have been made to *resemble* the rules of *Algebra*, or of *arithmetic*, as closely as the *nature of the subject* appears to me to admit of their being assimilated.

VIII. Conceding, however, that the foregoing conceptions or assumptions, are simple enough in themselves, and are combined with sufficient simplicity, you may still, with the most perfect fairness, demand, of what *use* are they? Do they lead to any *results*, before unknown? Are any new *expressions* supplied by them? Do they assist in any *researches* of science? Are they in any way adapted to become the basis of a new mathematical *method?* I think, that without entering deeply into any details of calculation, these questions may be proved to admit of being answered in the affirmative. And this will, perhaps, be felt to have been done, even if the limits of the present letters shall allow me to do no more than to point out briefly, that *equations* which *appear* to be mere *truisms*, and which at all events are *familiar*, as *forms*, to the merest *beginner* in *Algebra*, (such as the equation $s\,.\,rq = sr\,.\,q$,) have been found to acquire a *new significance*, and to include *expressions of new theorems in spherical geometry*, and generally in the *geometry of three dimensions*, when they are *interpreted* on the foregoing principles. And the conviction thus acquired may be strengthened, if it shall be shown, that what-seems to be the *only* but the fundamental *paradox* of this new CALCULUS of Quaternions, when contrasted with ordinary Algebra, namely, the *non-commutative property of multiplication*, ($ij = -ji$,) admits of a clear and useful *interpretation*, in connection with a subject which is so important in geometry and in mechanics, as is the doctrine of the *composition of rotations*. But I have written enough for one time; and propose, with your permission, to reserve for another opportunity the continuation of any such remarks: respecting which I feel at least the assurance, that you will find no difficulty whatever in following them.—I therefore close, for the present, by adding only that I remain, my dear Sir, very truly yours,

WILLIAM ROWAN HAMILTON.

——, Esq.

LETTER III.

OBSERVATORY, ——, 1857.

MY DEAR SIR,—You have been pleased to express a wish, that I should continue somewhat further my remarks on the Quaternions; and should explain the sense in which it was stated in my last letter, that so *simple* and *familiar* a *form*, as the equation

$$s\,.\,rq = sr\,.\,q,$$

acquires a *new* and *important significance*, in the *geometrical applications* of my Calculus: while, on the other hand, a formula, which appears at first sight so *strange* and *paradoxical*, as the equation

$$ij = -ji,$$

is found to admit of a *clear* and *useful interpretation*, connected with the theory of *composition of rotations*. I shall try, then, to make my meaning perfectly clear, on at least the former of these two points, in the present letter, which (if you please) may be numbered as LETTER III. of this little series; the "Letter to a Lady" being considered as number I., and the former communication to yourself being therefore counted as Letter II. You will permit me to continue also, for the sake of convenience of reference, the numbering of the paragraphs, and of the formulæ.

IX. The *associative* character of *multiplication*, expressed by the first of the two recent equations, namely by the formula,

$$s\,.\,rq = sr\,.\,q; \qquad (23)$$

or, more fully by the statement, that

$$\text{if } rq = s', \text{ and } sr = q', \text{ then } ss' = q'q; \qquad (24)$$

is a result so universally admitted, in arithmetic and in ordinary algebra, that the only *difficulty* here may seem to be the difficulty *how*, in quaternions, the *re-assertion* of this *old property* of the operation of multiplication can involve anything *new*, or which may deserve the name of a THEOREM. You will find, I think, that this first difficulty is completely met and overcome by the consideration, (which you know already that quaternions *require*,) of *angles* in *different planes*, or of *rotations* about *different axes*. But it may be useful first to throw a backward glance, on some of the chief senses in which the *associative property*, above mentioned, has long been *admitted* to hold good, in applications to more elementary subjects.

X. In the arithmetic of *whole numbers*, if we take, as an example, the following values of the factors,

$$q = 2,\ r = 3,\ s = 4,$$

we shall have

$$s' = rq = 3 \times 2 = 6,\ q' = sr = 4 \times 3 = 12$$

and here it is clear that $ss' = q'q$, as asserted in (24), because $4 \times 6 = 12 \times 2 = 24$. And the *rationale* of the process might be exhibited to a child, by arranging 24 dots in a line, as follows:

| . . . . . . | . . . . . . | . . . . . . | . . . . . . |

four sets, of six dots each, being thus separated from each other by vertical lines; and the six

ots, in each set, being distributed into *three* airs. For it would thus be seen, at a glance, hat the *four sixes* made exactly *twelve pairs*; nd that a corresponding result must hold good, n every similar example.

XI. Again, when we pass from whole numbers to *vulgar fractions* in arithmetic, and treat he three given factors, q, r, s, as denoting three ich fractions, we have only to take the whole number a, which is the product of the three given enominators, and to form from it successively he three other whole numbers,

$$b = q\,a,\quad c = r\,b,\quad d = s\,c; \qquad (25)$$

r then we shall have the transformations,

$$q = \frac{b}{a},\ r = \frac{c}{b},\ s = \frac{d}{c},\ s' = \frac{c}{a},\ q' = \frac{d}{b}; \qquad (26)$$

nd the associative property of the multiplication f fractions will be verified by observing that

$$\frac{d}{c} \cdot \frac{c}{a} = \frac{d}{b} \cdot \frac{b}{a} = \frac{d}{a}. \qquad (27)$$

or example, if

$$q = \tfrac{2}{3},\ r = \tfrac{4}{5},\ s = \tfrac{8}{7},$$

en we shall have the values,

$$a = 105,\ b = 70,\ c = 56,\ d = 64;$$

nd the verification will consist in observing that

$$\frac{64}{56} \cdot \frac{56}{105} = \frac{64}{70} \cdot \frac{70}{105} = \frac{64}{105}.$$

XII. If the three factors, q, r, s, remaining *positive*, should become *incommensurable* (or *surd*), ppose

$$q = \sqrt{2},\ r = \sqrt{3},\ s = \sqrt{5},$$

en the associative principle of multiplication ould be verified, by assuming any *continuous agnitude*, such as a *length*, which we may call a and deriving from it successively three other agnitudes of the same kind, b, c, d, by the conions (25), considered here as indicating the three *oportions*,

$$b : a = q : 1;\quad c : b = r : 1;\quad d : c = s : 1. \qquad (28)$$

r then, by the meaning of multiplication, conered here as corresponding to the *composition* *ratios*, we should have these two other proporons, answering to the two last equations (26),

$$c : a = r\,q : 1;\quad d : b = s\,r : 1; \qquad (29)$$

d the associative principle (23) might be exessed by the equations (27), or by the statent that the *ratio* $d : a$ may at pleasure be garded as *compounded*, either of the two ratios c and $c : a$, or also of these two other ratios, b and $b : a$. In such a manner that, by omitg the *point* in the expression of the ternary prot $s\,r\,q$, we may write simply, as our final ult,

$$d : a = s\,r\,q : 1. \qquad (30)$$

XIII. When we pass from arithmetic to *Alra*, and introduce the consideration of *positive* d *negative* quantities, or numbers, the associae equation (23) is still well known to be true; it involves now the *new element* of the *rule of signs*, namely (as its chief part), the rule that o negative factors give a positive product; of which known rule the simplest interpretation appears to be this, that *two reversals restore a direction*. In *Double Algebra*, this rule was *extended* by Argand, in the manner mentioned (or suggested) in my former Letter (II.); but the associative property of multiplication still holds good, in Argand's extended theory, because the *addition of angles* in one plane, as well as the *composition of ratios* of the lengths of lines, is obviously an *associative operation*.

XIV. What, then, was the *difficulty* of extending to *quaternions* so simple and familiar a result; or of showing that the apparently obvious formula of multiplication, $s\,.\,r\,q = s\,r\,.\,q$, is *still* a *true* equation, in the *new* theory, as well as in the old ones? The difficulty, as has been hinted, consisted in this, that I had to consider *three given rotations*, (q, r, s,) *in three different planes*, and to *compound* them variously, into one final or *resultant rotation;* the process introducing *two new and auxiliary planes* of rotation, answering to the two partial or *binary products* ($r\,q$ and $s\,r$, or s' and q'); and the final result, or the *ternary product* ($s\,r\,q$), being constructed by a rotation in a *sixth plane.* And it is this essential *reference to a system of six planes*, which in the doctrine of Quaternions appears to me to elevate the old *associative equation*,

$$s\,.\,r\,q = s\,r\,.\,q,$$

to the rank of an *expression*—not the less important on account of its extreme simplicity—of what I have found to be a fertile *theorem of solid geometry:* which may indeed be enunciated in various ways, but which, under all its forms, requires and admits of *demonstration.*

XV. To make more entirely clear in what sense the equation (23) is treated, in my Calculus, as expressing a theorem respecting composition of *rotations*, allow me to resume the first equation (18), which I adopt from ordinary algebra, namely, the formula,

$$\frac{\gamma}{\beta} \cdot \frac{\beta}{\alpha} = \frac{\gamma}{\alpha};$$

where α, β, γ may, as in my former Letter (II.), denote any three *vectors*, O A, O B, O C, supposed, for the sake of simplicity, to diverge or radiate, in any arbitrary directions, from one common *origin*, O. And let us now conceive, in particular, that the point B is determined on the line O C, (or on that line prolonged through C,) by describing, round O as centre, a circle with O A for radius, and in the plane A O C; and in such a manner that the *length* of the line O B may be *equal* to that of the line O A, but that the *angle* B O C may *vanish;* or that we may write

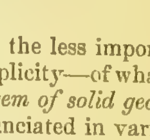


Fig. 3.

$$\text{length of } \beta = \text{length of } \alpha; \qquad (31).$$

but also,

$$\text{angle between } \beta \text{ and } \gamma = 0 \qquad (32).$$

You will observe that I *abstain* from writing the equation, $\beta = \alpha$; because, in this whole theory, as in Double Algebra, the assertion of an *agreement of directions* is considered as an essential element, in every assertion of an *equality of directed magnitudes:* whereas the line β has here the direction of the line γ, but *not* that of the line α. But *because* the direction of β thus *coincides* (in our present construction) with that of γ, their *quotient*, $\frac{\gamma}{\beta}$, *degenerates*, in the present case, from a *quaternion* to an ordinary *ratio*, or to the numerical expression of such a ratio, between two mere *lengths:* and it is *this ratio*, or its numerical expression, which I call the "TENSOR" of the quaternion $\frac{\gamma}{\alpha}$ as containing the rule for *extending* (or, as it were, *stretching*) the line α, till it becomes *equal in length* to the line γ. On the other hand, because the *lengths* of α and β are *already equal* (in our present construction), in comparing *these* two lines we have to *compare directions only*. And the result of *such* a comparison, or the *quotient* $\frac{\beta}{\alpha}$, of two lines which are *equally long*, I call a "VERSOR;" or more fully, I call it *the Versor of the quaternion*, $\frac{\gamma}{\alpha}$, with the construction recently described: because, when regarded as an *operator*, or (in a certain technical sense) a *multiplier*, in the following formula, *adopted* from algebra,

$$\frac{\beta}{\alpha} \cdot \alpha = \beta, \tag{33}$$

its effect consists in merely *altering the direction* without in any way changing the *length*, of the *operand line*, α; which line, by this conceived *version* (or rotation), is changed to the equally long line, β, and is thereby made to take the *direction* of γ. Indeed, I have lately found that Mourey, who was one of the many re-inventors of some of Argand's results, proposed (in a curious little book, which was published at Paris in 1828) to use (or perhaps to *coin*) the French word "verseur," with a signification almost the same, for the case of *angles in one fixed plane*, though not (as here) for *angles in space*. In my Calculus, for the reasons above assigned, I regard *every quaternion q*, such as our recent quotient $\frac{\gamma}{\alpha}$, as being the *product of two factors*, whereof one $\left(\text{as lately } \frac{\gamma}{\beta}\right)$ is the *tensor*, and the other $\left(\text{as } \frac{\beta}{\alpha}\right)$ is the *versor*, of that quaternion. And I use the two Roman capital letters, T and U, to denote the *two operations*, of *taking the tensor*, and of *taking the versor*, respectively, in such a manner as to write, *generally*, the formula,

$$q = \mathrm{T}\,q \,.\, \mathrm{U}\,q\,; \tag{34}$$

which is to be interpreted by the help of the recent construction, or by our last figure, and contains one of the most frequently occurring *transformations*, or *decompositions*, of a quaternion: being, indeed, connected very intimately with that *geometrical conception*, on which this whole theory and calculus is founded.

XVI. After these general remarks, if we now return to the associative equation (23), and abstract from the known property of *common* multiplication, depending on the mere *composition of ratios* of magnitude, and already referred to in this letter, we may *dismiss* the consideration of the *tensors* of the three given quaternions, q, r, s, and attend only to the *versors* of those quaternions; or which will come to the same thing, we may treat those three *factors*, and their partial and total *products*, as being *themselves* quaternions of the versor kind. But *every multiplication of versors* corresponds to, and is (on my plan) constructed by, some suitable *composition of rotations*. For if we resume the equation,

$$\frac{\gamma}{\beta} \cdot \frac{\beta}{\alpha} = \frac{\gamma}{\alpha}\,; \tag{35}$$

— supposing now that the three lines α, β, γ, are all *equally long*, (or, at least, *abstracting* from their *lengths*), we perceive that the formula expresses merely, on my principles, that we think of ourselves as *turning a line*, first in one plane from the *direction* α to the *direction* β, and then from this *second direction* β to a *third direction* γ, in a *plane* which will generally be *different:* and as then inquiring, *by what compound or resultant rotation* we might have passed, *at once*, *in a third plane*, from the *initial direction* α to the *final direction* γ; which *one* resultant or *compound rotation* we regard as being *equivalent* to the *system* of the *two* given or *component rotations*. In fact, the *three lines* α, β, γ, if drawn as before from any common origin, will not only make *three rectilinear angles* with each other, but also may be regarded generally as the *three sides* (or edges) of a certain solid and *triedral angle*: the *three faces* of which solid angle are the *three planes* that contain them, *two by two;* such as the three planes EOG, EOH, GOH, which were connected with the formula of multiplication (21), and with the equations (15) and (16) of Letter II. Now, on applying these general views, respecting multiplication of versors, to the case of the equation (23), or to the fuller statement (24), we see that since there are in it four such multiplications to be considered, namely, those which have been denoted by

$$r\,q,\ s\,r,\ q'\,q, \text{ and } s\,s', \tag{36}$$

there must in general be *four triedral angles* to be constructed, or at least to be conceived, for the *full* interpretation of that *associative*

inciple, to which the present Letter mainly lates. Of these four solid angles, (36) if they still supposed to have one *common vertex*, e two first have a *common plane*, namely, e plane of the *versor* r; and they have also eir *two rectilinear angles* in that plane *equal*, d *similarly directed*, as representing each the *ation* which answers to that factor. In like anner, the consideration of the versor q shows at the first and third of the four solid angles, *triedrals*, (36), have one common plane, and o equal angles therein. The versor s estables a similar connection between the 2d and 1 of the triedrals. And because, by (24), $= r\,q$, and $q' = s\,r$, the *binary product* s' connects in the same way the 1st and 4th triedrals; d the *other* binary product q' connects the 2d d 3d. But there are *still* the 3d and 4th, to (if possible) connected by the same kind of ations. It is to be shown that *these two tri-als* (answering to the products $q'\,q$ and $s\,s'$) *e also one plane common, and the two angles that plane equal, and similarly directed:* for erwise we should not have the *associative ution* (23), and it would be improper to *omit point*, in the expression of the *ternary pro-t*, $s\,r\,q$. And it is clear that the assertion of *s symbolical result* involves, when *interpreted* above, on the principles of quaternions, the ertion of a *geometrical theorem*, (or rather of a *r of geometrical theorems*,) which is by no ans of an *obvious* character. For you see, that only have we to consider, as stated in paraph XIV., a system of *six planes*, but also, in *h* plane, an equation between *two angles;* inling, for every plane, an agreement, of the d often called *algebraical*, because it can be ressed by plus and minus, between the *direc-s* of the two *rotations* compared. And you nit, of course, that such conclusions as these uire to be formally *proved*, and are not to be *umed* to be true, by any mere *instinct* of *ana-y;* or from any natural *pleasure* felt, in the *eralizations* of symbolical *language*. The reing theorem *looks* very simple, when it is *ressed* under so familiar a *form*, as that of the ady often cited equation (23); and an intor might perhaps be pardoned, on that acnt, for *wishing* that it should turn out to be : but we should have profited ill by the exence of former explorers, if we were to forget t such a *wish* cannot supersede the necessity rigorous *examination*, into what *is*, in reality, *truth*. A formal *demonstration* is requisite; such a demonstration, under more forms than I believe myself to have elsewhere given:* lough I may perhaps be induced, at some ire and not distant time, to resume the entire ject, and to seek to simplify the proof, and to ke the acquisition more easy, of that and of er theorems, respecting the quaternions.

* For instance, in my published Lectures on Quaternis. Dublin: Hodges & Smith, 1853.

XVII. Meanwhile you may wish that, while waiving *here* the question of *proof*, I should at least lay before you some of the many *enunciations* of the geometrical principle to which I have been lately alluding; and which are all (according to me) *concisely contained* in what, at first sight, must appear to be the *trite* (though true) formula of *association*, which has so often already been before us; namely, the equation,

$$s\,.\,r\,q = s\,r\,.\,q.$$

With a view, then, to a first enunciation, I remark that since the *positions* of the *six planes*, as distinguished from their *directions*, may in every form of the theorem be chosen at pleasure, it is perhaps the most *obvious* arrangement of all to assume them as *concurring* in one common point, or origin, O; and as intersecting a *sphere*, described round that point as its centre, in a system of *six great circles*. Accordingly, in my printed Lectures on Quaternions, I have drawn a diagram to illustrate this conception, which is there numbered as figure 58, but which, to save you the trouble of a reference, I shall here

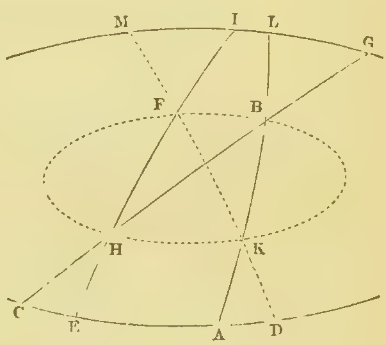


Fig 4.

transcribe. In this figure, the arcs A B and K L are supposed to be equal to each other, and are designed to be each a *representation* of the first given *versor*, q; the arcs B C and G H are intended to represent, in like manner, the second given versor, r; E F and H I represent each the third of the given versors, namely, s; A C and D E represent the first partial product, $r\,q$; G I and L M, the second partial or binary product, $s\,r$; finally, the arc D F is a representation of the *ternary* product, $s\,.\,r\,q$, with the order of *association* marked by the *point;* and the arc K M represents, on the same general plan, the *other* ternary product, $s\,r\,.\,q$, which is at least different in *symbolical form* from the previously mentioned product $s\,.\,r\,q$, as belonging to a *different order* of association; and which is, in fact, *constructed*, in the figure, by a *different arc* of a great circle. But that, with the foregoing *conditions of construction*, namely, with the five double coarcualities, and five equalities of arcs, expressed by the five formulæ,

$$\left.\begin{array}{r}\frown AB = \frown KL, \frown BC = \frown GH, \frown EF = \frown HI, \\ \frown AC = \frown DE, \frown GI = \frown LM,\end{array}\right\} (37)$$

we shall have *also*, as their *geometrical consequence*, the *sixth double coarcuality*, and *sixth equality of arcs* of great circles, expressed by this sixth formula,

$$\frown \text{K M} = \frown \text{D F}, \tag{38}$$

is a result which follows here from the *associative principle of multiplication of quaternions*, and is conversely a sufficient *expression* of that principle.

XVIII. Although I understand you to wish rather for *statements*, and for *illustrations*, of *principles*, than for *proofs* of *theorems*, in this little series of Letters, yet, that I may not have the air of pretending that there is any *difficulty* in *proving* the recently asserted theorem, you will perhaps allow me to sketch, in a few words, a *demonstration* which will appear extremely *easy*, to any one who is at all conversant with the modern doctrine of the *spherical conics*. Conceive that such a conic is described, as indicated by the dotted ellipse in the figure just now referred to, so as to pass through the three points, B, H, F, and to have the great circle D A E C for *one* of its two *cyclic arcs*. The second and third of the five equations (37) will then show that the great circle G L I M is the *second cyclic arc* of that conic; the first equation (37) proves next that the *same conic* passes through the point K; and if the spherical chord K F be drawn, and prolonged, so as to meet the above mentioned cyclic arcs, the fourth and fifth of the same system of equations (37), containing the conditions of the construction, suffice to prove that the transversal so drawn will meet those arcs precisely in the two points D and M: after which, the equation (38) is an immediate consequence of the same doctrine of spherical conics.—It would not in the least degree surprise me, if the *geometrical theorem*, involved in that equation (38), should be found to have been in some such way anticipated, though I have as yet no reason to suppose that it has been so, by some contemporary, and there are many such, far more familiar than myself with the geometry of the sphere. But what I request you to bear in mind, as the *only thing* which I lay any *stress* upon, in relation to the existing subject, is the *extreme simplicity of the expression* of the theorem, and its *early*, and indeed *fundamental* character, as an *element* in the *calculus* of quaternions, under our often cited form, (23). Did Lord Burleigh *mean all that*, by shaking his head? is asked by the critic, in the play:—He *did* mean it *all*, replies the author, *if* he shook his head as I desired him. And (playful allusions apart) I must own that the *severe compression* of meaning, the *closely packed* expression of thought, which enters thus into the *very first symbolical statements* of the theory to which these letters relate, appears to me interesting already, and full of hope for the future.

XIX. I do not know whether you have ever yielded to the fascinations of that high and beautiful part of modern geometry, which deals with the *infinites* and with the *imaginaries* of space, by so consistent and successful a method, as that which has been used by Poncelet and by Chasles, and by other recent discoverers, in France, in Germany, in Italy, and in these islands (and probably elsewhere): the happy daring of whose researches is closely allied to poetry. For even in those lofty and difficult regions of abstract science, to which I now allude, the presence of poetry is still felt: and with the alteration of a word, (which, I admit, destroys the metre,) we may still quote from Horace,

> [Poësin] expellas furcâ, tamen usque recurret,
> Et mala perrumpet furtim fastidia victrix.

As an example of the use which may be made, of such imaginative yet scientific contemplations, in connection with our present subject, it has lately occurred to me that the theorem (38), already stated in this Letter, as an interpretation of the associative formula (23), may be *extended*, or at least may be *transformed*, by the introduction of that wholly *ideal circle*, in which some modern geometers have conceived a *finite sphere* to be cut, by a *plane* which is *at an infinite distance*: the boldness of *this* geometrical *conception* being no fault, nor merit, of my own. For each *commediality* of *two real arcs*, of any one great circle upon a sphere, as, for instance, the arcs A L and K B, in the recently cited figure, 4, I am in this way led to substitute an *involution*, between those two arcs, and that third but *imaginary segment* of their common great circle, which is *conceived* to be *intercepted*, as a spherical chord, *within the ideal circle* just now spoken of. And thus, instead of a system of *six commedialities*, (37) (38), I am conducted to a *system of six involutions, whereof any three include the three others*, and which must *still* subsist, even when we *pass back* from the imaginary to the *real*, on a plan well known in modern geometry. *Dismissing*, therefore, now and here, the *ideal circle at infinity*, I am led to the following theorem, more complex in appearance than that of our recent paragraph XVII., but for which I see a sufficiently easy geometrical proof, (although I shall not trouble you with it,) derived from the known properties of involution and of conics, and especially from the celebrated theorem of Desargues, transferred from the plane to the sphere:—"If the four successive sides, A B, B C, C D, D A, of any spherical quadrilateral A B C D, be cut, respectively, in the four points A′, B′, C′, D′, by any one transversal arc E F, and in the four other points B″, C″, D″, A″, by any other transversal arc G H, where E F and G H are two spherical chords of any one given spherical conic (Σ), which conic cuts the same four sides of the quadrilateral in the four pairs of points, A‴ B$^{\text{iv}}$, B‴ C$^{\text{iv}}$, C‴ D$^{\text{iv}}$, D‴ A$^{\text{iv}}$; then, *of the six following involutions*, (each connecting three pairs of points on some one great circle, out of the 24 points of the construction,) namely,

$(A, B;\ A', B'';\ A''', B^{iv}),\ (B, C;\ B', C'';\ B''', C^{iv})$
$(A', C';\ B', D';\ E, F),$ (39)

d·

$C, D;\ C,' D'';\ C''' D^{iv}),\ (D, A;\ D', A'';\ D''', A^{iv}),$
$(A'', C'';\ B'', D'';\ G, H),$ (40)

y three include the three others." Of course, · may suppose, as a particular *case*, that the ıerical conic is composed of a *pair of great cles;* or, as another *limit*, that the given conic) becomes a *plane* one, and that the great cles, A B, &c., degenerate into *straight lines.* It ms unnecessary to take up your time, or my n, by drawing here any *diagram*, to illustrate foregoing enunciation: but, as a hint which y assist you in making out a *proof* of the theo-ı for yourself, I may just notice here, that under supposed conditions of construction, the *eight nts*, A, B, C, D, E, F, G, H, *are ranged upon one nmon conic* (Σ'), which is, however, generally *'inct* from the given conic (Σ). Another conic,), passes through the eight points, $A'' A''' B^{iv} B'$ $C''' D^{iv} D'$; and a fourth conic, (Σ'''), through se eight other points, $A^{iv} A' B'' B''' C^{iv} C'$ D'''. These results may easily have been an-pated, but I have not happened to meet with m. At all events, you will please to remem- that I mention them here, merely as things ich have been *suggested* to me by the consi-ation of the geometrical *meanings* of the very ple formula,

$$s\,.\,rq = sr\,.\,q, \qquad (23)$$

en *interpreted* on the plan of the quaternions.

XX. It was observed, in paragraph XVII. of letter, that the most *obvious* and *easy* ar-gement of the SIX PLANES, which enter *essen-ly* into the study of the geometrical meaning nterpretation, of that simple associative for-

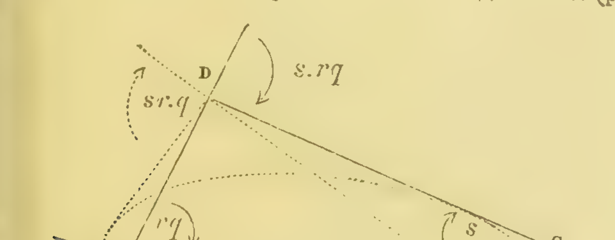


Fig. 5.

ı (23), appears to be *that* arrangement, al-y described, in which the six planes *concurred e point* O, assumed as the *centre* of a *sphere.* there is *another arrangement*, almost as ral, which in modern geometry would be the *reciprocal* of the former; and which, in my published Lectures, is illustrated by a figure, whereof I herewith enclose to you a copy (fig. 5): the *six planes*, in *this* construction, being *tangents* to one common sphere, *at six points*, A, B, C, D, E, F, which points are connected by *twelve arcs* of great circles,

$$AB,\ BC,\ CD,\ DA;\ EA,\ EB,\ EC,\ ED;$$
$$FA,\ FB,\ FC,\ FD; \qquad (41)$$

for the three arcs A C, B D, E F, are not required for our present purpose, any more than were the intersections of the three pairs of arcs, A L, E I; A E, L I; and D M, C G, in figure 4. Supposing, to fix our conceptions, that the two points E, F are situated in the *interior* of the spherical quadrilateral A B C D, the associative theorem of multiplication of quaternions may (as in my Lectures) be stated thus:—"*Of the six following equations between spherical angles,*

$$ABE = FBC,\quad BCF = ECD,\quad BEA + DEC = \pi, \qquad (42)$$
$$DAF = EAB,\quad FDA = CDE,\quad AFD + CFB = \pi, \qquad (43)$$

the three first (or indeed *any three*) *include the three others.*" In fact, it is found that the points E and F are the *two foci of a spherical ellipse, inscribed in the spherical quadrilateral* A B C D; a relation which I have proposed to denote concisely by the formula,

$$EF\ (\,..\,)\ ABCD; \qquad (44)$$

and it may be remarked that we are at the same time allowed to write on the same plan these two other analogous formulæ,

$$AC\ (\,..\,)\ BEDF, \text{ and } BD\ (\,..\,)\ AECF; \qquad (45)$$

because the points A and C are *foci of a spherical hyperbola*, which is touched by the prolongations of the four sides of the quadrilateral B E D F; and B, D are foci of *another hyperbola*, touched by the four sides (prolonged) of the third spherical quadrilateral A E C F. In preparing for the foregoing enunciation of the associative principle of multiplication, I use the following auxiliary theorem, or rule of construction, which can be deduced from what was stated in my former letter; namely, that *if any two versors*, q and r, *be represented by the two base angles*, A and B, *of a spherical triangle*, then *their product*, $r\,q$, *will be* (on the same plan) *represented by the external angle at the vertex*, C: a certain *order of rotation* being attended to.

XXI. You conceive, of course, (although it has only occurred to me while writing this letter,) that this last geometrical statement of the theorem of associative multiplication, like the statement previously made, must admit of being extended, or transformed, by the introduction of the ideal circle at infinity, which was mentioned in paragraph XIX. By substituting again, for that *ideal*

circle, an arbitrary conic (Σ), and by drawing *to it twelve tangent arcs*, from the six points A, B, C, D, E, F; namely, AA′ and AA″ from the point A; B B′ and B B″ from B, &c.; the theorem may be stated as follows:—"*If any three of the six spherical pencils*,

$$\left.\begin{array}{l}(\mathrm{AB},\ \mathrm{AD};\ \mathrm{AE},\ \mathrm{AF};\ \mathrm{AA}',\ \mathrm{AA}''),\\ (\mathrm{BC},\ \mathrm{BA};\ \mathrm{BE},\ \mathrm{BF};\ \mathrm{BB}',\ \mathrm{BB}''),\\ (\mathrm{CD},\ \mathrm{CB};\ \mathrm{CE},\ \mathrm{CF};\ \mathrm{CC}',\ \mathrm{CC}''),\\ (\mathrm{DA},\ \mathrm{DC};\ \mathrm{DE},\ \mathrm{DF};\ \mathrm{DD}',\ \mathrm{DD}''),\\ (\mathrm{EA},\ \mathrm{EC};\ \mathrm{EB},\ \mathrm{ED};\ \mathrm{EE}',\ \mathrm{EE}''),\\ (\mathrm{FA},\ \mathrm{FC};\ \mathrm{FB},\ \mathrm{FD};\ \mathrm{FF}',\ \mathrm{FF}''),\end{array}\right\} \quad (46)$$

be pencils in involution, the three other pencils will be in involution likewise." In *this* construction, besides the conic (Σ), which is touched by the *twelve* arcs A A′,... F F″, there are *three other conics*, (Σ'), (Σ''), (Σ'''), of which each is *touched by eight arcs*, namely, by four of the last mentioned twelve, and by four others in the figure, which I leave it to yourself to draw: for instance, the conic (Σ') is touched by these eight arcs,

$$\mathrm{AB},\ \mathrm{BC},\ \mathrm{CD},\ \mathrm{DA},\ \mathrm{EE}',\ \mathrm{EE}'',\ \mathrm{FF}',\ \mathrm{FF}''. \quad (47)$$

All this you will easily verify, if you are sufficiently familiar with spherical geometry. In the lately cited figure, 5, the dotted ellipse may represent the conic (Σ'); but the tangents to it from E and F are imaginary.

XXII. There is, however, a *third mode* of *arranging the six planes* of the six quaternions, q, r, s, rq, sr, srq, which, though it has only lately occurred to me, appears to possess some important advantages in point of simplicity, over both of the two former arrangements, and to be more perfectly adapted to become a base for elementary instruction, in some future work on the whole subject to which these letters relate. This third arrangement consists in treating the six planes of the quaternion factors and their products, as the *six faces of a hexaedron inscribed in a sphere;* and therefore as cutting, generally, the spheric surface, in a *system of six small circles.* In this manner, the *angle* of each quaternion comes to be constructed, not by the angle between *two radii*, nor by that between *two tangents*, but by the angle between two (rectilinear) *chords* of the sphere; and therefore by an *angle in a segment* (and in the plane) of one of the small circles, which of course may be replaced by any *other* angle in the *same* segment, or by the *supplement* of an angle in the *opposite* (or alternate) segment; the essential condition of having *two representations* for *each* of the *six quaternions* being thus very simply fulfilled:—while the *four multiplications* (36) are effected with great ease, at four of the eight *corners of the inscribed solid*, which represents here the *four triedral angles* of paragraph XVI. On considering the question from this point of view, we are led to regard the truth of the associative formula (23), when interpreted in connection with quaternions, as depending on this theorem of geometry, which can be understood and proved *without any reference to cones or to conics:*—"*If* A B′ C A′ B C′ *be any* (plane or gauche) *hexagon, such that the three circles*, C′ A B′, B′ C A′, A′ B C′, *concur in one common point* D, (as the second intersection of each pair), *then the three other circles*, A B′ C, C A′ B, B C′ A, *concur in another common point* D′; *and of the six quadrilaterals*,

$$\left.\begin{array}{l}\mathrm{C'AB'D},\ \mathrm{B'CA'D},\ \mathrm{A'BC'D},\\ \mathrm{AB'CD'},\ \mathrm{CA'BD'},\ \mathrm{BC'AD'},\end{array}\right\} \quad (48)$$

which are thus inscribed in circles, an even number (if any) *will be crossed, and another even number* (if any) *will be uncrossed quadrilaterals.*"—It is easy to see that if the hexagon be not a *plane* one, it must be inscriptible in a *sphere;* and then the principles of *stereographic projection* render it sufficient for us to *prove the theorem for the plane*, in order to *infer* that it is *true for space:* while, if we take for the *pole* of the projection the first point of *concurrence* D, the small circles which meet at the point will be projected into *right lines.* Interchanging, then, the accented and unaccented letters, we ultimately arrive at this *very elementary form* of the theorem, to the extreme simplicity of which it seems scarcely possible to hope, or even to *wish*, for any addition:—"If A′, B′, C′ *be any points on the three sides*, B C, C A, A B *of any plane triangle*

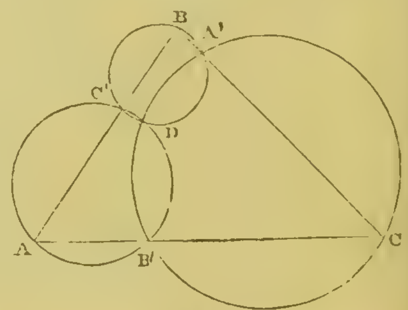


Fig 6.

A B C, or on those sides prolonged, *the three circles*, A B′ C′, B C′ A′, C A′ B′, *meet in one common point* D; *and of the six successions*, rectilinear and circular,

$$\left.\begin{array}{c}\mathrm{AB'C},\ \mathrm{BC'A},\ \mathrm{CA'B},\ \mathrm{A'BC'D},\\ \mathrm{B'CA'D},\ \mathrm{C'AB'D},\end{array}\right\} \quad (49)$$

an even number, if any, *will be direct, and another even number*, if any, *will be indirect.*" It is, however, to be remembered, that in *returning* from this very simple proposition of *plane geometry* to the formula (23) for *quaternions*, we are obliged to use some of the properties of the *sphere*, and some theorems of stereographic *projection*. The necessity for proving the constant existence of an *even* number of *direct* successions depends on the circumstance, that we are obliged to prove that, in the calculus of quaternions, we have *always* $s\,.\,rq = +\,sr\,.\,q$, and *never* $s\,.\,rq = -\,sr\,.\,q$.

XXIII. It would be unreasonable to add to the length of this letter, by entering upon the consideration of any of those *other* formulæ,

:pressing principles not less important in this leulus, such as the equation

$$s + (r + q) = (s + r) + q, \qquad (50)$$

hich contains the *associative principle of addition of quaternions;* or on either of the two uations,

$$r+q) = s\,r + s\,q, \; (r+q)\,s = r\,s + q\,s, \qquad (51)$$

hich jointly contain the *distributive property of ultiplication*, and which must at first sight pear to be mere *truisms*, to a person familiar th ordinary algebra, or even to a beginner rein. You are now fully prepared to conceive it, when *interpreted* on the principles of my mer letter, they *require to be proved anew;* d that, *when so proved*, they rise at once to the ik of *theorems in solid geometry*. It is worth icing, however, as we pass, that the *associa-* principle (50) of *addition* of quaternions, al-ugh it looks so simple, is really a *more* com-x one in its *interpretation*, not only than the responding principle (23) of *multiplication*, even than the *distributive* principle under ier of its two forms (51), which is itself not te so simple as (23). The ultimate *reason* of s fact appears to be, that the geometrical *ception* of a *quaternion* being with us referred what we have regarded as the *division* of one ected line by another, *multiplication* has na-ally a closer and *more intimate* connection h the *quotients* thus obtained, than *addition* with them. But in a slightly more technical r, on the plan of paragraph (15), it may be iarked that while *tensors disappear entirely* n the equation (23), (except in the old and ectly well known sense, of *composition of os*), there enters *one arbitrary ratio of tensors* : Tq), into each of the equations (51); and *such ratios* (Ts : Tr : Tq) enter into the equa-(50). Accordingly, on *fully* constructing, spherical geometry, what is *meant* (in my ulus) by that last cited equation, I am con-ted to what may seem to the eye a compli-d, yet not inelegant *figure*, of which I spare the examination. You conceive that *invo-on* may scarcely suffice for the *interpretation* he geometrical relations *here* included; and , at least when combining them with the ception of the ideal *circle at infinity*, I find elf almost compelled to employ Chasles' ry of *double homographic division*, upon a e; or of *homographic pencils*, having a mon vertex and a common plane. In the *plest construction* which has occurred to me, hus representing the geometrical meaning of equation (50), I have been obliged to con- a *system of ten such planes;* namely, those ie *ten quaternions*,

$$; s ; r + q ; s + r ; s + r + q ; \qquad (52)$$

$$q ; s : r ; (r + q) : s ; (s + r) : q ; \qquad (53)$$

re the colon is used as a mark of division.

But when I wish to *incorporate*, with the *associative* property of *addition*, the *commutative* property thereof, or the equation

$$q + r = r + q, \qquad (54)$$

which is *true* for *quaternions*, as well as for ordinary algebra, *new planes* require to be considered, which I will not now delay to enumerate. You guess, no doubt, that because *addition of lines* is, according to a whole host of moderns, whom in this respect I merely follow, performed by the *rule of the parallelogram*, (compare paragraph VII.), there must enter *ratios of sines* into the interpretations of such results as these; and that thus I have been led to reproduce many *old equations*, between *products of sines of segments* of the arcs employed in the constructions; and *perhaps*, (though I do not venture to assert it), to find out some *new* equations of that class; because *some* of my diagrams involve simultaneously a *greater number of points* and of arcs, than it seems to have been usual to consider in any *one* construction upon the surface of a sphere.

XXIV. It has been my desire to avoid, as far as it was possible, all *technicalities of calculation*, in this series of letters, now drawing to its close. But this appears to be a proper place for mentioning *another fundamental connection* between *quaternions and trigonometry*, which arises out of the very *conception of addition* in the present calculus. You remember the two formulæ (11), which I have adopted from algebra, or from arithmetic, as being sufficient to fix the *rules* of the *addition* and *subtraction* of *any two quaternions*, when the addition and subtraction of two directed *lines* are performed in the modern way, just now alluded to. Employing, therefore, the fundamental formula of addition of fractions or of quotients,

$$\frac{\gamma}{\alpha} + \frac{\beta}{\alpha} = \frac{\gamma + \beta}{\alpha}, \qquad (17)$$

let

$$\gamma + \beta = \delta, \quad \beta \parallel \alpha, \quad \gamma \perp \alpha; \qquad (55)$$

the mark of *parallelism* $\parallel$ being so interpreted as not to exclude coincidence of *position*, on one common rectilinear axis. Then, the *general quaternion*, q or $\delta : \alpha$, will be broken up or decomposed into *two parts*, of which it is the *sum;* and of which *one*, ($\beta : \alpha$), being the quotient of two *similarly* or *oppositely directed lines*, is simply a *positive or negative number;* while the *other part*, ($\gamma : \alpha$), as being the quotient of two mutually *rectangular lines*, may be said to be a *right angled quaternion*, or more concisely a RIGHT QUOTIENT. I denote these two parts (or components) of any given quaternion q, by the symbols, S q, and V q; so as to write, generally,

$$\mathrm{V}\,q + \mathrm{S}\,q = q, \text{ or, } q = \mathrm{S}\,q + \mathrm{V}\,q; \qquad (56)$$

and am accustomed to say that the merely *numerical*, or rather *algebraical* part, S q, is the

scalar part, or more briefly, that it is the SCALAR of the given quaternion q; and that the *other part*, $\mathrm{V}\,q$, which has just now been called a *right quotient*, is the *vector part*, or simply THE VECTOR of the same quaternion, q. For I have long been led, by several concurring considerations, to regard every such *right quotient* ($\gamma : \alpha$, where $\gamma \perp \alpha$), as being one which may be properly and usefully *constructed*, or geometrically *represented*, by a *right line perpendicular to its plane;* the *direction of this line*, or *vector*, being distinguished from its own *opposite*, by the condition that the *quadrantal rotation*, performed *round it as an axis*, *from* the *divisor line* (α), *to* the *dividend line* (γ), or from the denominator to the numerator of the geometrical fraction $\left(\dfrac{\gamma}{\alpha}\right)$, shall be *always right-handed*, or always left-handed, according as the *right-handed* or the left-handed *direction* of rotation shall have been previously *assumed* to be the *positive* one: while the *length* of the same VECTOR-AXIS shall bear to an assumed *unit of length* that *ratio*, whatever it may be, which the *length* of the *dividend* or numerator line (γ) bears to the *length* of the *divisor* or denominator line (α). The *general quaternion*, q, (as it early occurred to me,) might on this account be said to be a GRAMMARITHM (from the words, γραμμή and ἀριθμός); because it is thus regarded as being the symbolical *sum of two parts*, of kinds quite *distinct* from each other; whereof *one*, namely, the *vector part* ($\mathrm{V}\,q$), is *constructed by a line* (γραμμή), in space of three dimensions, and is therefore *geometrical*, or GRAMMIC (if we may venture to coin such an adjective, from the Greek word just now mentioned); while the *other*, namely, the *scalar part* ($\mathrm{S}\,q$), is expressed by a *positive or negative number* (ἀριθμός), and may therefore be said to be ARITHMIC, or *algebraical*. And the *essential connection* of my *geometrical quaternion* with the *number* FOUR, (which is required for the *propriety* of the *name*, QUATERNION), is here made manifest in a new way, or at least in one which has not hitherto been noticed in these Letters. For we see that while the *arithmic* (or *scalar*) *part*, of the *general geometrical quotient* of two directed lines, admits only of what may be called an *unidimensional variety*, analogous to that which is exemplified in the progression of TIME;* the *grammic* (or *vector*) part, of the same general quotient (or fraction) involves, on the contrary, all that *tridimensional variety* which is included in the notion of SPACE: so that, *by this analysis also*, of the conception which is the subject of these Letters, we are *still* conducted, as before, to the consideration and employment of a SYSTEM OF FOUR ELEMENTS.

XXV. As regards the connection of quaternions with *trigonometry*, lately alluded to, it may suffice to remark, that because we may decompose any geometrical quotient of two coinitial lines, such as the quotient

$$\mathrm{OD} : \mathrm{OA} = q, \tag{57}$$

into its scalar and vector parts, namely,

$$\mathrm{OB} : \mathrm{OA} = \mathrm{S}\,q; \quad \mathrm{OC} : \mathrm{OA} = \mathrm{BD} : \mathrm{OA} = \mathrm{V}q, \tag{58}$$

by letting fall a perpendicular DB, from D, on the indefinite right line OA, and then completing the rectangle OBDC; the two partial quotients (58) must evidently, on account of the right-angled triangle OBD, be related to each other (at least, as one element in their mutual relations), as COSINE is related to SINE. Indeed, the *vector* character of the *right quotient*, $\mathrm{V}q$, gives to *it* the sort of *tridimensional variety*, already mentioned; so that, whereas the *cosine*, with which we have just now connected $\mathrm{S}\,q$, is simply *positive*, or *negative*, or *null*, according as the *angle* AOD, which we may here denote as

$$\mathrm{AOD} = \angle\, q, \tag{59}$$

is *acute*, or *obtuse*, or *right;* the *Sine*, with which we have, on the same plan, compared $\mathrm{V}\,q$, must, with reference to *this* comparison, be thought of, as having a relation to a *variable plane*, admitting of a *variable aspect*. But, if we are content to attend only to the mere *numerical* and *positive quantity* of the *Sine*, which suffices for the wants of ordinary trigonometry, so far as it relates to the solution of *triangles*, as well spherical as plane, we may retain the *usual* meaning of the *Sine* of an angle, such as $\angle\, q$. And then we may establish the two following general formulæ, which I have found to be frequently useful:

$$\mathrm{S\,U}\,q = \cos \angle q; \quad \mathrm{T\,V\,U}\,q = \sin \angle q. \tag{60}$$

It is also very often convenient to employ this other notation,

$$\mathrm{K}\,q = \mathrm{S}\,q - \mathrm{V}\,q \tag{61}$$

where $\mathrm{K}\,q$ may be said to denote the quaternion *conjugate* to q: the *geometrical character* of such *conjugation* of two quaternions consisting in this, that when any three diverging lines, OA, OB, OC, furnish two *conjugate quotients*, such as

$$\frac{\mathrm{OB}}{\mathrm{OA}} = q, \quad \frac{\mathrm{OC}}{\mathrm{OA}} = \mathrm{K}\,q, \tag{62}$$

then the line OA *bisects the angle* BOC, between the two other lines. You conceive that this notation must be useful in many researches of pure and applied mathematics; for instance, in some *optical* investigations, when conducted by the method of quaternions; since the two quotients,

"incident ray, divided by normal to mirror," (63)

and

"reflected ray, divided by the same normal," (64)

are, in this view, quaternions *conjugate* to each other; at least, if the incident and reflected rays be treated as two lines equally long. And you will not have unduly flattered the METHOD, to

* "And how the One of Time, of Space the Three,
Might in the Chain of Symbol girdled be."
[From the "Tetractys"—an unpublished sonnet, addressed to Sir John F. W. Herschel, in 1846.]

which (or at least to the *conception* of which) the present Letters relate, if you shall give it credit for admitting of being usefully applied, to *physical* as well as to *mathematical* science, in a great variety of other ways. But before entering on even the slightest *sketch* of any such *applications* of the present CALCULUS, a word or two must be said respecting its *only other peculiar notations*, in addition to the *five* symbols, or *characteristics of operation* on a quaternion, namely,

$$\mathrm{T},\ \mathrm{U},\ \mathrm{S},\ \mathrm{V},\ \mathrm{K}, \qquad (65)$$

which have been thus recently employed.

XXVI. These *other peculiar symbols*, or (at least) symbols used in a *peculiar sense*, are merely the *three letters*,

$$i,\ j,\ k; \qquad (66)$$

treated, however, as being subject to a few *special laws*, which it shall now be my endeavour briefly to explain. Nearly *all*, indeed, that is necessary, will have been said, when it is remarked, that the $i\ j\ k$ of this calculus are considered by me as representing *three right quotients in three rectangular planes, with unity for their common tensor;* (compare paragraphs XV. and XXIV.) in such a manner that we may write,

$$\mathrm{T}\,i = \mathrm{T}\,j = \mathrm{T}\,k = 1; \qquad (67)$$

$$i \perp j;\ j \perp k;\ k \perp i. \qquad (68)$$

You see that the introduction of such a system of symbols is very closely connected with the notion of TRIDIMENSIONAL SPACE, to which the *quaternions throughout refer:* for although I *can* apply my calculus to *plane problems*, as a *limit*, yet I frankly admit that it would not have been *worth while* to *invent*, or even to *learn*, this new system or method of calculation, if no problems *beyond the plane* were to be treated; and therefore I regard it as *not quite fair* to be asked (as I sometimes am), to exemplify the method by the treatment of *plane conics:*—to be *tested*, and indeed to be rightly *understood*, it must be seen in its applications to *higher questions*, such as those which relate to *spherical conics* (already alluded to in this Letter), or to *surfaces of the second order;* or to some other mathematical or physical subject, in which the *three dimensions of space* enter essentially. One does not use (except as a mere exercise) the method of fluxions to estimate the area of a plane triangle; nor the calculus of variations to establish that a right line is the shortest between two points. But since I have been led to *allude* to such branches of calculation as those just mentioned, you will permit me to remark, in passing, that I have found them all to admit of being usefully *combined* with the method of quaternions; in which, accordingly, I am compelled to admit differences and differentials, variations and integrals, though modified by the peculiar conditions of the subject, or by the laws of the symbols $i\ j\ k$. Indeed, in passing from one form to another, of the *general equation in quaternions* for *surfaces of revolution*, and in a few other applications of that sort, something like a *new calculus of partial differential equations*, and of the *integral equations* corresponding, has forced itself upon my notice; some hints of which you may see, if you think it worth while, in my printed Volume of Lectures. As one of the things which you will *not* see there, because it has too recently occurred to me, I may mention that at the request of my friend, John T. Graves, Esq., of Cheltenham (but formerly of Dublin), to whom my first results respecting quaternions were communicated in 1843, I lately solved the problem of determining the *greatest common measure* of *any two integer quaternions*, such as

$$1 + 2i + 3j + 4k, \text{ and } 5 + 6i + 7j + 8k, \qquad (69)$$

which happened to be the two that he selected. I found this greatest common measure to be, in this example, $i - k$; multiplied, it is to be understood, by ± 1, or by $\pm i$, or by $\pm j$, or by $\pm k$. But as regards the *laws* of such *multiplications*, of the symbols $i\ j\ k$, it is necessary to say a few words.

XXVII. Conceive, then, that OA, OB, OC, or α, β, γ, are any *three rectangular unit lines*, and that the *quadrantal rotation* round γ, from α to β, is *positive*. To fix our thoughts, we may suppose that such *positive rotation* is *right-handed*, like the motion of the hands of a watch; and may conceive that α and β are *two horizontal foot-lines*, directed respectively to the *south* and *west*, while γ is a *vertical* foot-line, directed *upward:* so that if we make

$$\alpha' = -\alpha.\ \beta' = -\beta,\ \gamma' = -\gamma, \qquad (70)$$

the three new lines α', β', γ', (which are thus *opposite* to α, β, γ,) shall be respectively, a *northward*, an *eastward*, and a *downward foot*. We may then suppose, in consistency with the conditions (67), (68), that the three right quotients $i\ j\ k$ admit of being expressed as follows:

$$\left.\begin{aligned} i &= \gamma : \beta = \beta : \gamma' = \gamma' : \beta' = \beta' : \gamma; \\ j &= \alpha : \gamma = \gamma : \alpha' = \alpha' : \gamma' = \gamma' : \alpha; \\ k &= \beta : \alpha = \alpha : \beta' = \beta' : \alpha' = \alpha' : \beta; \end{aligned}\right\} \qquad (71)$$

and thus shall have, by the formula of multiplication (18), or (35), the nine following values for the *squares* and *binary products* of these three symbols i, j, k:

$$\left.\begin{aligned} i^2 &= (\gamma : \beta) . (\beta : \gamma') = \gamma : \gamma' = -1; \\ j^2 &= (\alpha : \gamma) . (\gamma : \alpha') = \alpha : \alpha' = -1; \\ k^2 &= (\beta : \alpha) . (\alpha : \beta') = \beta : \beta' = -1; \end{aligned}\right\} \qquad (72)$$

$$\left.\begin{aligned} ij &= (\beta : \gamma') . (\gamma' : \alpha) = \beta : \alpha = k; \\ jk &= (\gamma : \alpha') . (\alpha' : \beta) = \gamma : \beta = i; \\ ki &= (\alpha : \beta') . (\beta' : \gamma) = \alpha : \gamma = j; \end{aligned}\right\} \qquad (73)$$

$$\left.\begin{aligned} ji &= (\alpha : \gamma) . (\gamma : \beta) = \alpha : \beta = -k; \\ kj &= (\beta : \alpha) . (\alpha : \gamma) = \beta : \gamma = -i; \\ ik &= (\gamma : \beta) . (\beta : \alpha) = \gamma : \alpha = -j; \end{aligned}\right\} \qquad (74)$$

together with the six following *ternary products*,

$$\left.\begin{array}{l} i\,j\,k = j\,k\,i = k\,i\,j = -1; \\ k\,j\,i = i\,k\,j = j\,i\,k = +1. \end{array}\right\} \qquad (75)$$

And they may *all* be briefly expressed by this ONE FUNDAMENTAL FORMULA,

$$i^2 = j^2 = k^2 = i\,j\,k = -1; \qquad (\text{A})$$

which would in rigour be a *sufficient base* for the erection of the *calculus of quaternions*, at least when combined with the *associative* and *commutative* properties of *addition*, and with the *associative* and *distributive* properties of *multiplication*. In fact, we can *recover* all the six values (73) and (74), for the *binary products* of $i\,j\,k$, from this one formula (A); for it gives, immediately, $i\,j = k$, $j\,k = i$, as in (73); then $j\,i = -k$, $k\,j = -i$, $i\,k = -j$, as in (74); and, finally, $k\,i = j$, as in the remaining expression (73). And it admits of being proved that *every geometrical quaternion* (of the kind considered in these Letters) is *reducible to the* QUADRINOMIAL FORM:

$$q = w + i\,x + j\,y + k\,z; \qquad (\text{B})$$

where w denotes the *scalar part*, and $i\,x + j\,y + k\,z$ denotes the *vector part*, of the quaternion, q. (compare par. XXIV). So that we may write, *generally*,

$$\text{S}\,q = w, \ \text{V}\,q = i\,x + j\,y + k\,z; \qquad (\text{C})$$

where the *four* letters $w\,x\,y\,z$ are supposed to signify some four *positive or negative numbers*, which may be called the *four constituents* (or *co-ordinates*) of the quaternion q, and are *subject as such to all the rules of common algebra:* while the three symbols $i\,j\,k$ are subject to *peculiar rules, contained in the formula* (A).

XXVIII. Among these *peculiar* rules, it is natural to be struck first by the occurrence of *three distinct* square *roots of negative unity*, namely, the three symbols $i\,j\,k$, whose *negatives also* possess the same general property, since we have

$$(-i)^2 = (-j)^2 = (-k)^2 = -1. \qquad (76)$$

The symbol $\sqrt{-1}$ occurs very often in *algebraical calculations*, but always as denoting what is called an *impossible*, or an *imaginary quantity:* for example, in the solution of an ordinary quadratic equation, which has *no real roots*. And, hence, when I first asked leave, in 1843, to read a paper on the present subject to the Royal Irish Academy, I described it as "a paper *on a new species of imaginary quantities*, connected with a theory of quaternions." But when I came to see that these *new imaginaries*, as they were by analogy called at first, admitted of a perfectly clear and simple *interpretation in geometry*, the name appeared to me to become *inappropriate:* and I soon came to call the *trinomial* $i\,x + j\,y + k\,z$, into which those peculiar symbols entered, the *vector part* of the quaternion. Indeed, it had been distinctly pointed out by the Abbé Buée, in a remarkable paper already alluded to, which appeared in the *Philosophical Transactions of London* for the year 1806, that the symbol $\sqrt{-1}$ might be interpreted in geometry as a sign of *perpendicularity*; and it appears that Argand, in his pamphlet of the same year, published in Paris, arrived independently at the same result, in connection with his theory of *coplanar proportion* (compare par. IV). At a later date, in 1828, Mr. Warren of Cambridge published a work "On the Geometrical Interpretation of the Square Roots of Negative Quantities," in which nearly the same view was taken; although apparently as a result of his own independent speculations. This work of Warren had been read by me, before I first came to think of the quaternions, and before I happened to know anything of the earlier investigations of Buée and Argand; or of any of those other and analogous researches, which are briefly mentioned in the preface to my Lectures. Consequently the mere *notion of using the sign* $\sqrt{-1}$ *in geometry* is nothing in any way peculiar to myself. But the thought of using a *system of signs*, whereof *each* should be thus a square root of negative unity, had not (so far as I yet know) been anticipated. Indeed, with me there are not merely *six* values, such as

$$\pm i, \ \pm j, \ \pm k, \qquad (77)$$

but even *infinitely many values of the symbol* $\sqrt{-1}$ in geometry; for if you *square the trinomial* which represents the vector part of a quaternion q, attending to the *rules* included in the fundamental formula (A), you will find that this square reduces itself to the expression,

$$(i\,x + j\,y + k\,z)^2 = -(x^2 + y^2 + z^2) \ldots\ldots (\text{D})$$

which has very extensive applications; and which shows that it is sufficient to make $x\,y\,z$ the rectangular *co-ordinates of any point on the unit-sphere described about the origin as centre*, so as to have the equation

$$x^2 + y^2 + z^2 = 1; \qquad (78)$$

and that then the symbol $i\,x + j\,y + k\,z$ will denote *some one of the square roots of* -1; so that we shall have concisely the equation

$$\rho^2 = -1, \qquad (79)$$

if, under the condition (78), we make

$$\rho = i\,x + j\,y + k\,z. \qquad (80)$$

In fact, it is easy to see that on our principles, the *square of every right quotient* is equal to *some negative number;* and this for the very simple reason, that *two successive quadrantal rotations, in any plane, reverse the direction of the turning line*. But it may be observed in passing, to what a *simple* (*though new*) *form* (79) the *equation of the unit-sphere* (78) comes in my system, to reduce itself. Accordingly, I have found this *form* (79) to lend itself with ease to a great number of useful calculations, the results of which admit of *interpretations*, and furnish *theorems* in spherical geometry, which again can in general be varied, by the modern methods of *transformation of figures*, so as to assist in solving problems respecting *surfaces* of *the second order*.

XXIX. The *other* principal *peculiarity* of the rules of the symbols $i\ j\ k$ consists in their *non-commutative character*, as *factors*, in any *multiplication* into which they enter. And such accordingly was the chief seeming *difficulty*, or *paradox*, which, at the end of the last, and at the beginning of the present letter, I expressed a wish to explain before this correspondence should conclude. You see, in fact, on comparing the expressions (73), (74), that while $i\,j = k$, we have at the same time $j\,i = -k$; and consequently,

$$i\,j = -j\,i; \qquad (\text{E})$$

a result which must seem *much more strange*, at first, than anything about mere *roots of negative unity*. But if we attend to the *geometrical signification* of this equation (E), you will see that it expresses simply what is certainly *true in geometry*, that "*if any two quadrantal rotations, in two rectangular planes, be compounded with each other, first in one order of succession, and afterwards in the opposite order; then, although the two resultant rotations will each be quadrantal, and each performed in the plane perpendicular to the planes of the two components, yet either of these two resultants will have, in that common plane, a direction opposite to the other.*" And it is useful to have, for this theorem, although the *proof* of it involves no difficulty, so *simple* an *expression*, as the lately derived formula (E).

XXX. Some additional light may be thrown on this geometrical result, if, as suggested in par. XXIV., we *replace* the two given *right quotients*, or *right-angled versors*, i and j, by the *two vector-axes* corresponding. For in this view we need only consider the *two rotations*, of these two axes *about one another*: and then, whatever *paradox* there may at first seem to be, in the equation $j\,i = -i\,j$, it receives an easy explanation. Let there be two horizontal lines, from one origin, directed respectively towards the south and west, and lettered i and j; while a third line, drawn vertically upwards from the same origin, shall be called k: so that these lines $i\ j\ k$ are *now* the $\alpha\ \beta\ \gamma$ of par. XXVII. Then if, treating the southward line i as an *axis* of rotation, we cause (or conceive) the line j to revolve round it through a right angle, and *towards the right hand*, this revolving line will be brought into the position k, being thus *elevated* from a westward into an *upward* direction. But if, on the contrary, we take the line j, in its original or westward position, as the axis, and oblige the line i to revolve round it, but still through a right angle, *and still towards the right hand* as before, this new revolving line i will be *depressed* from the southward to the *downward* direction, or will be brought to the position denoted by $-k$. The *fundamental contrast* in the quaternion calculus, between the values of $i\,j$ and $j\,i$, reproduces itself therefore in geometrical constructions, through the conception of these *two right handed and right angled rotations*, one of j round i, and the other of i round j, where i and j are two right lines perpendicular to each other. We see, however, by this process as well as by the former, that the *commutative equation of multiplication* ($q\,r = r\,q$) is *not generally true in quaternions*; a result which may be illustrated in other ways, and which may be regarded as constituting the *chief distinction* in *calculation* between my method and those previously employed, though it is only just to mention that a species of *non-commutative multiplication* has been used by Grassmann in his very profound work, the Ausdehnungs-lehre, which appeared at Leipzig in 1844.

XXXI. Of such *applications* as those alluded to at the end of par. XXV., I have already published some specimens, while many others remain unpublished. For instance, in *pure* mathematics, several theorems have been deduced respecting cones and other surfaces of the second order, including two new *constructions* for an *osculating cone of revolution*, to any cone with circular base, along any side thereof; and several *new generations of the ellipsoid*, derived from the *quaternion equation* of that important solid, or of its surface, namely, the equation

$$\text{T}(\iota\,\varrho + \varrho\,\varkappa) = \varkappa^2 - \iota^2; \qquad (81)$$

where ι and $\varkappa$ are two *constant vectors* (perpendicular to the two cyclic planes), while ϱ is a *variable vector*, drawn from the centre of the ellipsoid to an arbitrary point of the surface: and the *products* $\iota\,\varrho$, $\varrho\,\varkappa$, are interpreted as two quaternions, formed by treating, in each, the two *vector-factors* as being the *vector-axes* of two *factor-quaternions*, in a way which was lately explained; the *squares*, ι^2, $\varkappa^2$, being also interpreted in an analogous way. It seems to have been reserved for this new method of geometrical investigation, to assign the complete solution of the problem "to *inscribe, in a given surface of the second order, a rectilinear* (but generally *gauche*) *polygon*, whose *n sides* shall pass in succession *through n given points*;" the corresponding problem for the *plane* having been very elegantly resolved by Poncelet. But, whereas for the inscription of such a polygon in a *plane conic*, the method of Poncelet (*Traité des Propriétés Projectives*) gives only *two* solutions (real or imaginary), whether the *number* of sides of the polygon be *odd* or *even*, the method of quaternions, when applied to the *ellipsoid*, gives *always four solutions, two real and two imaginary*, if the required polygon be *even-sided*; the *two chords of solution*, which the method employed by me furnishes, being always *reciprocal polars* of each other. For the problem of inscribing an *odd-sided polygon*, in *any* surface of the second order, I find, as in the plane, only *one chord of solution*: and for inscription of an *even*-sided polygon, in a *single-sheeted hyperboloid*, the four solutions become either *all real* or else *all imaginary*. These are merely *specimens* of the results that

have already followed, from the application of quaternions to *geometry*. I might have mentioned also some *extensions* of the theory of *involution*, from the plane to *space;* and a conception of geometrical *syngraphy*, which is *not identical* with Chasles' theory of *homography*, although I gladly admit that it was suggested thereby. The conceptions of what I call the ANHARMONIC QUATERNION *of four points*,

$$(\mathrm{A\,B : B\,C}) \,.\, (\mathrm{C\,D : D\,A}), \qquad (82)$$

and of the EVOLUTIONARY QUATERNION *of six points*,

$$(\mathrm{A\,B : B\,C}) \,.\, (\mathrm{C\,D : D\,E}) \,.\, (\mathrm{E\,F : F\,A}), \qquad (83)$$

where the points A B C D E F may be situated *anywhere in space*, have also led me into some interesting, but as yet unpublished investigations; in *some* of the *results* of which (though *not* in the *conceptions themselves*), I have had the satisfaction (for such it really has been to me) of finding that I have been lately anticipated by one whom I so much admire as I do Moebius (the author of the Barycentric Calculus).

XXXII. In *physics*, I may just mention that my method lends itself with surprising facility, to all the fundamental problems of *mechanics*, including those which belong to Poinsot's justly celebrated theory of *couples;* the *axis* of any such statical or dynamical couple being precisely the *vector-part* of a certain *quaternion-product of two vectors*, interpreted as *axes of right quotients*, on the plan already explained. That some new theorems respecting *rotations of bodies* should offer themselves, can appear to you in no degree surprising, from the important part which the *geometrical conception of rotation* plays, in this whole system. If I had known the present mathematical method, in 1832, the theoretical deduction of those two *laws of light*, which have been named external and internal *conical refraction*, and which have been experimentally verified by my friend, the Rev. Humphrey Lloyd, would have cost me less trouble than they did. The quaternions have been found to furnish also easy proofs of some of the optical results of the justly admired and regretted MacCullagh; for instance, those which relate to what he called the *polar plane*, in connection with crystalline reflection and refraction. A transformation, and in some sense a solution, of Laplace's well known and widely applicable *Equation*, in partial differential co-efficients,

$$(\mathrm{D}^2_x + \mathrm{D}^2_y + \mathrm{D}^2_z)\, v = 0, \qquad (84)$$

which bears on heat, and electricity, as well as on the attraction of spheroids, have arisen out of the same general method; especially when handled by my friends of the Dublin University, Carmichael, and Graves. In fact, the formula (D), of the present Letter, will easily enable any one who looks at it to see, what I communicated to the Royal Irish Academy in 1846, that if we introduce, as I then did, the *new symbol of operation*,

$$\lhd = i\,\mathrm{D}_x + j\,\mathrm{D}_y + k\,\mathrm{D}_z \qquad (\mathrm{F})$$

we shall have, generally,

$$\mathrm{D}^2_x + \mathrm{D}^2_y + \mathrm{D}^2_z = -\lhd^2; \qquad (\mathrm{G})$$

so that *Laplace's Equation* takes (as I showed) this very simple *form*,

$$\lhd^2 v = 0. \qquad (\mathrm{H})$$

Mr. Carmichael, (F.T.C.D.), at a later date, perceived this *other* transformation, depending on my *non-commutative law*, above marked (E), but which had escaped myself,

$$\mathrm{D}^2_x + \mathrm{D}^2_y + \mathrm{D}^2_z = (\mathrm{D}_x + i\,\mathrm{D}_y + j\,\mathrm{D}_z)\,\cdot\,(\mathrm{D}_x - i\,\mathrm{D}_y - j\,\mathrm{D}_z); \qquad (\mathrm{I})$$

and Professor Charles Graves has still more lately pointed out a method of estimating what he proposes to call the *Mean Values* of *Functions of Quaternions*, which method seems likely to assist much in rendering useful such *symbolical transformations* as these. It was through the quaternions, applied to the theory of elliptic or other *undisturbed* motion of a planet, that I was led, in 1846, to that conception of the *Circular Hodograph*, on which I have been informed that the already cited and eminent Moebius has done me the honour to lecture; and to the theorem of *Hodographic Isochronism*, (designed to replace Lambert's theorem), my unpublished demonstration of which has lately been thought worthy of being supplied (though by a different process) in England. Applying the same general method of quaternions, to the investigation of the *disturbing effect* of the sun upon the moon, or to that of Neptune upon Uranus, I showed, in 1847, at the second meeting of the British Association in Oxford, that the expression of this effect might be developed in a new way, as follows. The function

$$\phi\,\alpha = \alpha^{-1}\,\mathrm{T}\,\alpha^{-1}, \qquad (\mathrm{K})$$

being called the TRACTOR of α, because it represents, in this calculus, at once the direction and the quantity, when estimated according to Newton's law, of the *accelerating force of attraction*, "which an unit of mass, placed at the origin (or beginning) of the vector α, exerts on a point or body placed at the end of the same vector; the *difference* of two such co-initial tractors, namely, the new function,—

$$\Delta\,\phi\,\alpha = \phi\,(\alpha + \Delta\,\alpha) - \phi\,\alpha, \qquad (\mathrm{L})$$

may be said to be a TURBATOR in this theory; because it expresses, in amount and in direction, the force which an unit-mass, supposed to be situated at the common origin B, of the two vectors, α and $\alpha + \Delta\,\alpha$, or α and $\alpha + \beta$, exerts on a point or body A, situated at the end of the *latter* variable vector $(\alpha + \beta)$, *to disturb its relative motion* about a body C, situated at the end of the *former* vector (namely α); the letters α and β denoting here the lines B C and C A. For instance, in the *lunar theory*, the letters A, B, C, may denote respectively the Moon, the Sun, and the Earth; or in that part of the *planetary*

theory, to which allusion has been made, the same letters may be used as symbols for Uranus, Neptune, and the Sun; it being understood that, in every such application, the *turbator function*, $\Delta\,\phi\,\alpha$, is to be multiplied by the *disturbing mass* or by the number which represents it. *So far*, the method is, at least in its *conception*, quite *general*, and extends to *all perturbations* of the solar system; but a peculiar *method of development*, for the *case* when the vector $\Delta\,\alpha$ or β, is a *shorter line* than α, or has (in the language of this calculus) a *lesser tensor*, so that

$$\mathrm{T}\,\beta < \mathrm{T}\,\alpha, \qquad (\mathrm{M})$$

as in the two astronomical instances lately mentioned, was found to admit of being presented with great *simplicity of process*, and with (what appeared to be) interesting *interpretations;* for which, however, I must refer you to printed pages of mine, in the *Proceedings of the Royal Irish Academy* for 1847, or in my separate volume of *Lectures on Quaternions*, already often cited. The *contrary case*, of the *perturbation of a superior planet by an inferior*, or of a *planet by its satellite*, namely, the case where

$$\mathrm{T}\,\Delta\,\alpha = \mathrm{T}\,\beta > \mathrm{T}\,\alpha, \qquad (\mathrm{N})$$

requires a *different development of the turbator*, $\Delta\,\phi\,\alpha$, namely, one which proceeds according to *descending* instead of *ascending powers* of the quotient $\mathrm{T}\,\beta : \mathrm{T}\,\alpha$; but which can be effected according to exactly the *same rules of calculation*, although it conducts to a *different series* or system of *component forces*, but still arranged in *groups decreasing in intensity;* so that the *physical interpretations* are not now the same as before.

XXXIII. I have tried, with what seemed to me as much success as in the present state of the calculus could fairly be expected, some other applications of *quaternions* to *physical astronomy*, for example, in some investigations respecting the *invariable plane* of a *system of bodies*, attracting each other according to Newton's law; but am not anxious to multiply such applications, until the calculus itself shall be more mature, and its principles more generally understood. I may, however, mention here, that the method has enabled me to *express* in a new way, some of the fundamental properties of what I called in 1834 and 1835, the *characteristic*, and the *principal functions* of such an *attracting system* of bodies. Those properties had the good fortune to receive the favourable notice of the Imperial Academy of St. Petersburg, as marked by a diploma awarded by that Academy, in 1837, and by the transmission to me of bulletins, which was continued during the late Russian war. They also engaged the attention of the great German mathematician, Jacobi, and were by him so much developed and extended, as to acquire, in his hands, an entirely new importance. Accordingly, in a lithograph, dated Caen, 8th November, 1856, which has reached me since this Letter was begun, the author, Dr. Houel (who had also published in Paris, about a year before, two very elaborate and important *theses*, of mechanics and of astronomy, relating mainly to the same subject), has been pleased to entitle his last paper as follows: —"Note sur le théorème d' Hamilton et de Jacobi, et sur son application à la théorie des perturbations planétaires." And after a sentence or two, M. Houel adds: "Mon but en rédigeant cette Note, a été de fair voir, combien, de toutes les méthodes qui ont été proposées pour arriver aux équations de la variation des constantes arbitraires, celle que Jacobi a déduite des découvertes d' Hamilton est la plus directe, et la plus simple." In mentioning such compliments as these, I trust that I have not been influenced *solely* by that egotism to which it may be hoped that *you* will be indulgent, because it is perhaps inseparable from the very act of writing at any length, although in compliance with your own repeated request, on a subject which, like the subject of these Letters, has hitherto been left *chiefly* in my hands, notwithstanding the important contributions to it, that have been incidentally made by several other persons, since my first papers on quaternions appeared, which contributions I wish that time and room made it possible for me here to specify. My wish has been to express, at least in part, how much I feel the encouraging kindness which has been uniformly evinced to me, by my scientific contemporaries in foreign countries, as well as in our own; and also to suggest how well aware I am, that the capabilities of that new calculus, to which the present Letters have had reference, will never be done justice to, until it shall be taken up in earnest by persons at home and abroad, and many such there are, better fitted than myself for the task. Indeed, the quaternions seem to me to admit of entering into an alliance so close, yet new, with *every part* of pure and applied *geometry*, and at the same time to require such *large* additional developments, before their relations of analogy and contrast to existing methods of *calculation* shall be fully known, that I count myself *merely* to have *begun* them. The field is far too wide to be tilled by a solitary labourer, even with occasional assistance from a few friends, who feel some interest in his exertions. The time may come, though if so, it will be due to other explorers rather than to me, when the *mathematics* of this calculus having become comparatively mature, it shall admit of being extensively and usefully *applied* to *physics*, as a new *instrument* in the study of *Nature.* In the prospect of such a time, I feel with no jealous pain, that although it may have been permitted to me to accomplish *something* in this enterprise, as an honourable Suitor of Science, yet the Bow awaits its Ulysses.—I am, my dear Sir, in conclusion,

Very truly yours,
WILLIAM ROWAN HAMILTON.

OBSERVATORY OF TRINITY COLLEGE,
DUBLIN, *January* 24, 1857.

[Since the date of the foregoing *Letters* (which the Editors have thought it convenient to reprint as they were written, some few sentences only being omitted, and accents or indices corrected), the new branch of mathematics to which they relate appears to have attracted an increasing degree of attention, in our own and in foreign countries. The *Lectures* (Dublin, 1853) have, for example, formed the subject of a favourable article in the *North American Review* for July, 1857: and, indeed, the Quaternions had been mentioned as among the sources of hope for the future progress of analytical mechanics, in the conclusion to a very beautiful volume (*A System of Analytic Mechanics*, &c., page 476. Boston, 1855) on that science; by Professor Benjamin Pierce of Harvard University, U. S., as follows:—" . . . . and much must soon become antiquated and obsolete as the science advances, and especially when we shall have received the full benefit of the remarkable machinery of Hamilton's *Quaternions.*" More recently, in a paper read at Leipzig, in April, 1859, and entitled, "A. F. Möbius, neuer Beweis des in Hamilton's *Lectures on Quaternions* aufgestellten associativen Princips bei der Zusammensetzung von Bögen grösster Kreise einer Kugelfläche," the eminent inventor of the *Barycentric Calculus* has communicated some valuable and interesting elucidations of that *Associative Principle* which has occupied so large a space in the foregoing Letter III.; and indeed has thought it worth while to reproduce the diagram which, in art. XVII. of that Letter, has been given as Fig. 4, omitting, however, the *dotted ellipse* of that figure, because Professor Möbius proposes to prove the theorem of association by certain considerations of *rotation* (avowedly in part suggested by the Lectures*), and not by any properties of *spherical conics*, which, indeed, had been chiefly used by Sir William Hamilton as illustrations. In the same year (1859), Professor P. A. Tait, of Queen's College, Belfast, has published, in the May No. of the *Quarterly Journal of Pure and Applied Mathematics*, a paper on the application of quaternions to Fresnel's Wave Surface; which paper, although based, of course, on the *principles* of the *Lectures*, exhibits considerable power and originality in the handling of the *calculus*. To that important surface, it appears that Sir W. R. H. had long since (as was natural) applied, to some extent, his own mathematical instrument; and recently, besides a paper read to the Royal Irish Academy, he circulated, in section A. of the British Association at Aberdeen, a lithographed account of a singularly short process of *analysis*, whereby he arrived almost mentally at that celebrated *construction* for the wave which had been assigned by Fresnel as the result of extremely complex calculations. We understand that Sir W. Hamilton has been for some time past engaged in drawing up a new work, to be entitled *Elements of Quaternions*, in which he hopes to present the principles and some of the applications of his calculus, in a form more easily accessible to mathematical students than that adopted in the *Lectures*. January, 1860].

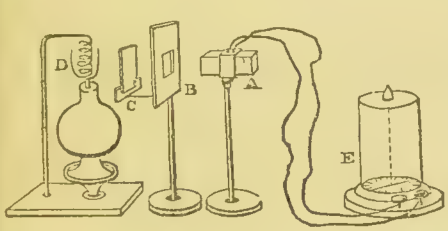
R

Radiant Heat. Heat travels by one or other of three different methods:—1st, By conduction slowly from particle to particle; 2d, By convection, in which the particles of liquids or gases become heated, and by alteration of density consequent thereon, move their position, and thus carry the heat along with them; 3d, By radiation or instantaneous passage by wave motion from one point to another, in the same manner, and following the same laws of propagation as light. By radiation Heat is diffused from world to world in space, passing from the sun to the earth in about eight minutes, a portion of it returning to the sun again in an equally short interval of time. Thus, from body to body on the earth, and from one point of space to another, is heat continually moving. Like all forces or motions emanating from a centre, and spreading as it recedes, it becomes enfeebled according to the square of the distance which it traverses. Thus, at twice the distance it is only one-fourth the intensity, and so on, by which law we are enabled to compute its effects in a multitude of cases, not only in its passage from one body to another in our experiments, but also in its progress through the heavens. Like light, it is reflected and refracted, and according to similar laws, not only when it is accompanied by light, as in the sunbeam, but also when it exists as invisible radiation only perceptible by the thermoscope. Sir John Leslie, by exposing a hollow cube of tinned iron, containing heated water, near a delicate air thermometer, proved that the quantity of heat emitted or radiated from a body depends not only on its temperature, to which it is proportional, but also on the nature of the surface. Thus one side of the cube being polished tin, the next roughened tin, the third covered by writing paper, and the fourth smoked over by lamp black, these sides gave out respectively quantities of heat proportional to the numbers 12, 45, 98, 100. He likewise proved that the power of these surfaces in absorbing heat was represented by the same numbers, and that the power of reflecting it, of course, was in the inverse proportion. Melloni has since shown that the quality of the surface, which favours reflection, and prevents radiation and absorption, is its *density* or compactness. These principles find many applications in natural

* "Herr Hamilton ist in seinen an neuen Ideen und Sätzen sehr reichen *Lectures on Quaternions*" . . . "Wenn man von einem von Herrn Hamilton gefundenen und in seinen Lectures ebenfalls mitgetheilten die Zusammensetzung von Axendrehungen einer Kugel betreffenden Fundamentalsatze ausgeht."

RAD

arrangements, and are of great importance in artificial operations.—Heat passes by radiation not only through celestial space and atmospheric air, but also through many substances, both solid, liquid, and gaseous. Delaroche showed that while the heat of the sun can traverse glass nearly without obstruction, it is otherwise with the heat emanating from a common fire or even from the flame of a candle. A much greater proportion of the latter is kept back; a circumstance which seemed to indicate a difference in the nature of the heat emitted by terrestrial and celestial sources. M. Melloni of Naples, favoured by the delicate means of measuring even very small changes in the quantity of heat afforded by the new science of Thermo-Electricity, has done much toward the investigation of the laws of radiant heat in its passage across different media; and more recently M. Knoblauch, Professor of Natural Philosophy in the University of Marburg, has successfully laboured in the same field. We shall here shortly give some account of their principal results. The apparatus made use of by these experimenters consisted of a Thermo-Elec-



tric Pile, A, acting on a very delicate astatic galvanometer. Different sources of heat, one of which is represented at D; a screen B, with an aperture about the size of the cross section of the pile, and the different media to be experimented on as at C, placed in the course of the beam in its progress toward the pile. Melloni divides bodies into athermanous, or those incapable of transmitting radiant heat, and diathermanous, or those which are pervious to radiant heat. He found that the property of diathermanism, or transparency for heat, was not at all proportional to the transparency of the same substance for light. Thus, dark brown rock crystal nearly opaque to light, yet transmitted much more heat than a similar thickness of almost perfectly transparent crystallized alum. Rock salt is the only perfectly diathermanous solid experimented on by Melloni. He found that out of 100 parts of heat falling on the surface of a plate of this substance, 7 per cent. are reflected, and the remaining 93 per cent. are transmitted, there being no absorption, as shown by the fact that the interposition of such a plate between the source of heat and the thermo-electric pile produced very little change on the position of the needle of the multiplier, and also by the circumstance that the crystal itself remains unchanged in temperature. Bodies are less diathermanous, the lower the temperature of the source from which the heat proceeds. Melloni also found that rays of heat were decomposed by absorption in their passage through diathermanous bodies, in the same way as light suffers partial absorption in its passage through some materials; white light passing in, that is light containing all the colours, but only some rays emerging, giving the appearance of colour to the substance, the others being absorbed in the passage. Such media Melloni called Thermo-chroic. Heat which has passed through glass can pass through a great additional depth of glass without suffering much absorption, while it cannot penetrate alum. Water was found to be nearly athermanous, that is, opaque to heat, notwithstanding its transparency for light. These investigations of Melloni must be looked on with great interest, as opening up new views of many natural phenomena, and also as laying the foundation for more extended research. It has already been stated that M. Knoblauch has since repeated and extended Melloni's observations, and has in many cases confirmed them; while in others he has subjected the results to considerable modification. His chief conclusions, drawn from experiments with a similar, but perhaps a superior apparatus, are as follows:—The amount of heat which passes through diathermanous bodies bears no direct relation to the temperature of the source from which the heat has emanated. Substances are heated in different degrees by the same temperature of the source, and this difference depends not on the heat, but on the peculiarity of the body receiving it. Bodies become heated in proportion to the thickness through which the heat passes up to a certain limit. "The radiating power of a body is the same, be the calorific rays, by which it has been heated, of ever so many kinds." The heat which is radiated by a body is independent in "kind" of the source or sources from which its heat has been derived. The heat emitted by all sorts of bodies at the same temperature, is the same in kind. The following criterion is given by Knoblauch, as a test of whether a substance is a diathermanous or not. "If the heat which impinges upon the plate under examination, when exposed to an argand lamp, cannot by transmission be distinguished from the heat of any other known adiathermanous body, the plate itself is adiathermanous. If differences do occur, it is diathermanous. Heat is greatly modified by diffuse reflection in some cases, while in others it is unaffected; thus rays of heat, reflected from a surface of carmine, had a much greater power of penetrating calcareous spar than those coming directly from an argand lamp, and suffering no such reflection. It was ascertained that the changes experienced by heat on diffuse reflection are merely the result of a select absorption by the reflecting surfaces of certain rays of heat transmitted to them. It cannot thus be said that any body reflects heat better or worse than

any other (metals and charcoal excepted), because this relation varies with each kind of radiation. The complexity of the heat emitted by any source increases, though not proportionally, with the temperature. Knoblauch, in 1852, applied himself to the question whether, in one and the same diathermanous body, the quantity of heat transmitted through any given thickness of it, depends in any degree on the direction of that transmission. Melloni had already concluded that heat passes equally freely through crystals of quartz and calcareous spar in all directions. Knoblauch, on the contrary, found that in brown crystals of quartz rays parallel to the axis passed through in a quantity represented by 100, while if perpendicular to that axis, they only passed in quantity represented by 92. With beryl and tourmaline similar results were obtained. He likewise found that heat which has passed through some crystals such as beryl, parallel to the axis, has a different power of penetrating diathermanous bodies from that which has passed perpendicular to that axis. In all directions of transmission, except along the axis of the crystal, the position of the plane polarization if the heat be polarized, influences the quantity transmitted. This result is only what might have been expected, considering that a ray of light or heat, in traversing a doubly refracting crystal, is divided into two—the ordinary and the extraordinary—the one polarized in a plane at right angles to the other; and that this occurs in every direction of transmission except along the axis. The preceding investigations by Knoblauch, while curious in themselves, are also satisfactory, as agreeing well with the modern theory with regard to the nature of heat. This theory, holding heat to be not a substance but a vibratory motion of the etherial medium in space, or of the ultimate particles of matter, would of course indicate that a ray of heat, from whatever source, ought, at the same temperature, to be of the same nature, as it is merely a wave traversing the ether. This Knoblauch's experiments have shown. He has proved likewise that radiant heat is altered in its qualities by reflection from different kinds of surface—a circumstance which the undulatory theory would lead us to expect, as the particles of every different surface will have periods of vibration of their own, which must be independent of the rapidity of undulation of the wave which sets them in motion; and from these new series of waves originate, and of course partake of their rapidity. The modern theory points out that there may be an indefinite variety of heat in a thermal beam, each kind differing from the other in rapidity of undulation, and that the one may be converted into the other by any agency which has the power to alter the rapidity of vibration. Waves of heat in general have a less rapid vibration than those of light; and it is easy to conceive in this way how the same medium may transmit the two sets of motions in different degrees, in some cases stopping the heat and permitting the light to pass. This no doubt occurs with the comparatively slow thermal undulations into which the sunbeams are converted in their reflection from the surface of the moon, when they are absorbed as they traverse the earth's atmosphere,—a circumstance which has been brought to explain the comparative absence of heat from even the most intense moonbeam, taking for granted that the air is not perfectly diathermanous. Mr. Hopkins has recently applied the foregoing principles of diathermancy to investigations with reference to the temperature of space, and the probable effect of atmospheres in modifying the climate of the different members of the solar system, and has arrived at conclusions not without interest to the science of Radiant Heat.

Radiation. If any influence whatsoever is propagated through space in straight lines, it is said to be propagated by *Radiation.* Laplace attempted to deduce from this simple idea of Radiation, the necessity of the Law that all central forces must diminish in intensity *as the inverse squares of the distances:* but his reasoning is wholly unsatisfactory. This most eminent analyst was a very poor metaphysician, and he wanted some of the qualities essential to the sound physicist.

Railway, or **Railroad,** means, strictly, a road or way furnished with rails or bars, on the upper surfaces of which the carriage-wheels roll. In an extended sense of the word, "Railway," it comprehends all the land, works, buildings, and machinery required for the support and use of the road or way with its rails. The *mechanical advantages* of railways are, *first,* the smallness of the resistance opposed to the motion of a given load by them, as compared with common roads at all speeds, and with canals at all except very low speeds; and, *secondly,* the facility afforded by railway for the use of mechanical methods of propulsion, whereby loads are transported, and speeds attained, which would be otherwise impracticable. The *commercial advantages* of railways are, *first,* direct economy in cost of transit, whether of passengers or of goods; and, *secondly,* economy of time, whether of that occupied in the transport of valuable goods, or of that occupied in travelling by those whose time is valuable. The *social* advantages of railways are, *first,* the cheap and swift transport of those who practise any art, whether by manual or by mental labour, to those places in which their work is required: —*secondly,* the extension of the benefits of rapid and easy travelling to the poorest classes of the community; and, *thirdly,* the rapid and extensive diffusion of knowledge. Railways are of two kinds,—*Plate Railways,* or *Tramways,* in which broad and flat rails of stone or cast iron, with or without ledges or flanches, are used to diminish the rolling resistance of such carriages as are used to convey heavy goods and minerals on com-

non roads; and *Railways* proper, or *Edge Railways*, suited for carriages of a special kind, having flanched wheels, and for locomotive engines. Stone tramways were used at a remote period in Italy. Tramways of granite or cast iron are of frequent occurrence in Britain and elsewhere.—*Railways* proper appear to have been first used in the mining districts of the north of England, for the conveyance of coal, about the middle of the seventeenth century. The rails were at first of wood, and afterwards of wood protected on the upper surface by a thin plate of wrought iron, Cast iron rails were first used at Colebrookdale Ironworks, in Shropshire, in 1769. Wrought iron rails were first tried in 1805, by Mr. Nixon, and, in 1820, were improved so as wholly to supersede cast iron rails, and brought to a form resembling those now used, by Mr. Birkinshaw. About this period, and for ten years afterwards, the construction of local railways of moderate length became much extended, and they were used in various districts for the conveyance of agricultural produce, merchandise, and passengers. In 1829-30, the completion of the Liverpool and Manchester Railway, by George Stephenson, and the practical demonstration of the possibility of attaining rates of speed enormously exceeding anything previously known in travelling, gave that impulse to the extension of railways which has since led to their adoption as the chief mode of conveyance in every civilized country. In the *laying out* and *construction* of the earliest railways, the natural declivities of the ground were followed. Afterwards the practice was by degrees introduced of moderating those declivities by embanking or arching across valleys, and by cutting or tunnelling through hills, as well as the practice of avoiding interference with the traffic of roads and streets, by carrying the railway over or under them by means of bridges, until at length the works in earth, masonry, timber, and iron, employed in the construction of railways, have become the greatest of all mechanical structures. The extent to which it is advisable, on a proposed line of railway, to moderate the declivities or "gradients," and so to diminish the cost of transport, by means of works which increase the cost of construction, is a question for the judgment of the engineer. The *motive power* which was first employed on railways, and which continues to be employed in particular cases, was that of *horses.* On railways, as on common roads, the speed at which the efficiency of a horse is greatest, varies according to his breeding and other peculiarities, from three to ten miles an hour. *Gravity* is employed as a motive power, where the declivity of a descending gradient is sufficient to overcome the resistance of the carriages. Where the whole of the heavy traffic descends from a higher to a lower level—as in some of the local mineral railways—*self-acting inclined planes* are used, on which the loaded waggons descending, draw up the returning empty waggons by means of a long rope, supported by sheaves or pulleys. *Stationary Steam Engines* are used to draw trains of waggons or vehicles by means of ropes, (now generally made of iron wire). The employment of such engines is chiefly confined to very steep inclined planes, although they have occasionally been used elsewhere. For a short time, stationary steam engines were used on some lines of railway, called "Atmospheric," to exhaust air from a tube lying between the rails, containing a piston, to which the train was attached, so as to be dragged by the atmospheric pressure. The motive power employed on all main lines of railway, and on almost all branch lines, is that of the *Locomotive Steam Engine.* In 1759 (according to Watt), the idea of employing steam to move wheel-carriages was suggested by Robison. In 1784, Watt patented a locomotive steam engine, which, however, he never constructed. About this time, or shortly afterwards, a small working model of a locomotive engine was made by Murdoch, assistant to Watt. In 1802, Trevithick and Vivian patented a locomotive engine, which, about 1804, was constructed and set to work, and travelled at about five miles an hour, with a net load of ten tons. From time to time after this date, the locomotive engine was gradually improved by various engineers, but by none so much as by George Stephenson. To him, in particular, is due the invaluable invention of the *blast-pipe*, which, by blowing the waste steam into the chimney, regulates the draught of the furnace, and the consumption of fuel, according to the work which the engine is performing at the time. Up to 1829, the ordinary speed of locomotive engines did not exceed that of horses; and, according to Mr. Wood, the maximum practical performance of an engine, weighing, with its tender, about ten tons, was to convey forty tons at six miles an hour. In 1829, the *tubular boiler*, essential to the obtaining of a large heating surface in a limited space, was invented by Mr. Booth, and executed by Mr. Stephenson; and on the 6th of October, 1829, occurred that famous trial of locomotive engines, when the prize offered by the directors of the Liverpool and Manchester Railway was gained by Mr. Robert Stephenson's engine, the Rocket,—that engine having attained the speed, till then considered impossible, of twenty miles an hour. Since that time the locomotive engine has been varied, and improved in various details, and by various engineers, too numerous to be mentioned here. Its weight now ranges, according to the work it has to do, between six tons, and fifty tons, including the tender. Its load ranges from fifty to 500 tons. Its speed, for the conveyance of minerals, is from ten to twenty miles an hour; for that of goods from fifteen to twenty-five; for that of passengers and mails, from twenty to sixty miles an hour. The true theory of the locomotive engine was first published by Pambour in 1836. The *passive resistance* to the motion of the carriages on railways consists of three parts;—first, the friction

of the axles in their bearings, and of the wheels rolling on the rails, which is a constant quantity at all speeds, and varies according to the construction of the wheels, axles, and axle-boxes, and the kind and mode of application of the unguent, from $\frac{1}{500}$ of the load to $\frac{1}{250}$:—secondly, the resistance arising from vibration, the law of which has not been exactly ascertained, but which appears to vary nearly as the velocity, and to be inappreciable, or nearly so, below ten miles an hour;—and, thirdly, the resistance of the air, varying as the square of the velocity. The general result is, that the total resistance of a railway train varies from about $\frac{1}{300}$ to $\frac{1}{100}$ of the gross load. The extremely low resistance of $\frac{1}{500}$ of the gross load has never been obtained except with one peculiar description of passenger carriage, formerly used on the horse-worked railway between Edinburgh and Dalkeith. Much remains still to be ascertained respecting the resistance of railways.—AUTHORITIES. Wood *On Railroads;* Simms's *Public Works of Great Britain;* Pambour *On the Locomotive Engine; Reports of the British Association; Report of the Commissioners on the Gauges; Annual Reports of the Railway Commissioners;* Gouin, Lechatelier, &c., *Traité des Machines Locomotives;* Clark *On Railway Machinery;* Rankine *On Cylindrical Wheels*, &c.

Rain. A meteor too well known to need description. The integrant particles of a cloud or fog are hollow vesicles, capable of floating in the air or of being kept from falling by the slightest breeze. When these vesicles break or coalesce, they produce solid drops, varying in size from the slight molecules of a *drizzle*, up to the massive globes of a thunder-storm. It has not hitherto been explained completely, why or under what circumstances a cloud is resolved into rain; but the following are the most frequent causes of condensation : — 1. The cooling of the clouds through effect of radiation from them; 2. The mingling of vapours at different temperatures,—a mingling effected through the agency of the winds; 3. The rising of vapours towards colder strata of the atmosphere; 4. The increase of atmospheric density or pressure; 5. The accumulation and impinging of masses of vapour against some obstacle.— The comparative quantity of rain falling at any locality, depending on the relation of that locality to the foregoing efficient causes, is therefore the consequence or result of very complex circumstances, of which the principal are,—the latitude and elevation of the place: the nature of the prevailing winds: the laws of the seasons there: its degree of proximity to the sea: and the contour of the surrounding surface, whether flat or mountainous. We shall hastily review what has been ascertained on this important subject, as well as with reference to two other collateral inquiries of high physical interest.

I. DISTRIBUTION OF RAIN OVER THE SURFACE OF THE GLOBE.—As explained under HYDROMETEORS, the laws of the distribution of rain over the earth's surface, would have been extremely simple, but for certain disturbing forces. One class of these forces,—viz., the *winds*, is so powerful, and so intimately connected with the chief cause of rain,—viz., the *gradually diminishing heat of the globe from equator to pole*, that the two influences cannot be divided; a second disturbance arises in the *change of seasons*, although this, too, is, for the most part, inseparable from the first class; and to the *third*—a very multifarious class—depending on *elevation, neighbourhood, nature of soil*, &c., we owe the existence of *anomalies*. Although no clear separation can really be effected, it may tend to distinctness that we examine these three subjects apart.

(1.) *Effects of Temperature and Wind.*—The effects of these two leading influences throughout the year, over all the earth, appear with sufficient clearness, through the following leading facts:— 1. Under the tropics, rain falls with a regularity unknown in other regions. On the ocean, where the trade wind blows regularly, it scarcely rains; but, in the zone of calms, it rains frequently, and then, also, sudden storms frequently occur. On land, in these torrid spaces, we have, on the other hand, alternately dry and wet seasons. Thus, in Southern America, the sky is serene during winter; in spring it becomes moist; the series of storms commences in March; in some places the nights are severe, while in others, it rains more during the night than during the day. The direction of the wind, and the heating of the soil, amply suffice to explain these phenomena, which last all summer. 2. In Africa, also near the equator, the rainy season commences in April; between the northern tropic and 10° N. lat., that season continues from June to October. In places near the equator, whose zenith the sun crosses twice, great quantities of rain fall twice a-year, so that there are two wet and two dry seasons. The rain which falls is so great that the whole night is moist. This is the period in which maladies occur that are so fatal to Europeans. 3. Still keeping within the torrid zone, turn now to India. In that country the alternation of rainy and dry seasons keeps pace with the winds. On the east coast, during the south-west monsoon, there is no rain ; while, during the north-east monsoon, it falls in torrents. On the west coast the phenomena are precisely reversed. In the interior of that continent, rain seldom falls; and in places on either side of the central region, the climate partakes of that of the nearest coast; some places are even affected by both coasts, *i. e.*, it rains there more or less during the whole year. The cause of these phenomena is simple. Winds that have blown over the ocean, and therefore laden with humidity, reach the land; where, through effect of a sudden diminution of temperature,

ey discharge the greater proportion of their oisture at once. Hence the quantity of rain lling at some places in India is prodigious; e drops are of great size, and they fall to the rth with astonishing force. 4. Receding now om the equator towards the North, we reach untries where the maximum of rain falls dur- g summer, although they are comparatively oist at all seasons. These are the regions over ich south and south-west winds prevail,— nds that are comparatively humid, as, for the ost part, they blow across oceans. Now, when ese winds mingle with the colder masses that me from the north or north-east (currents that so belong to the countries spoken of), it is anifest that rain must fall, and that the fall ll be most frequent during the seasons when the uthern winds mainly blow. Advancing farther the North, we reach those polar countries that e visited only by north winds. In these high itudes the temperature of the continents is, ring summer, considerably higher than that of e sea; so that, in that period of the year, the evailing winds will not part with their humi- ty, but rather appear dry winds. In winter e distribution of temperature is the reverse; d the aerial currents, accordingly, as soon as ey reach the land, begin to get rid of their oisture in the form of snow. 5. The *winds* ing themselves the direct effect of variations temperature, and of the rotation of the earth, is clear that, when referring the fall of rain to eir influence, we are indirectly referring it to mperature as its primal cause. Nevertheless, the winds act directly in controlling this eteor, as well as in modifying other cognate encies, we must look to them as our best clue what might otherwise appear inextricable. e winds, indeed, are a clue to all the leading riations or fluctuations of climate; only it ust not be forgotten, that, in every separate cality, the different winds have a distinct and culiar character. This is emphatically illus- ated by the annexed table, giving the propor- tions of rain due to the different winds in three important localities in Europe,—Paris, Berlin, and Petersburg.

TABLE I.

Rains due to the several Winds.

Localities.	N.	NE.	E.	SE.	S.	SO.	O.	NO.
Paris,.........	0·13	0·09	0·11	0·29	0·39	0·85	0·54	0·38
Berlin,.........	0·48	0·31	0·30	0·26	0·33	0·51	0·57	0·58
Petersburg,..	1·00	0·46	0·82	0·71	0·84	1·00	0·45	0·79

(2.) *Influence of the Seasons.*—The seasons are best known through their differences of temperature; and as that difference must modify the prevailing winds, it cannot be doubted that great diversities in the fall of rain will also accompany these changes,—a diversity proportionate to the intensity of the change. Referring to our previous course of remark, and, farther, to the close of this section of our article, we merely subjoin in this place two important tables: the *first* indicates the *mean quantities of rain* due to the different seasons in a few important localities, and the *second*, the *number of rainy days* characteristic of each. Our next table, containing the number of rainy days during the different seasons at different localities, is perhaps the best indication of the peculiarities of the productive character of a climate, in so far as it depends on this Hydrometer. Productiveness demands, not only a certain although moderate supply of rain, but such a distribution of it likewise, among the different seasons, as shall be suitable to the processes of growth and ripening, and to the operations of harvest. A slightest glance at this table, may show that it marks out the true distribution of cultures in Europe. For instance, the *fourth* region especially, and after it the *fifth*, are the countries which, *par excellence*, are fitted for the cultivation of *corn*. And it is in these regions, that we find the superb plains of Lombardy, and all those old granaries of Europe—Sicily, Egypt, Barbary, &c.

TABLE II.

Mean Quantity of Rain at Different Seasons.

Country.	Winter.	Spring.	Summer.	Autumn.	Entire Year.
West of England,.........................	9·5	6·7	8·7	11·2	37·0
Western Coasts of Europe, ...	7·3	5·5	6·7	9·7	29·3
East of England,.........................	6·5	5·7	6·7	8·0	27·0
South of France,......................... / Italy, South of the Apennines,..........	7·6	7·6	5·1	11·4	31·6
Italy, North of the Apennines,..........	5·5	9·9	10·8	13·9	40·2
North of France and Germany,.........	4·9	5·8	9·0	6·8	26·8
Scandinavia,................................	3·2	3·0	6·7	5·8	18·7
Russia,..	1·6	2·4	6·5	3·8	14·3

TABLE III.

Number of Rainy Days.

Country.	Winter.	Spring.	Summer.	Autumn.	Entire Year.
West of England,	43·1	37·6	33·9	44·9	159·5
East of England,	40·0	39·5	34·4	38·8	152·7
Western Coast,	34·4	34·4	32·9	38·0	139·7
South of France, } South of Italy, }	25·4	25·2	15·2	25·4	91·2
Italy, North of the Apennines,	25·4	27·1	25·1	26·6	104·2
Northern France and Germany,	36·1	37·0	36·8	35·0	144·9
Scandinavia,	35·2	30·3	32·6	35·1	133·2
Russia,	23 1	23·4	27·9	26·5	100·9

(3.) *Anomalies in the Fall of Rain.*—If one examines any large table giving the mean fall of rain at a great number of localities widely scattered over the earth, the truth of the General Laws already shadowed forth would sufficiently clearly appear: but, alongside of these, a number of extraordinary exceptions or anomalies. For instance, while the annual mean quantity for Scandinavia is 18·7 in., the mean fall at Bergen is 88·58 in.: the mean quantity for Bombay is 92·51 in.; while that of Seringapatnam is only 23·6 in. Russia, again, is generally a remarkably dry region; and there are isolated districts great or small in various parts of the earth—some of these almost equatorial, in which no rain whatever falls during any part of the year. The causes of these facts have clearly nothing to do with latitude, and not much with the direction of the winds. They are the true irregularities or anomalies of the subject, and depend on the lesser or more purely local circumstances that influence the fall of rain, viz., the structure of the soil, the neighbourhood or absence of mountains, &c., &c. In illustration of the mode of their operation, a few remarks must suffice. 1. When a humid wind impinges on an immense mountain wall, its vapour is discharged in great quantities on the wind-side of that wall. The south-west wind, for instance, that beats against the range of Mont Blanc, discharges at Chambery no less than 65 in. of rain during the year. Bergen lies in the same relation to the same wind and the Scandinavian Alps,—hence its excessive wetness. And a third instance, in circumstances exactly corresponding, is found at Tolmerro near Venice, one of the rainiest districts in Europe. It is precisely this class of agencies that induces the large quantities of rain on the west coast of Scotland, in Cumberland, Westmoreland and Wales, and on the west of Ireland.—2. Comparative dryness, again, frequently arises from this;—a moist wind after passing a mountain barrier, or blowing across an elevated and cold plateau, where it has discharged its moisture, becomes virtually a dry wind. Hence the rarity of rain at Seringapatnam; hence, also, the comparative dryness of Russia. Rain and snow fall there in small quantities, because the vapours belonging to the prevailing winds have been nearly discharged before these winds arrive at Russia.—3. There are a few spots on the earth's surface where rain never falls; viz., the Desert of Sahara; the North of India and China; some places on the coasts of Peru and Chili, and some others on the shores of Mexico. The causes last referred to operate exclusively in some of these localities. In others, as in the Sahara, the phenomenon is solely owing to the excessive heat of the soil, which rather augments the capacity for vapour of any wind that can blow over it. ——— We cannot conclude this enumeration of facts without referring to the admirable graphic representations of them now within reach of the student. The Hyetographical maps of Berghaus, especially as they have been reproduced and amended by Mr. Keith Johnston in his Physical Atlas, impress the facts themselves and their arrangements, on the mind, with equal force and precision. On these most valuable charts the position and extent of the bands or zones of summer and winter rains are represented with every clearness, as well as the regions of tornadoes, and the influence of the monsoons. The eye is obliged to rest, *first*, on a zone of summer rains stretching towards that limit where the south-west winds become dominant; *secondly*, on a zone of rains at all seasons; and, *thirdly*, on the zone of winter rains and snows, in the neighbourhood of the polar circle.—In the continent of Europe, as we advance from South to North, these rain zones are twisted in conformity with the shape of its coasts. Under the meridian of Central Europe we find equatorial summer rains; and near that, in Russia (*vide supra*), a space comparatively free from rain. Succeeding the dry zone, we have a secondary zone of winter rains; then a zone of rain at all seasons, but principally in summer; and, finally, the polar zone of winter rain.

II. State of the Barometer during Rain.

- All observers who pursue barometrical variaons as a study, have remarked that the mercury abitually falls on the approach of rain, and, on ie contrary, rises when the weather is fine: this incidence, however, proceeds solely from the sition of our country, and the direction of the inds which bring rain. It is necessary to reember that the barometric column measures ily the weight of the atmospheric column sitied above it, changes with the different winds, id is generally lower when the temperature ses; that is to say, hot winds lower the baroeter, and cold winds raise it; hence it follows iat the south-west winds, which, in our country, 'tenest bring rain, being the hottest, the baroeter falls when the winds blow in that direction. his general law has many exceptions, and it is ecessary, in order to understand the phenomenon ight, to enter into some details respecting it.—ccording to the theory of vapours, water evarates in the air as in a vacuum, although more owly; hence, if a mass of air is brought into intact with water of a certain temperature, the r becomes saturated, and the weight is increased; ence, further, a mass of air, or a wind, blowing 'er a surface capable of yielding vapour, necesrily becomes heavier. A wind, therefore, acts i the barometer alike by its *temperature* and its *umidity;* the first influence tending to lower the ercurial column, the second to raise it. In our imates, the first cause is the strongest; in others ie contrary; thus, Flinders discerned that, on ie coasts of New Holland, the land winds, dry id hot, depress the barometer; at the mouth of ie Plata, winds from the eastern sea raise the irometer higher than west winds which blow om the land.—When, in any place, rain falls, ince the weight of the vapour increases the eight of the atmosphere above the barometer), ie mercury should fall after the fall of rain; it the reverse is frequently observed. It is ecessary, in such cases, to take into account the rection of the wind, and the causes which have casioned that wind.—When the rain falls connuously, the state of the barometer depends on ie direction of the wind, and on that condention of vapours which diminishes the weight the atmosphere; but if the rain fall in a short, eavy, and isolated shower as in a storm,—*i.e.*, om the passing of one or more clouds above the server, then—the weight of this cloudy mass eing supported by the atmosphere—there results om it a kind of wave which passes over the irometer, and we may have an elevation of the ercurial column during rain, that ceases someme after.—In general, in times of rain, the irometer falls below the mean corresponding to ie prevailing wind: the barometer accordingly in only serve to prognosticate rain by inditing the direction of the prevailing wind; the irometer falls, not because it must rain, but ther because the diminution of pressure is ocsioned by a wind bringing rain: the rain is not a cause, but an effect. It would then be more correct to put on the barometers in our houses, instead of the words fine weather, rain, or wind, &c., the names of the winds, N.-E., S.-O., &c., the means of which correspond to these barometric heights.—M. Dové has explained the variable winds of these countries, by the simultaneous meeting of the south-west and north-east winds; and he has deduced from a great number of observations, that, in our hemisphere, the wind passes most often from east to west by the south, and in the southern hemisphere from east to west by the north. He has concluded from these conflicts of two winds—the one hot and humid in our regions, viz., the *south-west*, the other dry and cold, viz., the *north-east*—that at the west a cold wind succeeds to a hot wind, and that at the east it is the reverse, a hot wind succeeds to a cold wind. He has sought the pressure during rain under the different winds, and has been led to this conclusion, that, during rain, the barometer falls with the east wind, and rises with the west wind.—When, in any portion of the atmosphere, the equilibrium of the gaseous mass is broken, a movement is always and immediately communicated and transmitted as an immense wave, giving birth to winds more or less violent. The diurnal heat, and a crowd of causes which have not been fully appreciated, give rise to regular movements of the barometrical column; but if, in one portion of the earth, the temperature is accidentally much raised, the equilibrium is destroyed, and its restoration occasions the displacement of air, and agitations of the atmosphere capable of producing disastrous consequences. In general, during these great displacements, the barometrical column oscillates irregularly and at short intervals. Thus, frequent oscillations or a great change in the height of the barometrical column, indicates great atmospheric perturbations; we may thence foresee a storm or gusts of wind in the place where the observations are made, or in neighbouring places. Navigators, who have a great interest in knowing the precursory signs of tempests, always consult the barometer; and Scoresby assures us he foresaw, from such observations, seventeen out of eighteen storms.

III. The Quantities of Rain falling at the same time at different heights in the same Vertical.—A most important inquiry concerning what may be called the generation of rain at different heights in the same vertical column, was recently taken up, and carried to a conclusion—in so far as its relation to temperature and the seasons are concerned—by Professor Phillips and Mr. Gray, at York. York is admirably fitted by its position for the conduct of any meteorological inquiry. It is in the centre of one of the greatest and least interrupted plains in England; and accordingly the march of temperature there is most regular;—the difference between the diurnal mean at any

date, and the annual mean, is sensibly proportional to the *sine* of the declination of the sun twenty-five days previous to the date in question. Three rain gauges were employed, one on the surface of the ground, another on the top of the Museum, and a third on the top of the lofty tower of the Cathedral. Their elevation above the Ouse were respectively, 29, $72\frac{2}{3}$, and 242 feet. The fall of rain diminished with the altitude; and, after continued and careful observations, Professor Phillips concluded, that *the diminution* of rain at any height above the surface corresponds with the quantity falling at the surface, and is represented by the formula,

$$m \,.\, \sqrt{h}$$

h being the height, and m a variable co-efficient. For the mean of the year the co-efficient at York is 4·2: but it varies with the seasons; and the following formula serves to express the law of that variation:—

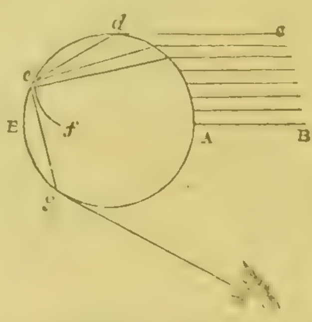
$$m = \frac{a \cdot \frac{1}{t} + a \cdot \frac{t^2}{t'^2}}{2}$$

where a is the value of m for the year; t the mean annual temperature in degrees of Fahrenheit, and t' the mean temperature of the season in question.—This co-efficient is plainly dependent on the humidity of the air, and may be further expressed as a function of that humidity. But the dryness of the air is usually expressed by the difference between the mean temperature and the dew point. Observations of the dew point, however, being comparatively rare, we must take instead the difference between the maximum and minimum temperature of each day. Calling the general mean temperature of the year M, and d the mean difference between the diurnal maxima and minima of the special period, we have the formula

$$m = a \cdot \frac{M}{d},\text{—}$$

a formula answering all the observations. For further information we must refer to the original memoir. But the conclusion reached is as follows:—The difference of the quantity of rain at different heights above the earth's surface, is owing to the *augmentation of each drop* as it traverses the lower humid strata of the atmosphere: the temperature being lower among the strata where the drop originated, that drop must, since it carries its original temperature along with it, condense fresh vapour around it as it descends.—This conclusion, which is not an hypothesis, but a rigorous deduction, gives account of all the facts as yet ascertained connected with the subject.

Rainbow. The ordinary rainbow consists of a series of successive zones or bands, coloured like prismatic spectra. These bands make little concentric circles on the sphere, which have a common centre almost always below the horizon, and diametrically opposite to the sun. The red band has the largest radius (42° 20′). It is, consequently, situated on the convex or outer edge of the rainbow, while the violet is on the inner edge, having its radius 40° 30′, and the intermediate prismatic colours come between. The part of the sky on which the rainbow is thrown is much more bright within than without the bow,—the outer space is dark, almost black; and the inner space, on the contrary, melts into the violet almost insensibly. Often, in the latter, coloured rays alternately red and brown show themselves—bordering the violet towards its inner part: these are called *supernumerary arcs.* Besides the first arc, there is often a second seen parallel to the first, consisting, that is, of concentric zones, the red band having a radius of 50° 20′, and the violet of 53° 45′. Hence 1. This is broader than the *primary* rainbow. 2. the colours are arranged in quite the opposite way. The dark area is inside now, and the bright outside. The space between the two is very notably dark and black. There are in this also some supernumerary arcs. Finally, one sometimes sees about 40° from the sun a third very feeble coloured arc, in which, as in the first, the red is farthest from the sun. The rainbow is sometimes replaced by another phenomenon—an arc which has a radius varying between 34° and 42°—which is almost completely white. It forms on clouds very near the observer. Biot first showed that rainbow light is almost completely polarized, in a plane passing through the centre of the sun,—such the phenomena of the rainbow.—The physical explanation we must succinctly exhibit:—Suppose a spherical rain drop, on which rays coming from a very distant body, and therefore parallel, are falling. Clearly these rays are partly reflected, and partly refracted into the drop. By the former, the drop itself becomes visible. The intersection of the latter forms a caustic curve $e\ f$—that is, all the rays falling on A d get massed into E $c\ f$, and so, much more concentrated. These rays are partly refracted out, and partly reflected against the surface of the



drop. These reflected rays may be either refracted out, producing the primary rainbow; or, again, reflected and then refracted out, giving the secondary rainbow, and so on for those of the

ird, fourth, fifth, &c., order. Suppose the red ys to be considered, then taking the index of fraction known for red rays, we find that the ngle of the line A B, and a line from g just bove, between which a great many of the rays nstituting the rainbow are massed, is 42° 20′. s the drop is spherical, we see that this line om g will describe a cone round a parallel to e direction of the ray through the eye, accord- ng as the circle E A is turned round the axis A B, so as to describe the real spherical rain rop. Hence as the eye sees, for the plane re- resented, a red spot in the direction of g, this ed will revolve and describe a circle in the sky, onstituting the red band of the rainbow. It is ear how the other colours, having different re- ractive indices, will form different caustic curves f, and will have the side of the cone to a point igher on the circle than g, that is, making a less ngle with the direction of the rays. This ex- lanation gives the whole theory of the subject. he same is repeated over and over for all the inbows of different orders. We refer the reader Engel and Shellbach's *Graphical Representa- ons of Optics*, plate 14, which will give him a uch clearer idea of the whole process of the rain- ow than can in any other way be obtained. The athematical expressions for this theory we shall ot give here, because, as generally stated, they re little more than the explanation above; and e accurate detail of the theory is somewhat long nd difficult. M. Bravais has found this formula r the deviation of the ray from the middle of e spectrum, for the nth rainbow,

$$D = (n + 1)\,83^\circ \cdot 4' \cdot 8 - \frac{50^\circ \cdot 46'}{n + 1}$$

ainbows of the third order have been seen by bservers, but the phenomenon is very rare. Of igher orders the bow is never seen. The ex- lanation is, that so much light is lost in the uccessive reflexions and refractions that the bow ecomes invisible. A further one is, that the bow ecomes always broader, and the light therefore ore diffuse. Apparatus has been devised to how distinctly the phenomenon up to even the thir- enth order. A cylinder of glass, filled with water, s used to give an approximation to the spherical ain drops. The results of such experiments are ully given in Babinet's Memoir in the *Comptes Rendus* for 1838.—The rainbow is usually pro- uced during rain—often towards the end of a hower. But many other conditions of formation re possible. The dew drops often produce the ppearance of a rainbow along the ground a ittle after sunrise: the arc appears a large para- ola. The rainbow is also produced by the play f fountains and in cascades. If the sun be in a osition suitable, the rainbow may even appear in uch cases a complete circle. The rainbow may e seen also upon sea foam. The thickness of he sheet of water (cloud or sheet of rain drops) eed be very slight. Bravais noticed a true rain-bow formed against the mere water-dust flying against the saloon windows of a steamer, from the paddle wheels. He estimates the thickness as not more than about six inches.—There is such a phenomenon as a *lunar* rainbow. It is usually pale and colourless, but not always. There must be a very dark cloud-ground behind the sheet of water, to show it. It is very rare to see a secondary lunar rainbow.—Another remarkable species of rainbow, consists in the arc produced by the reflected image of the sun in a sheet of water. The tops of this rainbow and the natural rainbow, will evidently be separated by twice the altitude of the sun. In this way, however, the whole new bow may take all kinds of positions with respect to the ordinary one—cutting it, lying between the primary and secondary, &c. If the sheet of water be very small, this rainbow by reflection must be itself very incomplete, and the arc itself will appear rather in fragments of luminous sky. If the sun be near the horizon, and the reflecting water slightly agitated, the top part of this bow of reflection disappears, and the sides appear like vertical columns, tangents to the ordinary arcs. The image of the sun by reflection is, therefore, much lengthened out vertically; the bow by reflection is the envelope of a series of primary bows having their centre in the sun's vertical, which, in the culminating part, separate from one another, and by the diffuse light produce the invisibility of which we speak. When a rainbow forms on a mist it is white and colourless, or at most with a slight reddish edging. Further, the radius of the bow is less by from 0° to 0° than the ordinary rainbow. Bravais shows the phenomenon to arise from the fact that the spherical drops are hollow, the rainbow beginning to show itself at all when the ratio of the outer to the inner diameter is not less than 1·38: the radius will then be about 34°. As the proportion increases to 1·43 the radius increases to 39°· 20′, and there begins to be a slight reddish fringe at the edges. When the ratio has reached 1·55 the hollow drop produces all the effects of the solid drop.—It remains only to discuss the supernumerary arcs which we have spoken of—bands red and green alternately which border the interior of the arc of the first order. The arcs are not always seen, and vary in number and character. Young has shown them to be a result of interference, for which the drops must have sensibly the same diameter;—it is due to the rays near the efficacious rays of the rainbow, which traverse the drop in such directions, that their deviation differs from the minimum deviation. For every deviation a little above that, there are two distinct rays, the one with incidence a little greater than that which gives the minimum of deviation, and the other with incidence a little smaller. These rays having routes slightly unequal, may interfere and produce alternations of light and dark in the colour answering to these rays. The

fringes which result from these interferences for all the colours are superimposed in the sky; and bands analogous to those in the coloured rings of thin plates result. Space compels us to refer for detailed experiments, and explanations of these supernumerary bands, and of much of the whole theory and history of the subject, to an article by Bravais, in the *Anuaire Meteorologique* for 1849; and to the *Graphical Representations* of Engel and Schellbach, of which we have spoken.

Rain-Gauge, or Pluviometer. An instrument whose object it is to measure the depths of rain falling within certain intervals on the earth's surface, at the locality where it is placed. No meteorological instrument, generally speaking, is so imperfect and little trustworthy as rain-gauges in general. The simplest form is the best; nor can a better be devised than that suggested by the Rev. Professor Fleming, of Edinburgh. It consists simply of a cup or receiver, whose opening should be flush with the soil's surface, and which by a funnel delivers the rain into a jar below. The liquid can at any time be poured into a graduated vessel, and measured; or the jar itself may be graduated. This arrangement, which costs only a few shillings, is superior, in point of accuracy, to every complex apparatus that men of over-ingenuity have from time to time proposed. The right placing of the rain-gauge is of paramount consequence. The writer of this has seen one on the top of a pavilion or tent roof, and across which any current of air whatever—necessarily deflected by the roof—simply carried both rain and snow in a fine scientific *swirl!* The extreme imperfection of our knowledge as to the distribution of rain is unquestionably owing, in most part, to the imperfection of this instrument, and the careless way in which it is used.

Range. A technical term connected with *gunnery* and *projectiles* (*q. v.*)

Rarefaction. The laws of rarefaction under increased heat, and under diminished pressure, and under both combined, are given as Dalton's, Marriotte's, and Amonton's laws in PNEUMATICS.

Ratio. The ratio of two quantities simply signifies the number of times that the one quantity is contained in the other. Sometimes this number cannot be stated, *i.e.*, the one quantity is not contained in the other *any definite* number of times. The quantities are then said to be *incommensurable*. Euclid's Fifth Book contains the general Geometrical Doctrine of Ratios; and perhaps no portion of that remarkable volume is more worthy of the ingenuity of the Greek Intellect.—It is very clear that no ratio or proportion can exist between quantities of *different kinds;* for instance, between a solid and a surface, or between an angle and a line. Hence that general principle in geometry termed the *Principle of Homogeneity.* A Principle quite unquestionable, if it be kept in view that in Functional Equations between magnitudes of apparently different classes, *constants* may exist that convert, by their presence, magnitudes apparently heterogeneous into Ratios.

Ratio Harmonic: Harmonic Pencils: Anharmonic Ratio. The foregoing terms have now acquired so important a significance in the Modern Geometry, that an exact definition of them is necessary here. (1.) In *Arithmetical* proportion the differences of any two consecutive terms are equal. In *Geometrical* proportion, the quotients of two consecutive terms are the constants. The nature of *Harmonic* proportion is as follows:—Let the line A B be cut internally

Fig. 1.

at O and externally at O′, so that

$$AO : BO = AO' : BO',$$

it is said to be cut *harmonically:* O O′ is an *harmonic mean* between A O′ and B O′; and if A B be supposed invariable, while O and O′ change, these two points are named *harmonic conjugates.*—A *Harmonic Pencil* is this: from any point, O, let four straight lines be drawn cutting the line A D harmonically, or so that

$$AD : DC = AB : BC.$$

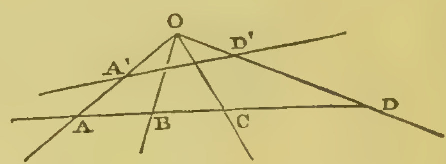


Fig. 2.

These four lines are an *harmonic pencil.* It may easily be proved that they will also cut harmonically any other transversal line, such as A′ D′.—*Ratio Anharmonic.* If four fixed right lines V A, V B, V C, V D be drawn from the same point, and any transverse right line A D be cut by them in four points A, B, C, D, the ratio of the rectangle A D · B C to the rectangle A B · C D is constant. (This ratio is called the *anharmonic ratio of the four points* A, B, C, D.) For, through C draw M N parallel to A V; then, A D . B C : A B · C D : : $\begin{Bmatrix} BC : AB \\ AD : CD \end{Bmatrix}$: : $\begin{Bmatrix} MC : AV \\ AV : CN \end{Bmatrix}$: : M C C N; but this last ratio remains constant when the point C changes its position along the line V C; the proposition is therefore proved. When the transversal coincides with M N, A B and A D become infinite, and are in a ratio of equality, and therefore A D . B C : A B · C D : : B C : C D : : M C : C N, as before. If

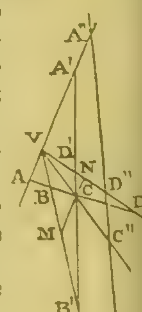


Fig. 3.

the transverse line be drawn through C, so as to intersect A V produced through V, we shall still find that A′ D′ . C′ B′ : A′ B′ . C D′ : : M C : C N; and if C moves along V C into any other position C″, we shall have for any line such as A″ B″, A″ D″ . B″ C″ : A″ B″ . C″ D″ : : A D . B C : A B . C D. From the last remark it immediately follows that, *if the four given lines be all produced through* V, *the ratio of the rectangles under the segments of any transversal cutting them in any way will always be the same*, *the rectangles being written as above, and the same letters* A, B, C, *or* D, *being used to represent a point anywhere along the whole extent of one and the same right line.* In other words, the anharmonic ratio of the four points on the transversal will remain constant. The right lines meeting in a point are said to form a pencil; and the constant ratio above-mentioned is called *the anharmonic ratio of the pencil.* This may be expressed by the notation V . A B C D, V being the vertex of the pencil. It is evident that, *if two pencils cross the same right line in the same four points, the anharmonic ratios of the pencils are equal*, both being equal to that of the four points. It is also evident, that *if the angles contained by corresponding legs of two pencils be equal, the pencils have the same anharmonic ratio.* The great importance of the anharmonic properties of a pencil was first indicated by Chasles in the Notes to his *Aperçu Historique.* The principle itself is given in the *Math. Col.* of Pappus, b. vii., prop. 129.

Ratios, Prime and Ultimate.—The terms which Newton employed to denote the fundamental and elementary notion of limits, already explained. See LIMIT. The ultimate ratio of two infinitesimals is that to which the prime or real ratio of any two of them continually approximates the smaller they become, beyond which, however, they in no case pass. The notion of *limits* is nearly the same; and as this term is more simple, it is commonly employed.

Rays. If any physical influence, such as *Light* or *Heat*, is propagated along straight lines issuing from a central point, it is said to be propagated along *Rays.* The significance of the term has recently been extended. In its most general sense it means any group of straight lines drawn from a fixed centre, and whether they are contained within the same plane or otherwise. In this very general meaning it is now frequently employed in geometry.

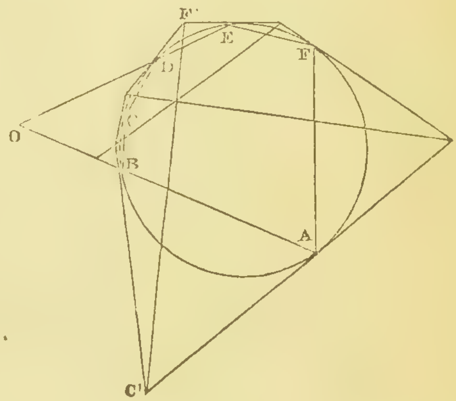
Reaction. *Action and reaction are equal and opposite.* Thus, when a pistol is fired, there is a back effect upon the pistol identically equal to that upon the bullet.

Reading Microscope. See MICROSCOPE and CIRCLES.

Reaumur. For a description of the thermometer so named, see FAHRENHEIT.

Receiver. The vessel from which the air is exhausted in an air pump.

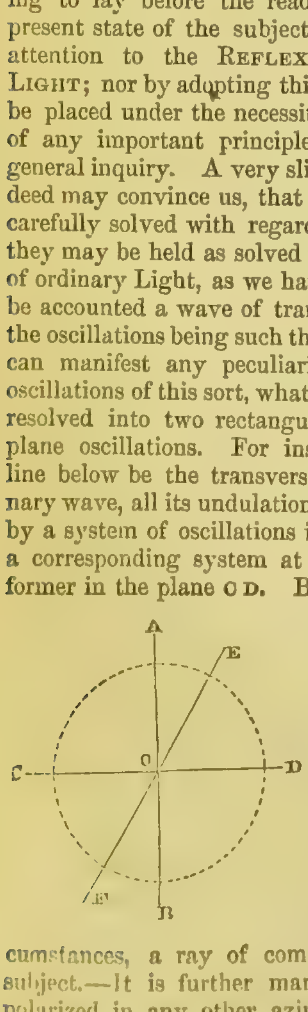
Reciprocals. By means of the theory of polars, already explained, every proposition becomes, as it were, double; that is, it leads immediately to another, called its *reciprocal.* The process by which one proposition is thus deduced from another is called *reciprocation.* The propriety of the name, as well as the general nature of the process itself, will be understood from an example: but the complete power and extent of the method can only be appreciated through its applications to the higher departments of geometry. —Suppose it were required to find the reciprocal of Pascal's theorem; viz., *if a hexagon be inscribed in a circle, the intersections of the opposite sides are three points in one right line.*—Draw tangents at the six vertices of the inscribed hexagon; by this means a circumscribed hexagon is formed, whose three diagonals (that is, lines joining opposite angles) are the polars of



the points of intersection of the opposite sides of the inscribed hexagon; for instance, C′ F′ is the polar of O. Now, as the three points of intersection are in one right line, their polars pass through one point. Hence we have Brianchon's theorem, "*The lines joining the opposite angles of a hexagon circumscribed to a circle, meet in one point.*"—If we examine this process, it will be seen that we have constructed a new figure (the circumscribed hexagon, and its three diagonals), each line in which has for its pole, in respect to the given circle, a corresponding point in the original figure, the tangents having for poles the points of contact. It will also be seen that the original figure stands in exactly the same relation to the new one. The two figures are, therefore, properly called *polar reciprocals;* and the same name is applied to express the relation between their properties. Pascal's and Brianchon's theorems are, then, mutually polar; and either being proved, the other follows as a matter of course.

Reflexion and Refraction. Terms which, in their original significance as applied to Light, comprehended nothing beyond that ancient Law of the Equality of the angles of Incidence and Reflexion, and that no less important Law of

Snell, whose practical applications constitute the two subjects of CATOPTRICS and DIOPTRICS. But circumstances have changed. We have explained under LIGHT, in what manner the *two* phenomena —the existence and direction of a reflected ray, and the existence and direction of a refracted ray—*necessarily* occur when a wave of Light impinges upon the surface of a transparent body. These two rays indeed arise from the resolution of the incident undulation into two others: and it thus has become a rational mathematical inquiry—not merely what are the directions of these rays, but also what are their *intensities?* The problem was first undertaken by Fresnel, and solved by him so satisfactorily, that all the empirical Laws of Brewster and Malus follow from his formulæ as a matter of course. While explaining the process of Fresnel, Mr. Airy in his treatise on the Undulatory Theory, has sufficiently indicated that its foundations are not unaffected by a certain indefiniteness and obscurity; and the subsequent progress of experiment has unfolded truths which these formulæ do not wholly reach.—In attempting to lay before the reader something of the present state of the subject, we shall confine his attention to the REFLEXION OF POLARIZED LIGHT; nor by adopting this restriction, shall we be placed under the necessity of omitting notice of any important principle connected with the general inquiry. A very slight consideration indeed may convince us, that if such problems are carefully solved with regard to polarized Light, they may be held as solved universally. The ray of ordinary Light, as we have already said, must be accounted a wave of transversal oscillation—the oscillations being such that no *side* of the wave can manifest any peculiarity. Now, a set of oscillations of this sort, whatever they are, may be resolved into two rectangular and symmetrical plane oscillations. For instance, if the dotted line below be the transverse section of an ordinary wave, all its undulations may be represented by a system of oscillations in the plane A B, and a corresponding system at right angles to the former in the plane C D. But as these two systems really constitute two equal and rectangular plane-polarized waves, it follows that if we have traced the changes impressed on polarized waves by any circumstances, we may deduce the changes to which, under the same circumstances, a ray of common light would be subject.



—It is further manifest, that as a ray polarized in any other azimuth A O E, may have its vibrations resolved into a set of vibrations along A B, and another along C D, the inquiry on which we are entering altogether resolves itself into consideration of the habitudes of two plane-polarized rays of azimuths 0° and 90°. Further, in the case of *common light*, the two sets of vibrations must be mechanically equivalent,—*i. e.*, if the intensity of the incident wave be 1, each of the two sets of vibrations into which it is resolved must be ½.—These preliminary remarks will make our subsequent investigations abundantly clear.

Reflexion of Polarized Light. This subject naturally divides itself into two parts.

(1.) *Laws of Polarization by Reflexion from the Surfaces of Transparent Uncrystalline Bodies.* —The relations between such reflexions and polarization, according to Brewster's empirical Laws and Fresnel's theory, are the following:—*First*, when a beam of ordinary light falls upon an uncrystalline transparent surface, there is a certain angle of incidence which gives a reflected beam wholly polarized, and the index of refraction peculiar to the substance is the tangent of that angle. *Secondly*, if a unit of light polarized in the plane of incidence be reflected from such a surface as the foregoing, the intensity of the reflected beam will be measured by the square of the fraction,—

$$\text{I} = \frac{\sin(i - r)}{\sin(i + r)} \qquad (1.)$$

And if the incident unit be polarized in a plane perpendicular to the plane of incidence, the intensity of the reflected light will be measured by the square of the fraction

$$\text{I}' = \frac{\tan(i - r)}{\tan(i + r)} \qquad (2.)$$

From which expressions it follows, that if a beam of light be polarized at an azimuth A with the plane of incidence, and if A′ be the azimuth of the polarization of the reflected beam, we have

$$\tan \text{A}' = \tan \text{A} \,.\, \frac{\cos(1 + r)}{\cos(1 - r)}$$

whence

$$\text{Q} = \frac{\tan \text{A}}{\tan \text{A}'} = \frac{\cos(1 - r)}{\cos(1 + r)}, \qquad (3.)$$

In other words, Q must be a constant quantity in all cases, if Fresnel's laws be strictly true.—These formulæ of Fresnel's, as already said, directly yield Sir David Brewster's empirical law; and if they are strictly correct that law should hold universally, varying with different substances, simply in accordance with their refractive index. In the foregoing formulæ, i and r, are the angles of incidence and refraction.—— As pointed out by the Rev. Mr. Haughton, there is an easy means of verifying these formulæ, or of ascertaining whether they strictly represent the phenomena. For instance, we deduce from formula (3.)

$$\tan r = \frac{Q - 1}{Q +} \cot.$$

$$\text{and } \mu = \frac{\sin i}{\sin r}.$$

Now, as the azimuths of an incident and reflected polarized beam A and A′ can be detected in any case by suitable experiment, and as μ, the index of refraction of the reflecting surface, is rigorously determinable by the well-known means, there is no difficulty in the way of ascertaining whether Q is *constant*, and μ, as thus estimated, corresponds with the true refracting index and is also constant. The following table represents one set of careful experiments made by Mr. Haughton:—

Incidence 39° 22′.

A	A′	Q	μ
15°	7° 15′	2·106	1·593
30	14 24	2·249	1·495
45	22 45	2·385	1·421
60	35 39	2·415	1·407
75	56 0	2·517	1·363

The true value of μ in the case in question was 1·6229. Now, not only is Q not constant, and μ neither accurate in amount nor constant, but it is quite clear that the differences are not attributable to errors of observation, inasmuch as Q and μ both vary according to some law. But the fact that the view of reflexion originally entertained must be incomplete, was previously established by many separate experiments. The phenomena of metallic reflexion, indeed (discussed below), had long been familiar, but they were disposed of by being unceremoniously thrust out of the field, or considered a separate and peculiar class; but for years it had been recognized besides, that there were several transparent or translucid substances, for which no angle of complete polarization existed. The discoverers in this direction were Biot, Sir John Herschel, and Sir David Brewster; but it is due to Mr. Dale of Cambridge, to name him as the first inquirer in this country, who, by continuous and systematic labour, called the attention of physicists to the subject. He succeeded in demonstrating that there is no angle of complete polarization in the case of the following substances:—indigo; artificial realgar; the diamond; transparent crystals of sulphate of zinc; translucent glass of antimony; sulphur that had been poured melted on polished zinc; transparent tungstate of lime; carbonate of lead in transparent crystals, limpid as glass; translucid zircon; arsenious acid; idocrase; helvine; labrador; hornblende. And Mr. Dale did not rest with mere isolated experiments. Arranging these substances in the order of their departure from the previously known laws of polarization, he found that this order of departure *bore clear relation to the refringency of the reflecting substances.* The deviations in this case, therefore, appear as clearly to follow a *law*, as the deviations of Q and μ in the table given above; so that both sets of circumstances indicate not only the incompleteness of Fresnel's theory of reflexion, but what is of much more importance, that the formulæ yielded by the complete theory will probably differ from Fresnel's formulæ in this —they will contain co-efficients depending on the magnitude of the index of refraction, and which will be extremely small, unless when that index is large. Nay, it seemed not improbable that no substance could be found capable of polarizing light into a plane under any angle with absolute exactness; the reflected ray appearing plane-polarized, merely because the other portion of the light is in certain circumstances too small to affect the eye. Should this turn out correct, the general phenomenon might be a case of *elliptic polarization*—that most probable of all forms (see POLARIZATION)—the ratio of the axes being some function alike of the angle of incidence and the refringency of the substance.——All doubts, however, regarding these facts and the laws of this interesting department of physical optics, have been finally set at rest, by the recent elaborate and most successful researches of M. Jamin. In his two memoirs in the *Annales de Chimie*, for 1850 and 1851, he has exposed the results of a scrutiny on a hundred different reflecting transparent substances, solid and liquid. The nature of M. Jamin's experimental method we have no room to detail, but the theoretical principle at its foundation was, that elliptic, circular, or rectilineal polarization would ensue according to the *phase* in which the two constituent reflected plane-polarized rays meet after reflexion;—a truth already fully explained under POLARIZATION. The following are M. Jamin's results:— 1. Almost all solid substances polarize light imperfectly by reflexion: meaning that they do not convert a common ray of light at any angle of incidence, into a purely plane-polarized ray.— 2. They transform an incident plane-polarized beam of a given azimuth, into a beam elliptically polarized.—3. If the incident plane-polarized beam be resolved into its two components, and *in perpendicular to* the plane of incidence, the difference of phase in the reflected components will be 180°, 270°, 360°, at the perpendicular, principal and grazing incidences respectively.—4. That the laws of reflexion depend on *two* constants, one, the index of refraction (known to Fresnel), and the other (omitted by Fresnel), the *co-efficient of ellipticity* arising from the reflective power of the body.—5. All substances whose index of refraction is greater than 1·46 accelerate the phase of the component in the plane of incidence.—6. All substances whose index of refraction is less than 1·46 retard the phase of the component in the plane of incidence. —7. All substances whose index of refraction is sensibly equal to 1·46, polarize the reflected light rectilineally, and obey Fresnel's and Brewster's Laws.—These experimental deductions include

every known fact, and they may be termed final. Mr. Haughton of Dublin has offered an admirable memoir in supplement, in which he investigates the position of the major axis of the elliptically polarized beam, and tabulates the ratio of the axes of the ellipse for different conditions of incidence and azimuth of incident plane-polarized light. See *Phil. Magazine* for December, 1854, in which Mr. Haughton has evolved several extremely interesting and important secondary Laws.——The question remains, in how far pure theory can account for the novel aspect thus assumed by the phenomena. The reply is in the highest degree satisfactory. It must be stated of an inquirer of great genius, Mr. Green, that he has given, in the seventh volume of the *Transactions of the Cambridge Society*, formulæ, deduced from theoretical considerations, that meet the whole case—evidently without knowledge of the labours in the same direction of any other Analyst. But while this is cheerfully conceded to a man far too little appreciated, and whose grasp over physical Laws has been surpassed in the case of few recent Englishmen, it is nevertheless impossible to withhold the honour of having surveyed and sounded the whole of this subject, from that remarkable French geometer, M. Cauchy. In 1838, Cauchy communicated to the Institute, with an account of the grounds on which they rest, formulæ of which Fresnel's are only a limited case, and which are so correct and simple, that one might imagine them to have been deduced empirically from Jamin's elaborate experiments. Reference will again be made to the achievements of Cauchy under UNDULATORY THEORY. Meanwhile we may be permitted to express a regret that the general and very lucid account he has himself given of them in volumes vii., viii., and ix. of the *Comptes Rendus*, has not been fully brought before the English student.

(2.) *Metallic Reflexion.*—Previous to that discovery of the exceptional facts in the case of transparent bodies which issued so happily, Sir David Brewster had detected the wholly anomalous character of reflexion from Metallic surfaces, in its bearings on the change indicated by the term polarization. The following propositions were laid down by him as satisfactorily established.—1. If a beam of natural light falls on a metallic mirror, it is not polarized by reflexion at any incidence, but always presents the appearance of a partially polarized ray. Nevertheless there is a particular incidence for which the polarized portion of the light is a maximum. (This proposition is plainly tantamount to saying, that the light reflected from metallic surfaces is elliptically polarized; that the ratio of the axes of the ellipse varies with the angle of incidence; and that there is an angle at which that ratio is a minimum. It thus enters easily as a case of that general phenomenon to which reflexion from transparent bodies also belongs. For the exact law of the changes of the ratio of the axes, see the memoir by Mr. Haughton, already quoted).—2. If a ray polarized in azimuths 0° or 90° be reflected from a metal any number of times, it always remains polarized in the same plane after reflexion.—3. Every ray which, polarized before reflexion, is plane-polarized in any other azimuth, loses its plane-polarization after it has undergone the action of the metal. (It becomes elliptically polarized).—4. When plane-polarized light is reflected any number of times from parallel metallic mirrors, at the incidence of maximum polarization, the polarization is restored after an *even* number of reflections.—5. Finally, the reflected beam becomes again polarized after an even or uneven number of reflexions, under a great number of incidences, determined by laws which remain to be found.——This subject of Metallic Reflexion was the first that occupied the attention of M. Jamin. The careful student will readily detect that there is much entirely in common between this class of phenomena and the class relating to transparent bodies. M. Jamin found the key to the proposition regarding depolarization, likewise as before in the alteration of *phase* resulting after reflexion, in the two rays polarized in azimuths 0 and 90°,—into which two any ray plane-polarized at other azimuths can be resolved. This alteration of phase will produce, generally, an elliptic polarization; and the previous plane-polarization will evidently be restored after as many reflexions as shall make the retardation of phase amount to an entire vibration. Not a shade of mystery, in fact, now rests on the subject; and every conclusion of Jamin's experiments, in this instance also, finds its counterpart in Cauchy's formulæ. The original memoir of Jamin is given in part 17th of Taylor's *Repertory*.—Jamin concludes his memoir by researches, and a satisfactory solution of the *colour of metallic surfaces* in various circumstances. We regret we have nothing more in our power than to refer the reader to the original dissertation.——It is scarcely necessary to draw attention to the fact, that, in the phenomena of Reflexion, we have another singular instance of the grasp of the Undulatory Theory of Light. The formulæ of Cauchy were evolved long prior to the discovery of the facts; yet although these facts as they gradually came up, appeared anomalous as well as intricate, we have found the Law of them all, in the deductions or even previsions of the Analyst. See APPENDIX, article PHASE.

Reflexion Total. When a ray enters from a rare into a dense medium, the ray within the new medium is nearer the perpendicular through the point of incidence than the incident ray; and when a ray passes from a dense into a rare medium the reverse holds. It will readily appear that, in the latter case, as the angle of the intrant ray with the perpendicular, increases, the angle of the emitted ray will come to coincide

with the horizontal line; and that if the former angle is farther augmented, there will be no emitted ray at all, but one bent back within the dense medium. In other words, the whole ray is reflected at the second surface of the medium —a phenomenon named *total reflection.* This fact has many important practical applications. For instance, if one desires to alter the direction of a ray of light, it may be done by use of this principle of total reflection, with scarcely any loss of light. The same change of direction could not be effected by means of a metallic mirror without a considerable loss of light. Hence the great value of prisms of total reflection in Newtonian reflecting telescopes.

Refraction. The Law of the *direction* of the refracted ray, in cases of single or simple refraction, has already been fully stated; and, in various articles, the applications of that law have been unfolded. The relations of this phenomenon with the phenomenon of Reflexion, as regarded by the Undulatory Theory, have also been explained. Inquiries remain—sufficiently interesting—respecting the *intensity*, &c., of the refracted ray. The principles at their root, however, are similar to those unfolded in the previous article; on which account as well as our want of space, we must refer the reader, desirous to pursue the subject, to systematic treatises on Physical Optics,—restricting ourselves here to a notice of the more remarkable manifestations by Crystalline bodies when employed as refringent substances.

I. Refraction Double. This curious subject is divisible into three parts.

(1.) *The Phenomena.*—We have explained under Mineralogy Optical, that there are three great classes of crystals. The *first*, or those whose fundamental form is the cube, have one mathematical axis; or rather their three rectangular axes of form, are all equal: The *second*, the rhombohedral and pyramidal systems, in which two of the rectangular axes of form are unequal: And the *third*, belonging to one or other of the prismatic systems, in which these three rectangular axes of form are all unequal. It was mainly to the unceasing industry and acuteness of Sir David Brewster that science owed the discovery of the intimate connection between this classification of crystalline forms, and the phenomena of double refraction. Crystals, of the first class whose three axes are equal, yield no double image: they act as simple transparent bodies, and produce one refracted ray. Both of the other classes yield two images, only in quite different ways. Crystals of the second form—of which Iceland spar is the most available and instructive specimen—act in the following manner. In most positions of the crystal with regard to the incident light, two beams are produced, one of which—the *ordinary ray*—obeys the law of Snell,—*i. e.*, it is in the same plane with the incident ray, and the sine of the angle of incidence has to the sine of the angle of refraction a constant ratio, for the same medium. The second beam does not generally obey either of these conditions, that is, it is not always in the plane of the incident ray, and it disregards the law of the sines: this ray is called the *extraordinary ray.* Now in the class of crystals of which we are speaking there is one line, or rather one direction, termed the *optic axis.* In the rhomb of Iceland spar it is the line joining the opposite obtuse solid angles: all the faces of either extremity making equal angles with this line. As however the optic properties are the same for lines passing, in this same fashion, through all elementary and symmetrically placed rhombohedrons, this optic axis is manifestly a *direction in space* and not a mere *line.* Now if we polish an artificial face on the crystal perpendicular to this optic axis, and cause a ray of light to fall perpendicularly on that face, there is no double refraction—the ordinary ray passes straight through—without deviation, and the extraordinary ray vanishes. For every other angle of incidence there are two rays; but in this case the extraordinary ray obeys one-half of the law of simple refraction—it is always in the plane of incidence. The ratio of the sines however in the case of the extraordinary ray is not constant for all incidences: on the contrary, it diminishes as the inclination of the ray to the optic axis increases; being least of all when the ray is perpendicular to the axis. This least value of the ratio, is called the *extraordinary index.* If a plane were cut *parallel* to the optic axis, and a ray made to fall on the crystal at any angle in a plane perpendicular to that face, the two images would again emerge in the plane of incidence, and at all angles of incidence the ratio of the sines of incidence and refraction would, in both rays, be found constant: *i. e.*, the *extraordinary index* just referred to, in the case of the extraordinary ray, which thus obeys both the laws of ordinary refraction. The direction of that ray in all circumstances will be given in a subsequent paragraph. The optic axis around which the phenomena of double refraction are evidently grouped, is clearly the *axis of form*—or the line around which the whole rhomboid is symmetrical. It would seem at first sight reasonable to suppose that the *elasticity* of the crystal is either greater or less along this line than along any others, and that it is equal along all directions perpendicular to it; so that we have, at outset, a significant hint as to the probable cause of double refraction.—Turning now to crystals of the prismatic order—an order comprehending by far the larger number of crystals, we detect there no single axis of form; in fact there is no single line around which their forms are symmetrical. It was reserved for Sir David Brewster to discover that these crystals—such as *arragonite*, *mica*, *sulphate of barytes*, *sulphate of lime*, *topaz*, and *felspar*—have *two optic axes*, and are therefore termed *bi-axial crystals.* These two optic axes have relations to the incident and the refracted twofold ray,

quite analogous to the relations discerned in the case of Iceland spar; and we owe to our distinguished countryman the farther most important fact, that if two lines be drawn, one bisecting the acute and the other the obtuse angle between the optic axes, these lines (together with a *third* at right angles to both) are closely connected with the primitive form of the crystal;—they are indeed the fundamental lines of the physical theory of double refraction—the two former being those in which the elasticity of the vibrating medium is greatest and least. Neither of the rays in the case of biaxial crystals is an ordinary ray. Both are refracted in an extraordinary manner and according to a new law.

(2.) *Theory of the Phenomena.*—We must be satisfied with the simplest practicable explanation of the principle by which the phenomena of double Refraction are accounted for. The beautiful geometrical construction by which Huyghens represented the direction of the ordinary and extraordinary rays in a rhomb of Iceland spar, is as follows. Let A C be the incident ray and C F the section of the surface of the crystal made in *the plane of incidence.* Produce the incident ray to any point B, and from B raise the perpendicular B F. Find C D, so that

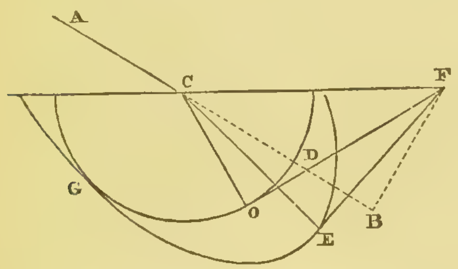


Fig. 1.

Sine incidence: Sine refract: = C B : C D

in the case of the ordinary ray. With radius C D describe a circle; and imagine a *sphere* existing in space, of which that circle is a section. Next imagine a spheroid of revolution existing in the same space whose centre is C, its axis of revolution in the direction of the optic axis of the crystal, and equal to the diameter of the sphere; the other axis being found by the proportion.

Ord. index: Extraord. index = C O: semi-axis.

The surfaces of these two solids—the sphere and the spheroid—will represent the surfaces of the ordinary and extraordinary waves propagated within the crystal. The remaining part of the construction follows as a matter of course. At F imagine a line drawn perpendicular to the plane of the diagram. Through that line draw two tangent planes, one to touch the sphere and the other to touch the spheroid. The point of contact, O, in the former case will be in the plane of the diagram, and C O will be the *ordinary* ray. The point of contact E with the spheroid may not be in that plane, but wherever it is *in space*, the line C E, will give the direction of the *extraordinary* ray. This law of Huyghens was found true with regard to all uni-axial crystals: but, so soon as the researches of Sir David Brewster opened the vast and varied field of biaxial crystals, the construction proved to be a restricted one, as one at least of the rays in these crystals seemed to follow some new and unknown law. The mystery however soon yielded to the analysis of Fresnel,—an analysis which, in this case also, has outstripped observation and suggested predictions afterwards verified by experiment. The student must not expect to apprehend the nature of this theory without a certain expenditure of thought; nevertheless, if at each step he realizes the actual *mechanical conditions* referred to, he will satisfactorily follow the general account of it which we give in the very lucid words of Professor Lloyd.—"Fresnel starts from the supposition that the elastic force of the vibrating medium within every crystal, is in general different in different directions. This is, in fact, the most general supposition that can be made; and whether one suppose that the vibrating medium is the ether within the crystal or that the molecules of the body itself partake of the vibratory movement, there will be obviously such a connection and mutual dependence of the parts of the solid and those of the medium in question, that we cannot hesitate to admit for the one what has been already established on the clearest evidence for the other.—It is easy to see, generally, that the phenomenon of double refraction is a necessary consequence of this hypothesis and of the principle of transversal vibrations.—Let us take, for example, the simple case of a beam of light proceeding from an infinitely distant point, and falling *perpendicularly* on the surface of a uni-axial crystal, cut *parallel to the axis.* The incident wave being plane and parallel to the surface of the crystal, the vibrations are also parallel to the same surface; and we may conceive them to be composed of vibrations *parallel* and *perpendicular* to the axis of the crystal. Now, the elasticity brought into play by these two sets of vibrations being different, they will be propagated with different velocities; and there will be two *waves* within the crystal *oppositely polarized.* If the second face of the crystal be *parallel* to the first, the two rays will emerge perpendicularly; and the only effect produced will be, that one will be *retarded* more than the other, in its progress through the crystal. But if the second face be *oblique* to the direction of the rays, they will be both *refracted* at emergence and *differently;* and they will therefore diverge from one another.—To return to the general theory. Let us suppose a disturbance to be produced in a medium such as we have been considering, and any particle of the medium displaced from its position of rest. The resultant of all the elastic forces which resist the displacement will not, in general, act in the direc-

tion of the displacement (as would be the case in a medium uniformly elastic), and therefore will not drive the displaced particle directly back to its position of equilibrium. Fresnel has shown, however, that there are three directions at right angles to each other, in every medium of this nature, in any of which if the particles are displaced, the elastic forces do act *in the direction of the displacement*, whatever be the nature or laws of the molecular action; and the only supposition which he makes is, that these three directions are *parallel* throughout the crystal. In fact, the first principles of crystallization compel us to admit that the arrangement of the molecules of the crystalline body is similar in all parallel lines throughout the crystal; and the same property must belong to the ether within it, if (as we have every reason to presume) its elasticity be dependent on that of the crystal itself. These three directions Fresnel denominates *axes of elasticity;* and he thinks that they must also be axes of symmetry, with respect to the crystalline form. —If, on each of these axes, and on every line diverging from the same origin, portions be taken which are as the square roots of the elastic forces in their directions, the locus of the extremities of these portions will be a surface, which Fresnel denominates the *surface and elasticity.* Its equation is

$$r^2 = a^2 \cos^2 \alpha + b^2 \cos^2 \beta + c^2 \cos^2 \gamma;$$

a^2, b^2, c^2, being elasticities in the directions of the three axes, r the radius-vector of the surface and α, β, γ, the angles which it makes with the axes.—This surface determines the *velocity of the propagation of the wave*, when the direction of its vibration is given. For, suppose the particle to vibrate in the direction of any radius-vector, r, the elastic force which governs its vibration will be proportional to r^2; and, since the velocity of wave propagation in any elastic medium is as the square root of the elastic force, it must in this case be represented by the radius-vector of the surface of elasticity in the direction of the vibrations. Hence, if we conceive the vibration in the incident wave to be resolved into two within the crystal, performed in two determinate directions, these will be propagated with different velocities; and, as a difference of velocity gives rise to a difference of refraction, it follows that the incident ray will be divided into two within the crystal, which will in general pursue different paths. Thus, the bifurcation of a ray, on entering a crystal, presents no difficulty, provided we can explain in what manner the vibration comes to be resolved. —To see in what manner this takes place, let us conceive a plane wave advancing within the crystal. By the principle of transversal vibrations, the movements of the etherial molecules are all parallel to the wave; but the motion of each molecule, when thus removed from its position of equilibrium, is resisted by the elastic force of the medium; and that force is, in general, oblique to the direction of the displacement. If the plane containing the direction of the force and that of the displacement were *normal to the plane of the wave,* the force would be resolvable into two,—one perpendicular to the plane of the wave, which, by the principle of transversal vibrations, can produce no effect; and the other in the direction of the displacement itself, which will thus be communicated from particle to particle without change. But this, in general, is not the case. Fresnel has shown, however, that in all cases, the displacement may be resolved in two directions in the plane of the wave, at right angles to one another, such that the elastic force called into action by each component will be in the plane passing through the component and normal to the wave; and thus each component will give rise to a wave in which the direction of the vibrations is preserved, and which therefore will be propagated with a constant velocity.—The two directions above alluded to are those of *the greatest and least diameters of the section of the surface of elasticity* made by the plane of the wave; so that if the original displacement be resolved into two, parallel to these directions, each component will give rise to a plane wave, in which the vibrations will preserve constantly the same direction. The velocity of propagation being represented by the radius-vector of the surface of elasticity in the direction of the displacement, the velocities of the two parts of the wave will be proportional to the *greatest* and *least diameters* of the section of the surface of elasticity, to which the vibrations are parallel. Thus it appears that an incident plane wave, in which the vibrations are in any given direction, will be resolved into two within the crystal; and these will be propagated with *different velocities*, and consequently assume *different directions.* The vibrations in these waves being *parallel to two fixed lines*,—namely, the greatest and least diameters of the section of the surface of elasticity, —it follows that the two rays are *polarized*, and that their planes of polarization are at *right angles*, being the planes passing through the direction of the ray and these two lines. From this it follows that the plane of polarization of one of the rays bisects the dihedral angle made by the two planes, which pass through the *normal to the wave*, and the *normals to the two circular sections* of the surface of elasticity; and that the plane of polarization of the other is perpendicular. This coincides with the rule previously given by M. Biot, namely, *that the plane of polarization of one of the pencils bisects the dihedral angle formed by planes drawn through the ray and the two optic axes; while that of the other is perpendicular, or bisects the supplemental dihedral angle.*—Thus we see that the two fundamental facts of crystalline refraction,—namely, the bifurcation of the ray and the opposite polarization of the two pencils,—are completely accounted for.—Further, the amplitudes of the resolved vibrations are represented

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by the co-sines of the angles which the direction of the original vibration contains with the two fixed rectangular directions; and, as the squares of these amplitudes measure the intensities of the two pencils, the law of Malus respecting these intensities is a necessary consequence."—We must again refer the student very earnestly to these *Lectures* by Professor Lloyd.

(3.) The optical phenomena of double refraction are thus resolved into immediate dependence on the mechanical axes of elasticity; *so that whatever changes the one must affect the other also.* In signal confirmation of this view, are the multitudes of experiments now opening upon us, showing that the bi-refringent quality can be impressed on any naturally isotropic substance, if by partial application of *heat*, or by application of *mechanical pressure or traction*, we affect its isotropism, or impress upon it an unequal elasticity in different directions. The fullest discussion on the influences of pressure or traction is contained in a phamphlet by M. Wertheim, of which a copious account will be found in the *Philosophical Magazine* for October and November, 1854. The experiments and deductions of the writer are equally worthy of regard, and he has even succeeded in educing practical applications from one of the most refined and remote inquiries in physical science. For instance, from the simple phenomena of colours evolved by pressure, or by aid of what he has termed the *Chromatic Dynamometer*, he obtains immediately, without employing any co-efficient of correction, the effective pressure of a vice, press, hydraulic press, lever, &c., and the ratio between the useful and theoretic effect of any machine. See farther the researches of Brewster, Biot, and Mitscherlich. The latter inquirer has especially and laboriously analyzed the effect of heat on bi-refringent crystals of both classes, and detected a number of curious laws. He has succeeded in impressing so large a change on some bi-axial crystals, that the plane of the two optic axes has reached a position perpendicular to its primitive plane. The elasticities of all crystals affected by heat, change in a different ratio along the axes of mechanical elasticity; and the plane of the optical axes must therefore change also. Valuable papers on the elasticity of crystals, by Professor Rankine, and Professor Maxwel, have recently been printed in the *Transactions* of the Royal Societies of London and Edinburgh.

II. Conical Refraction. — Considering the origin of the class of phenomena of which we have treated, it must be sufficiently manifest, that Inquiry might be expected to bring incessantly under review novel and delicate results in great numbers. Several of these, such as Circular Double Refraction, we must leave unnoticed; but there is one result so very remarkable, alike in its own nature and the manner of its discovery, that it cannot be passed by. Reverting to the construction of Huyghens, it will be seen that the directions of the rays through the bi-refringent crystal, are determined by the points of contact of tangent planes, with the surfaces of the spherical and spheroidal waves. Now, these contacts are *points*, and it seemed natural to expect, that, in every case of double refraction, the contacts would be points also; so that we should always have one or two definite emergent rays. But, Sir William R. Hamilton, while considering the case of bi-axial crystals, reached the unexpected conclusion, that, in certain circumstances, the contact would not be mere points. The efficient, or determining contact in these special cases, is that of *four planes*, and he discerned that they could not touch the surface of the wave in two points, but in an infinity of points, constituting what may be termed a small circle of contact. No conclusion could have been more unexpected, yet it followed rigorously from the theory of Fresnel; and Sir William did not hesitate to assert, that, in the given circumstances, we should have, not two emergent rays, but a *cone of rays diffused from a point*, and manifesting themselves in the form of a luminous circle. Certainly the history of gravitation itself records no case of a purer prediction; for no phenomenon, in the remotest degree akin to it, had ever been noticed or anticipated. The experimental verification was speedily accomplished at Sir William's entreaty, by Professor Lloyd. Two separate classes of phenomena were predicted by Hamilton, named by him *exterior* and *interior* conical refraction: the ray, in the one case, issuing as a cone with its vertex at the surface of emission; and, in the second case, issuing as a cylinder, having been converted on entering the crystal into a cone, whose vertex is at the point of incidence.—(1.) The first or *exterior* phenomenon, ought to manifest itself when a ray passes through a crystal, along one of the lines of *equal velocity*,—lines, that is, along which the two sets of waves move with equal velocities. Mr. Lloyd obtained a plate of *arragonite*, in which these two lines (nearly coincident with the two optical axes), include an angle of 20°. The plate was cut with faces perpendicular to the line dividing the angle of the two optic axes into equal parts. If O P, therefore be drawn perpendicular to the faces of the arragonite, and O M, O N drawn, so that M O P and P O N be angles of 10°, then the ray passing from M to O, or from O to M (and, in the same way with P), ought to issue conically refracted. Mr. Lloyd received that ray, and found that instead of the two usual rays of double refraction, it presented

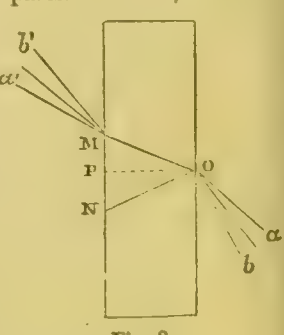


Fig. 2.

the luminous circle below! (2.) The phenomena of interior conical refraction were verified and established as facts, with equal success: also by the care of Professor Lloyd. We may conclude in the words of M. Plucker: "No physical experiment has made such an impression upon me as that of conical refraction. A ray of light entering a crystal, and issuing as a luminous cone, was a thing unheard of, and without analogy. Sir W. Hamilton announced the fact as a consequence of a form of the wave which he had deduced by long calculations from an abstract theory. I confess I felt on the rack to learn if a result so extraordinary could be confirmed by experiment,—a result predicted by the theory recently erected by the genius of Fresnel. And now that Mr. Lloyd has demonstrated that the facts are in perfect correspondence with Sir William Hamilton's expectations, every prejudice against a Theory so marvellously confirmed, ought surely to disappear."

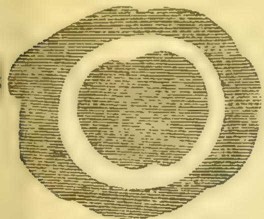


Fig. 3.

Refraction Atmospheric. The mode in which a ray of light is bent by refraction as it passes through our atmosphere, has been already explained generally under DIOPTRICS; and MIRAGE refers to instances in which irregular variations in the atmosphere have been known to impress upon the rays passing through them very curious effects. The question, however, as to the regular or normal influence of our aerial envelope on the direction of light, is of vastly greater consequence; for until we have obtained a reply to it, we cannot assure ourselves how far the apparent position of any body, seen through the atmosphere, is apart from its real position. Uncertainty of this sort remaining, would evidently affect with serious error our conclusions alike as to the true places of the stars, and to the true altitude of any terrestrial object.—(1.) *The Theory of Astronomical Refractions*, or the principles on the ground of which we must apply a correction to the observed places of the heavenly bodies has long attracted profound attention. It is a most arduous problem—one that in all likelihood will never be completely solved. The standing and intractable difficulty is this—*we do not know the exact constitution of the atmosphere through which the ray passes.* It is satisfactorily established indeed, that the refringent power of common air is proportional to its density, so that we can readily obtain a constant that will enable us to deduce the amount of that power for any ordinary density; and fortunately the consideration of one very variable element of the atmosphere may be omitted, inasmuch as the variation of refringent power incident on the presence of humidity, is almost entirely compensated by the concomitant variation of density. But what shall we say regarding the arrangement of the atmosphere in reference to our globe's surface? The answer universally given in these investigations, is in the form of an *assumption*, viz., that the atmosphere is arranged with its surfaces of equal density, *spherical and concentric to the earth*, and therefore that the differential of refraction is a function of the density and distance from the centre. The inquiry remains, What is the relation between these two variables, viz., the density of concentric strata, and their altitude or distance from the earth's centre? This relation is utterly unknown, and the equation cannot be integrated unless it is known. Assumptions are again resorted to; sometimes a law is taken for granted, because of its supposed conformity with known physical laws; sometimes one is assumed, because of the facilities it affords for integration. Besides these uncertainties however, it may further be confidently asserted, that the original or fundamental assumption is not true. Near the earth, surfaces of equal atmospheric temperature and density depend on the figure of the ground; varying even in contiguous localities, according to the structure of the soil—whether it be covered by forests, large bodies of water, &c., &c. Such irregularities must extend at least as far as the region of the clouds—about six miles high; and this is the portion of the atmosphere which most powerfully acts on the ray of light. Happily these and other disturbing causes produce no appreciable effect when the external object is *near the zenith;* and it is exceedingly fortunate, that through the multiplication of Observatories in different latitudes, so large a portion of the sky is now within neighbourhood of the zeniths of the best Instruments. Each Observatory has thus its peculiar and appropriate range of sky—a range within which its determinations must in all delicate inquiries be accounted of greatest weight. It must not be supposed, however, that analysts have succumbed under the embarrassments now enumerated. Since the period of Newton—(who seems to have discerned the law that until we reach low altitudes the refraction is sensibly proportional to the tangent of the zenith-distance)—remarkable skill and industry have been expended on the construction of acceptable theories and convenient tables; and with almost unexpected success. The places of stars within 70° of the zenith of an Observatory can now be corrected by aid of these tables, to a precision so great that the errors of single determinations due to incalculable irregularities, may reasonably be expected to be eliminated in the *mean* of a sufficient number; and corrections of the places of objects considerably nearer the horizon may be much relied on, if they have been observed under favourable circumstances.

(*The reader will now turn to page 650.*)

Dr. Robinson's Tables of Refraction.

Table I.

μ = 57″546 ; ther. = 50° bar. = 29·60 inches.

T.	A.	B.	T.	A.	B.
0	0·33343$_{94}$		47	0·29126$_{85}$	+ 11
1	0·33249$_{94}$		48	0·29041$_{85}$	+ 7
2	0·33155$_{94}$		49	0.28956$_{84}$	+ 3
3	0·33061$_{94}$		50	0·28872$_{85}$	0
4	0·32968$_{94}$		51	0·28787$_{84}$	— 3
5	0·32874$_{93}$		52	0.28703$_{85}$	— 7
6	0·32781$_{93}$		53	0.28618$_{84}$	— 11
7	0·32688$_{93}$		54	0·28534$_{85}$	— 15
8	0·32595$_{92}$		55	0·28449$_{84}$	— 19
9	0·32503$_{92}$		56	0·28365$_{84}$	— 23
10	0·32411$_{92}$		57	0·28281$_{84}$	— 27
11	0·32319$_{92}$		58	0·28197$_{84}$	— 31
12	0 32227$_{92}$		59	0·28113$_{83}$	— 35
13	0·32135$_{91}$		60	0·28030$_{83}$	— 39
14	0·32044$_{91}$		61	0·27947$_{83}$	— 42
15	0·31953$_{91}$		62	0·27864$_{83}$	— 46
16	0·31862$_{91}$		63	0·27781$_{83}$	— 50
17	0·31771$_{91}$		64	0·27698$_{82}$	— 54
18	0·31680$_{91}$		65	0·27616$_{82}$	— 58
19	0.31589$_{90}$		66	0·27534$_{83}$	— 62
20	0·31499$_{90}$	+ 117	67	0·27451$_{82}$	— 66
21	9.31409$_{90}$	+ 113	68	0·27369$_{82}$	— 70
22	0·31319$_{89}$	+ 109	69	0·27287$_{82}$	— 74
23	0.31230$_{90}$	+ 105	70	0·27205$_{82}$	— 78
24	0·31140$_{89}$	+ 101	71	0·27123$_{81}$	— 81
25	0·31051$_{90}$	+ 97	72	0·27042$_{81}$	— 85
26	0·30961$_{89}$	+ 93	73	0·26961$_{81}$	— 89
27	0.30872$_{89}$	+ 89	74	0·26880$_{81}$	— 93
28	0·30783$_{89}$	+ 85	75	0·26799$_{81}$	— 97
29	0·30694$_{88}$	+ 81	76	0·26718$_{81}$	— 101
30	0·30606$_{89}$	+ 78	77	0 26637$_{80}$	— 105
31	0·30517$_{88}$	+ 74	78	0.26557$_{81}$	— 109
32	0 30429$_{88}$	+ 70	79	0·26476$_{80}$	— 113
33	0·30341$_{88}$	+ 66	80	0·26396$_{80}$	— 117
34	0·30253$_{88}$	+ 62	81	0 26316$_{80}$	— 121
35	0·30165$_{87}$	+ 58	82	0·26236$_{80}$	— 125
36	0 30078$_{87}$	+ 54	83	0·26156$_{80}$	— 129
37	0·29991$_{87}$	+ 50	84	0·26076$_{80}$	— 132
38	0·29904$_{87}$	+ 46	85	0·25996$_{80}$	— 136
39	0·29817$_{87}$	+ 42	86	0·25916$_{79}$	— 140
40	0 29730$_{87}$	+ 39	87	0·25837$_{79}$	— 144
41	0·29643$_{86}$	+ 35	88	0·25758$_{79}$	— 148
42	0 29557$_{86}$	+ 31	89	0·25679$_{79}$	— 152
43	0.29471$_{86}$	+ 27	90	0·25600$_{79}$	— 156
44	0·29385$_{87}$	+ 23	91	0·25521$_{79}$	— 160
45	0·29298$_{86}$	+ 19	92	0·25442	— 163
46	0·29212$_{86}$	+ 15			

Refraction = R′ C — D × (T — 50°) — E × barometer — 29·60) log. R′ = A + B + log. taug. app. zen. dist. + log bar.

Argument of A. external thermometer = T.

Argument of B, attached thermometer.

Argument of C, D, and E, apparent zenith distance.

TABLE II.

Z. D.	C.	D.	E.	Z. D.	C.	D.	E.
4°	0·01			80° 10′	$12{\cdot}05_{29}$	0·003	0·36
10	$0{\cdot}01_{1}$			15	$12{\cdot}34_{30}$	0·004	0·37
15	$0{\cdot}02_{1}$			20	$12{\cdot}64_{31}$	0·004	0·38
20	$0{\cdot}03_{1}$			25	$12{\cdot}95_{33}$	0·004	0·39
25	$0{\cdot}04_{1}$			30	$13{\cdot}28_{33}$	0.004	0·40
30	$0{\cdot}05_{2}$			35	$13{\cdot}61_{35}$	0·004	0·41
35	0.07_{3}			40	$13{\cdot}96_{35}$	0·004	0·42
40	$0{\cdot}10_{5}$			45	$14{\cdot}31_{36}$	0·004	0·43
45	$0{\cdot}15_{1}$			50	$14{\cdot}67_{38}$	0·004	0·44
46	$0{\cdot}16_{1}$			55	$15{\cdot}05_{40}$	0·004	0·45
47	$0{\cdot}17_{1}$			81 0	$15{\cdot}45_{41}$	0·004	$0{\cdot}46_{2}$
48	$0{\cdot}18_{1}$			5	$15{\cdot}86_{42}$	0·004	$0{\cdot}48_{1}$
49	$0{\cdot}19_{1}$			10	$16{\cdot}28_{44}$	0·004	$0{\cdot}49_{1}$
50	$0{\cdot}20_{1}$		0·01	15	$16{\cdot}72_{45}$	0·004	$0{\cdot}50_{2}$
51	$0{\cdot}21_{2}$		0·01	20	$17{\cdot}17_{47}$	0·004	$0{\cdot}52_{1}$
52	$0{\cdot}23_{2}$		0·01	25	$17{\cdot}64_{48}$	0·005	$0{\cdot}53_{1}$
53	$0{\cdot}25_{2}$		0·01	30	$18{\cdot}12_{50}$	0·005	$0{\cdot}54_{2}$
54	$0{\cdot}27_{2}$		0·01	35	$18{\cdot}62_{52}$	0·005	$0{\cdot}56_{2}$
55	$0{\cdot}29_{3}$		0·01	40	$19{\cdot}14_{54}$	0·005	$0{\cdot}58_{1}$
56	$0{\cdot}32_{3}$		0·01	45	$19{\cdot}68_{56}$	0·005	$0{\cdot}59_{1}$
57	$0{\cdot}35_{4}$		0·01	50	$20{\cdot}24_{58}$	0·005	$0{\cdot}60_{3}$
58	$0{\cdot}39_{4}$		0·01	55	$20{\cdot}82_{60}$	0·005	$0{\cdot}63_{1}$
59	$0{\cdot}43_{4}$		0·01	82 0	$21{\cdot}42_{63}$	0·005	$0{\cdot}64_{2}$
60	$0{\cdot}47_{5}$		0·01	5	$22{\cdot}05_{65}$	0·005	$0{\cdot}66_{2}$
61	$0{\cdot}52_{6}$		0·02	10	$22{\cdot}70_{68}$	0·006	$0{\cdot}68_{2}$
62	$0{\cdot}58_{7}$		0·02	15	$23{\cdot}38_{70}$	0·006	$0{\cdot}70_{3}$
63	$0{\cdot}65_{7}$		0·02	20	$24{\cdot}08_{73}$	0·006	$0{\cdot}73_{2}$
64	$0{\cdot}72_{8}$		0·02	25	$24{\cdot}81_{76}$	0·006	$0{\cdot}75_{2}$
65	$0{\cdot}80_{11}$		0·03	30	$25{\cdot}57_{78}$	0·006	$0{\cdot}77_{2}$
66	$0{\cdot}91_{12}$		0·03	35	$26{\cdot}35_{83}$	0·006	$0{\cdot}79_{3}$
67	$1{\cdot}03_{14}$		0·03	40	$27{\cdot}18_{85}$	0·007	$0{\cdot}82_{3}$
68	$1{\cdot}17_{17}$		0·04	45	$28{\cdot}03_{89}$	0·007	$0{\cdot}85_{2}$
69	$1{\cdot}34_{21}$	0·000	0·04	50	$28{\cdot}92_{93}$	0·007	$0{\cdot}87_{3}$
70	$1{\cdot}55_{25}$	0·001	0·05	55	$29{\cdot}85_{97}$	0·007	$0{\cdot}90_{3}$
71	$1{\cdot}80_{29}$	0·001	0·06	83 0	$30{\cdot}82_{1\cdot00}$	0·008	$0{\cdot}93_{3}$
72	$2{\cdot}09_{39}$	0·001	0·06	5	$31{\cdot}82_{1\cdot06}$	0·008	$0{\cdot}96_{3}$
73	$2{\cdot}48_{49}$	0·001	0·08	10	$32{\cdot}88_{1\cdot10}$	0·008	$0{\cdot}99_{4}$
74	$2{\cdot}97_{62}$	0·001	0·09	15	$33{\cdot}98_{1\cdot15}$	0·009	$1{\cdot}03_{3}$
75	$3{\cdot}59_{78}$	0·001	0·11	20	$35{\cdot}13_{1\cdot19}$	0·009	$1{\cdot}06_{4}$
76	$4{\cdot}37_{32}$	0·001	0·13	25	$36{\cdot}32_{1\cdot24}$	0·010	$1{\cdot}10_{4}$
76 20′	$4{\cdot}69_{35}$	0·002	0·14	30	$37{\cdot}56_{1\cdot31}$	0·010	$1{\cdot}14_{4}$
40	$5{\cdot}04_{38}$	0·002	0·15	35	$38{\cdot}87_{1\cdot37}$	0·011	$1{\cdot}18_{4}$
77 0	$5{\cdot}42_{42}$	0·002	0·16	40	$40{\cdot}24_{1\cdot42}$	0·012	$1{\cdot}22_{5}$
20	$5{\cdot}84_{47}$	0·002	0·18	45	$41{\cdot}66_{1\cdot50}$	0·013	$1{\cdot}27_{4}$
40	$6{\cdot}31_{52}$	0·002	0·19	50	$43{\cdot}16_{1\cdot57}$	0·013	$1{\cdot}31_{5}$
78 0	$6{\cdot}83_{28}$	0·002	0·21	55	$44{\cdot}73_{1\cdot64}$	0·014	$1{\cdot}36_{5}$
10	$7{\cdot}11_{30}$	0·002	0·21	84 0	$46{\cdot}37_{1\cdot72}$	0·015	$1{\cdot}41_{6}$
20	$7{\cdot}41_{31}$	0·002	0·22	5	$48{\cdot}09_{1\cdot80}$	0·016	$1{\cdot}47_{6}$
30	$7{\cdot}72_{33}$	0·002	0·23	10	$49{\cdot}89_{1\cdot89}$	0·018	$1{\cdot}53_{6}$
40	$8{\cdot}05_{35}$	0·003	0·24	15	$51{\cdot}78_{1\cdot99}$	0·019	$1{\cdot}59_{6}$
50	$8{\cdot}40_{36}$	0·003	0·25	20	$53{\cdot}77_{2\cdot09}$	0·022	$1{\cdot}65_{7}$
79 0	$8{\cdot}76_{39}$	0·003	0·26	25	$55{\cdot}86_{2\cdot20}$	$0{\cdot}023_{2}$	$1{\cdot}72_{7}$
10	$9{\cdot}15_{42}$	0·003	0·28	30	$58{\cdot}06_{2\cdot31}$	$0{\cdot}025_{3}$	$1{\cdot}79_{8}$
20	$9{\cdot}57_{44}$	0·003	0·29	35	$60{\cdot}37_{2\cdot45}$	$0{\cdot}028_{3}$	$1{\cdot}87_{8}$
30	$10{\cdot}01_{46}$	0·003	0·30	40	$62{\cdot}82_{2\cdot58}$	$0{\cdot}031_{4}$	$1{\cdot}95_{9}$
40	$10{\cdot}47_{49}$	0·003	0·31	45	$65{\cdot}40_{2\cdot71}$	$0{\cdot}035_{4}$	$2{\cdot}04_{9}$
50	$10{\cdot}96_{53}$	0·003	0·33	50	$68{\cdot}11_{2\cdot89}$	$0{\cdot}039_{5}$	$2{\cdot}13_{10}$
80 0	$11{\cdot}49_{28}$	0·003	0·35	55	$71{\cdot}00_{3\cdot06}$	$0{\cdot}014_{6}$	$2{\cdot}23_{11}$
5	$11{\cdot}77_{28}$	0·003	0·35	85 0	74·06	0·050	2·34

For an analysis and judicious appreciation of the procedures of Bradley, Laplace, Ivory, and Bessel, we must refer the student to recent memoirs by Biot—memoirs drawn from this veteran physicist by a discussion during the year 1855, within the French Academy of Sciences. It were easy to print in this place the formulæ arrived at by these Analysts; but as our space would not admit of due explanations, it appears needless. The foregoing tables—printed by permission of the author—are constructed by Dr. Robinson of Armagh, who, in respect of that combination of the speculative and practical faculties so necessary to an astronomical Observer, has at present very few rivals. The memoir on which the tables are founded, is printed in vol. xix. of the *Transactions of the Royal Irish Academy.*—(2.) The same atmospheric influence necessarily affects the apparent altitudes of points—such as the top of a mountain—not far from the surface of the earth. This influence is named *Terrestrial Refraction.* An approximate evaluation of this evidently much concerns the accuracy of measurements of elevation by the usual trigonometrical methods. It may be found very nearly by aid of simultaneous and reciprocal observations; *i. e.*, by observing the angle of depression from one station and the angle of elevation from the other, at the same moment. Such Observations however, cannot be obtained in very many cases, so that general rules are desiderated. Rules of a very general nature have been given by different geodetic surveyors of great eminence—depending on one variable only, viz., the assumed distance of the observed point from the observing station. Even these are far from corresponding. Maskelyne estimated the refraction at $\frac{1}{10}$th of the distance expressed in degrees of a great circle. Legendre allows only $\frac{1}{14}$th for the refraction in altitude. Delambre takes $\frac{1}{11}$th part of the arc of distance. Colonel Mudge prefers the $\frac{1}{12}$th part. It seems indeed that the fraction varies from the $\frac{1}{7}$th to the $\frac{1}{18}$th of the contained arc, according to the weather and the state of the atmosphere. A good solution, therefore, cannot be obtained, unless, along with the distance, we give attention to the variables affecting the state of the atmosphere. This has often been attempted—especially by Plana; most successfully and conveniently, however, of late years, by M. Bravais, whose rather complicated formulæ have been expressed in very manageable tables by M. Delcros. All who are practically interested in geodetics are referred to these tables in the *Annuaire Meteorologique* for 1851.

Refraction Solar. Many indications seem, at present, to make it likely that an ether containing multitudes of meteoric bodies, fills our interplanetary spaces; and that its substance concentrates, or augments in density, according to its nearness to the sun. If this be true, stars seen in the neighbourhood of the sun ought to undergo a peculiar *refraction.* This refraction is termed *solar refraction.* Observation has not yet confirmed its existence; but means are being used that will prove its reality or the reverse.—Professor Piazzi Smyth had this subject in view during his late residence on the Peak of Teneriffe.

Refractive Index. The *absolute refractive index* of a body is the proportion which the sine of the angle, made by a ray passing from vacuum into the body and the perpendicular to the surface of the body at the point, bears to the sine of the angle made by the refracted ray, with the perpendicular produced. The theorem on which the laws of refraction are based, is, that this is a constant quantity for any medium. The *relative refractive index* of two media is the proportion of the sine of the angle made by the ray passing from the first medium with the perpendicular to the surface of contact of the media at that point compared with the sine of the angle made by the ray passing after refraction through the second medium and that perpendicular produced. The method of finding the relative refractive indices for two media, whose absolute refractive indices are given, is indicated in DIOPTRICS (*q. v.*)

Refrangibility : Refringency. The refrangibility of any ray is its liability to be refracted, and the measure of it is the angle of refraction. Thus the several coloured rays have different refrangibilities depending on the lengths of the waves or undulations causing them. The *refringency*, if of a substance, on the other hand, is the power to refract a ray, belonging to the substance or medium through which the ray passes. Thus prisms of various substances have different refringencies. So also a rhomb of Iceland spar is bi-refringent.

Repeating Circle. See CIRCLE.

Repulsion. See BOSCOVICH, ELECTRICITY, for theory and illustration of repulsive forces.

Resistance. A most important subject in Molecular Physics, but which at present is so much more within the range of Practical Mechanics and Engineering, that we do not feel it our province to discuss it here. The term is applicable to a variety of phenomena; for instance, that very important class—the resistance of wires of different materials and lengths to the transit of electrical currents.—See TELEGRAPH. Its most common application, however, is to the energy with which materials resist the action of external weights or forces tending to bend or break them. We can add nothing to what has already been given under ELASTICITY and FRICTION. The same resisting force likewise appears when bodies endeavour to move through fluids, whether liquid or gaseous. These latter questions are about the most complex in practical mechanics, and cannot be said to be in any way theoretically resolved. The formulæ that have been deduced are little more than indications as to the direction in which experiments ought to be pushed. See any good work on engineering, and

everal valuable reports among the *Transactions* f the British Association.

Reticulus, or **Reticulum Rhomboidale.** One of Lacaille's southern constellations, situated right between the large stars of Argo and Eridanus. The star α reticuli is of the third magnitude.

Retrogradation. A movement in which the planets seem to go backward in the ecliptic, or contrary to the order of signs. A planet often seems to move forward, stops gradually, and then moves backward. Both the superior and inferior planets are subject to this apparent movement, but from different causes. Of course, since we know that their real motion is in ellipses, the cause of this apparent irregularity is the movement of our point of sight, viz., the Earth itself. The mean periods of retrogradation are—for Saturn, 140 days; Jupiter, 120 days; Mars, 73 days; Venus, 42 days; Mercury, 22 days.

Revolution. For the periods of the planetary revolutions, see ELEMENTS. For the theory of motions of revolution, see ROTATION.

Rhomb, Fresnel's. A rhomb of crown or St. Gobain's glass, so cut that a ray of light entering one of its faces at right angles, shall emerge at right angles at the opposite face, after undergoing within the rhomb, at its other faces, two *total reflections.* It is used to produce a ray circularly *polarized.*

Rhumb. This word has come to signify one of the thirty-two points into which the circumference round which the compass may vibrate is divided. Hence, to proceed upon a rhumb is to proceed constantly in one direction,

Rhumb Line. If a vessel sail either due E., W., N., or S., she evidently describes a circle on the earth's surface. But if she sail in one constant direction—towards the same intermediate point of the compass—that is, preserving a line which cuts the successive meridians at a constant angle, she will describe—as any one may see by drawing such a line on a sphere—a spiral curve, called the *loxodromic spiral*, which constantly approaches, but never reaches the pole. It is called the rhumb line for the point of the compass towards which the vessel's course is steered.

Rifle, a fire-arm constructed in such a manner as to make its bullet or projectile revolve on its axis of flight. The rifle is a most ingenious and successful application of mechanical science, —in theory, a beautiful deduction from a few simple principles—in practice, a weapon of terrible efficiency, either in the field of war, or in the hand of the sportsman who pursues the denizens of the forest, the jungle, or the plain. The name of its inventor and the period of its invention have not been correctly ascertained; but it appears to have been introduced in Germany about a hundred years after the invention of fire-arms. The theory is this: a projectile driven through the air is subject to deflection, from inequalities in its form or weight, or from the imperfect manner in which the original impulse is communicated. A bullet may be irregular in form, heavier on one side than the other, or the propelling force may not be given quite accurately at the last point of contact with the propelling weapon, whether that weapon be a bow, a musket barrel, or a cannon; or otherwise, the bullet, in being projected, may be made to roll on its transverse axis, so as to cause deviation on one side or the other from the line of direction. The causes of deviation are numerous, and the principle of the rifle is, that these shall be compensated for by their rapid reproduction on all sides. This is effected by causing the bullet to revolve on the axis of flight, so that any inequalities may be reproduced too rapidly to allow of deviation on one side or the other. The best illustration of the rifle is that given by Robins more than a hundred years since. It is that of a common top, used as a plaything by children. We cannot balance the top on its point. If the point be quite sharp, by no effort can we succeed in making the top stand upright. It falls over on one side, in spite of our utmost endeavours. Let us spin it, and immediately the top not only stands on its point, but we are unable to upset it, so long as the spinning motion continues. If spun on a table, we may throw it to the ground, or knock it on the side, or endeavour to overturn it; but so long as the rotatory motion continues, we cannot make it upset. If struck, it immediately recovers its upright position, and if the motion be fast enough, it will again spin true. Suppose, then, that we wished this top to fall through the air with its point always in advance, either down the shaft of a mine or from the top of a precipice,—we have only to spin it, and it will infallibly fall correctly, until the rotatory motion is exhausted. To apply this principle to a musket or cannon bullet, we have only to make it rotate on the line of fire, and its course, instead of being irregular, will be practically nearly perfect—subject, of course, to the force of gravity, which will make the bullet fall to the ground in the time it would require to fall from the muzzle of the piece, supposing the line of fire to be dead level—and subject also to the action of the wind. Rifling corrects the causes of deviation which may be inherent in the projectile, but not those general causes which operate externally. To cause the bullet to revolve on its axis, grooves are cut in the inside of the rifle barrel, which grooves are intended to communicate the rotatory motion. The grooves are not straight—in which case they would be inoperative—but are cut spirally, like a string on a walking-cane, which makes one turn, more or less, from one end to the other. The amount of turn is called the pitch, and this pitch may be one whole turn in the length of the barrel, or half a turn, or a quarter of a turn, according to circumstances. The rapidity with which the bullet turns may be understood from the consideration that a rifle

bullet travels through the air, say, 1,200 feet in a second; and if the bullet revolves once in three feet, it will turn on its axis 400 times in a second,—a velocity of rotation which we could scarcely communicate by any other means whatever. If the pitch were one turn in six feet, the bullet would only spin 200 times; and if the pitch were one turn in eighteen inches, the bullet would, on the contrary, spin 800 times in the second of flight—that is, in passing through the air a distance of 400 yards. The proper pitch or proper amount of turn given to the grooves of a rifle barrel has long been a matter of dispute. The extremes are from one turn in two or three feet to one turn in twelve or fourteen feet. The first would be called rapid rifling, the latter slow rifling. The principle was not understood until lately, that the rapidity of turn ought to increase with the length of the projectile. The turn in the Enfield musket, employed in the British army, is one turn in six feet six inches; but this, from experiments recently made, appears to be rather too slow for the best practice. One turn in four feet would probably be better for a barrel of the regulation calibre.

To make the bullet receive the rotatory motion—that is, to be influenced by the spiral grooves—it was formerly the practice to use a bullet large enough to require considerable force to make it enter the muzzle of the gun. It was driven in by a wooden mallet, or heavy iron ramrod, a process causing much delay and trouble. This system has been entirely revolutionized by the system of the expanding bullet. Lead being easily compressible, it occurred to various experimentalists to cause the bullet to expand by the force of the explosion, so as to be forced into the grooves sufficiently to make it "take the rifling." Mr. Greener claims the invention, but his suggestion was not adopted by Government at the time, though acknowledged at a later period. Captain Minié was the first who gained notoriety for the new system. He employed an iron cup or capsule at the base of the bullet, which cup being driven in by the explosion of the gunpowder, squeezed out the lead laterally, and made it fill the grooves. The cup was altogether misapplied in practice, though good in theory. It was sometimes driven right through the bullet, leaving a ring of lead in the barrel. It was too powerful an agent for the purpose. In the service, a wooden plug has been substituted, and is found to answer tolerably well. Another plan—the plan used by the Swiss—is to make rings in the bullet, so that when the bullet is acted on by the explosion of the powder, the lead *drives up*, and takes the rifling. This plan succeeds admirably where great nicety is observed in the fitting of the various parts—the bullet to the calibre, &c. Another plan—that used by the late lamented General Jacob, the greatest rifleman who ever put gun to shoulder—is to have the grooves cut deep, and to have rings or *flanges* on the bullet, which fit the grooves, and cause it to take the rifling without difficulty.

The number of grooves in the barrel of a rifled musket, or barrel of a sporting rifle, is very much a matter of taste. The smallest number in use is two; and the two-grooved rifle, some years since, was very extensively patronized, when it was found to obviate the difficulty in loading attending the old system. It has now almost disappeared. The Brunswick rifle, employed in the British service, was a two-groove, and probably the worst weapon ever put into the hands of the British soldier in the form of a firearm. The present Enfield musket has three shallow grooves. Five or six would probably be found more advantageous. Seven is a very common number in German rifles, which are, deservedly held in high repute; and in American rifles, which, in what may be termed the racehorse class, namely, those that possess the highest velocity, are the first in the world. Fifteen grooves may also be used, or, in fact, almost any number that can be cut by a machine.

Recently, however, a system has been introduced which bids fair to supersede all the ingenious plans at present in vogue for making the bullet take the rifling—that is, expand sufficiently to be acted on by the spiral grooves so as to give the transverse rotatory motion. We refer to the system of breech-loading. When the bullet is placed in the barrel by the muzzle, its diameter must be small enough to allow it to enter with some degree of ease. But in that case there is a danger that some of the bullets may fail to be rifled, and consequently may turn over and over. They then go astray. Instead of going forward like an arrow, they turn *head over heels*, like a stick thrown from the hand. They are then useless for all practical purposes, and, in addition, are extremely dangerous for target shooting, as their deviation may be much greater than could be imagined beforehand. We have, in fact, known cupped bullets perform antics in the air not unlike the performances of the boomerang. This is one objection attending the muzzle-loader. Another objection is, that it requires more time to load. Another, that if a flask be used, there is always the possibility of an accident by the blowing up of the flask. The probability may be very small, but every season accidents of this nature do occur, and the use of the flask gives at least one more chance of danger.

To remedy these inconveniences, the breech-loader has been introduced. In its present state it is sufficiently perfect for use, and at least rivals the muzzle-loader. The probability seems to be that in a few years the muzzle-loader will be laid aside, like the flint-gun, although it must still be stated that hitherto the muzzle-loader holds the first and best place for good and accurate shooting. No breech-loader has yet been constructed to surpass the muzzle-loader in accurate shooting, although the former has the advan-

vantage of rapidity. The breech-loading system, however, when applied to the rifle, has this great advantage, that it saves all trouble regarding the rifling of the bullet. If the system were perfected, there cannot be the slightest doubt of its superiority. In the meantime, so many new schemes are started by the ablest gunmakers and mechanics of the day, that no long period can elapse before the best and most practicable system is discovered.

In the rifle, the breech-loader assumes the form of an ordinary piece, with some small variations required for the change of mechanism. The barrel is first rifled throughout. At the breech end a space sufficient for a cartridge is then determined — varying in length according to calibre, &c.—and the rifling is bored out of this space—say two inches, or thereby. This chamber is made large enough to receive the cartridge, which contains a bullet of greater diameter than the rifled portion of the barrel. When the powder explodes, the bullet is driven through the barrel, and fits it so tightly, that no gas escapes round the bullet. There is no *windage*. Every bullet must infallibly be rifled if the cartridges are properly made; but if the cartridge case is too long, and enters the rifled portion of the barrel, the leaden bullet is crushed against the paper, instead of being crushed into the grooves, and of course does not sufficiently take the rifling. The ordinary cartridges sold for breech-loading rifles require improvement in this respect. The paper or pasteboard case projects too far forward on the bullet.

The *bullet* of the rifle, next to the barrel, is the object of most importance. Almost every imaginable shape has been tried; and, according to the purpose for which the gun is to be used, different shapes have their respective advantages. The elementary shapes, from which all rifle bullets may be said to be constructed, are the sphere and hemisphere — the cylinder, longer or shorter; and the cone or conoid—the cone with a curved side, instead of a straight side. The sphere, or round ball, was employed until about fifteen years since; and the true reason that the rifle made no progress was the fact that the round ball was still universally in use. It has now disappeared, or is employed for short distances. At 100 yards the round ball shoots as well as any. Beyond that distance it more and more shows its inferiority. For shooting animals, however, at short distances, it is probably as efficient as any, as it makes a big hole. The Americans, for their wonderfully accurate shooting, use a conoid, without any portion of cylinder. They are, however, compelled to use a loading muzzle, which may be described as a nuisance of over refinement. The military bullet is a cylinder with a hemisphere on the point, and a cup at the butt of the bullet. The cupped bullet is never to be depended on for accurate shooting, though well enough for military purposes. The best bullet we have ever seen in practice is composed of the hemisphere for the butt or part next the powder; then two rings, with a portion of cylinder between them; then, for the point, a gracefully curved conoid. We have shot this bullet for years from a well-made rifle, and on no single occasion ever saw one bullet go astray; and this circumstance rifle shooters will recognize as a fact of the utmost importance in estimating the value of a bullet. For piercing iron, we should, on the contrary, take a blunt bullet—in fact, a cylinder two or three diameters in length would be nearer the form for that purpose.

Next to the bullet is the patch, or piece of greased cloth, placed between the bullet and the barrel, to prevent the dirt from accumulating. We can let the reader into a secret. The great art of good rifle shooting is a straight bore and *plenty of grease*. Whatever ingenious gunmakers may say to sportsmen or rifle shooters who are afraid of dirtying their fingers or soiling their gloves, we can assure all rifle shooters, that without plenty of grease they will never make good shooting with guns constructed to burn gunpowder. The grease is not merely for the purpose of making the ball load easily, but for the purpose of preventing the powder caking in the lower part of the barrel. The best rifle in the world can be thrown out of its shooting by firing three shots with it without grease; and many guns have entirely deceived those who shot them from this simple circumstance.

For the material of the rifle barrel, we are inclined to side with the Americans, that fine cast-steel is superior to everything yet employed. It is coming into use in Great Britain, and is approved by some of the best rifle shots. It does not show a figure like the twisted barrels, but browns plain. Next to it we should select a piece of plain good iron, such as the Enfield musket barrels are made of. But the sportsman will still probably adhere to the figured barrel, as custom has associated it with the best class of fire-arms.

The powder for rifle shooting ought to be much larger in the grain than that used for the shot gun. If too fine, it explodes too rapidly, and jerks the bullet out of shape. A compressed or expanding bullet must always be driven into an altered shape. It becomes shorter and thicker. But too fine powder blows it out of shape; and this the Americans call upsetting, or setting up—a term which English writers have confounded with upsetting, or turning over.

The great error in all English rifles has been the want of weight. For shooting animals, the short double rifle is the right weapon; but for the practice of the art of rifle shooting in its perfection, weight is the essential next to a straight hole. If the hole is not straight, the piece is valueless. But weight and thickness are necessary—the one to prevent recoil, the other to prevent expansion. For a single-barrelled rifle,

thirty inches long and eighty bore, seven or eight pounds are not an ounce too much for the bare barrel, without ribs or patent breech. The volunteer rifle now used is a remarkably good weapon for its purpose; but it would shoot incomparably better if loaded with flask and patch, instead of the hideous cartridge, that seems designed to prevent the gun shooting well. From the cavity in the Government bullet being so deep, the powder pinches the sides of the cup against the barrel as powerfully as if the lead were held in a vice, and the head of the bullet is blown away, leaving the ring of lead in the barrel, to complicate the after proceedings. These cupped bullets are quite erroneous in principle when the sides of the cavity are parallel with the barrel.

In the form of a *pistol*, the rifle is a most efficient weapon. The revolver is only a rifled pistol with a repeating cylinder, with five or six chambers, each of which contains a charge. After each shot, when the finger is pulled for another shot, the cylinder revolves, so as to being a new chamber in presence of the single barrel, and the bullet is driven through the barrel when the explosion takes place in the chamber of the cylinder. This pistol is one of the best weapons that ever was constructed for rapid performance. If a certain number of men in each regiment were armed with them, if the seamen and marines were supplied with them, and if the volunteers were to adopt them, in addition to the rifle and artillery gun, the British nation would be so absolutely impregnable, that no hostile force could touch our shores without certain and inevitable destruction. In the form of *cannon*, the rifle has recently been converted into the most formidable arm that the world has ever seen. Sir William Armstrong has produced guns which, though fearfully expensive in manufacture, are yet completely successful in performance. The range these guns command would previously have been considered impossible—at least by military engineers. The first successful cast-iron rifled cannon appears to have been rifled in the city of Glasgow, in the month of June, 1858.

Rigidity. A body is said to be rigid when the connections of its parts are supposed to be capable of destroying some force tending to break them. In the last part of article ROTATION, it will be seen how the rigidity of a rotating body causes a definite percussion and a definite permanent pressure upon the rotating axis.

Roots of Equations. Suppose the following equation given

$$x^n + p_1 x^{n-1} + p_2 x^{n-2} \ldots\ldots\ldots p^n = o,$$

all quantities, a, b, c, &c., which, being substituted for x, satisfy this identity, are called *roots of the equation*. It is the object of the theory of equations to discover those roots in every case. To effect the general solution, would be to obtain an expression for x in terms of the co-efficients $p_1\, p_2$, &c., just as is done for quadratics. This has yet only been accomplished generally, for such as are of no higher than the fourth order. But methods have been obtained which give either accurately or approximately the numerical values of the real roots of numerical equations. The general propositions upon which these two processes, so far as they are complete, depend, constitute the theory of equations.

If a quantity a, satisfies the equation $f\,x = o$, then $f\,x$ is divisible by $x - a$ without remainder. Let

$$\frac{f\,x}{x-a} = Q + \frac{R}{x-a},$$

the division being carried on until R contains no power of x. Hence R will remain the same whatever special values are given to x.

$$f\,x = Q\,(x-a) + R. \quad \text{Let } x = a$$

Then $f\,a = Q \times 0 + R$, R being the same as before.

But $f\,a = 0$, $Q \times 0 = 0$, hence $R = 0$.

$$\text{Therefore, } \frac{f\,x}{x-a} = Q, \text{ without remainder.}$$

For the actual division it will be easy to prove this rule,—in the quotient, the co-efficient of x^{n-1}, the first term, is unity, and the co-efficients of the other terms are formed by multiplying that of the preceding by a, and adding the co-efficient of that term in $f\,x$, which involves the same power of x as the preceding term.—Assuming, then, that a, b, c, &c.,.......l satisfy the equation $x - a, x - b, x - c$, &c., will divide it accurately, and we shall have $f\,x = (x-a)\ (x-b)\ (x-c)\,(x-d)$, &c.,..........$(x-l)$, where the number of the roots a, b, c, d.........l is n, the same as the order of $f\,x$. If any of these factors, $x - a$, $x - b$, &c., be equal, we shall have $f\,x = (x-a)^m\ (x-b)^n\ (x-c)^p$, &c., where m, n, p, &c., indicate the number of times of repetition of that factor to which it is attached. We may consider, then, that all forms in x, such as $f\,x$, are really the products of certain factors of the first order, $x - a$, $x - b$, &c., n in number. Now we shall clearly obtain complete solution of $f\,x = o$, if we can reduce it to this factorial form.

For if $x = a$, $f\,x = (a - a)\ (a - b)$ &c., $= o$.
And so for $x =$ any of the quantities, b, c, d, &c.
And no other quantity but a, b, c, d, &c., can satisfy the equation. For if it could, let e be the quantity. Then

$$f\,e = (e-a)\ (e-b)\ (e-c)\ldots\ldots(e-l)$$

which cannot be equal to o, because none of its factors is so.—If by trial with two values a, b, we obtain $f\,a$, $f\,b$ of different signs, then *at least* one root lies between a and b. For in passing slowly from a to b, as we pass from one side of zero to the other with $f\,x$, there must be a point where $f\,x$ passes through zero,—that is, must be a value c between a and b, which is a root of

$f x = o$.—If p be the greatest co-efficient in $f x$, without regard to sign, it is easy to show that for $p + 1$, and every greater value, $f x$ is positive; also for $-(p + 1)$ and every less value (negative but numerically greater), $f x$ is positive or negative, according as n is even or odd. By using this proposition with the last, and considering the value of $f x$ for $x = o$, we shall see that every equation of an odd degree has at least one real finite root, of a sign contrary to that of its last term, and every one of an even degree has at least two finite roots of different signs.—Another proposition assisting us in determining the *number* of real roots is, that impossible roots enter equations in pairs. Let $a + b \sqrt{-1}$ be a root. Then the substitution will give $P + Q \sqrt{-1} = o$, an equation only satisfied for $P = o$, $Q = o$. Again, substituting in $f x$, $a - b \sqrt{-1}$ for x, we have $P - Q \sqrt{-1}$, which must also be o. Therefore if $a + b \sqrt{-1}$ be a root, $a - b \sqrt{-1}$ is so also. These propositions, then, give certain rough approximations to the number and value of these roots.—Before proceeding further, it is better to obtain power to transform equations into certain others, whose roots bear special relations to those of the first, before those of the first become known. Consider, then, in $(x - a)(x - b)(x - c)$ &c., what the co-efficients are. Evidently the co-efficient of x^{n-1} is $-(a + b + c +$ &c.), that of x^{n-2} is $a b + a c +$ &c., viz., the sum of the product of every two quantities a, b, c, &c.,—that of the third is, the sum of the products of every three, and so on. Hence, if we have the equation $(x + a)(x + b)(x + c)$ &c., $= o$, in which the roots are those of the last, with their signs changed in every case, the first term x^n remains the same,—the second changes its sign,—the third remains the same,—the fourth changes its sign, and so on. Hence, to change the signs of the roots of

$$x^n + p_1 x^{n-1} + p_2 x^{n-2} \dots\dots\dots + p_n = o,$$

we have only to write

$$x^n - p_1 x^{n-1} + p_2 x^{n-2} \dots\dots\dots \pm p_n = o,$$

for the new equation. Again, suppose we wish to change the roots by adding or subtracting one uniform quantity from each. Thus, to subtract h from the roots, we have only to substitute $x + h$ for x in the equation $f x = o$, which gives us the new equation

$$f h + f' h \cdot x + \frac{f'' h}{\angle 2} \cdot x^3 \dots \frac{f^r h}{\angle r} \dots x^r = o$$

the sign $\angle r$ standing for the product of all whole numbers up to r. Clearly one use that may be made of this is to take away one term of an equation. To take away the second, for example, we must have $n h + p$, which is the new co-efficient of x^{n-1}, $= o \therefore h = -\frac{p^1}{n}$, and by increasing the roots by $\frac{p_1}{n}$, we get rid of the second term of the equation. Similarly, for the third term, we must have $= o$

$$\frac{n \cdot (n - 1)}{1 \cdot 2} \cdot h^2 + (n - 1) p_1 h + p_2 - o,$$

and so for the other terms. Solving these equations we get values of h, by which the roots must be diminished. This is, in fact, as the reader will see, the process for quadratic equations.—Only one other transformation need be noticed—that in which each root is made a constant multiple of the original one. Thus, instead of

$$(x - a)(x - b)(x - c) \text{ \&c.,}$$

we are to have

$$(x - m a)(x - m b)(x - m c) \text{ \&c.}$$

Remembering how $p_1 p_2 p_3$ &c., are constituted, it will at once be seen that if

$$(x - a)(x - b) \text{ \&c.,} = x^n + p_1 x^{n-1} + p_2 x^{n-2} \dots p_n$$
$$(x - m a)(x - m b) \text{ \&c.,}$$
$$= x^n + m p_1 x^{n-1} + m^2 p_2 x^{n-2} \dots\dots m^n p_n$$

By means of this transformation we can evidently get rid of fractions in the co-efficients taking m as the L. C. M. of the denominators, or sometimes a less number.—Returning to the theory as distinguished from these auxiliary problems, we may state the following proposition. Quantities between which the real roots of an equation lie when substituted for x, give results alternately positive and negative. Let a, b, c, &c., be the real roots in order of magnitude, and ϕx the part containing the impossible roots, which is necessarily positive and of even order. Then

$$f x = (x - a)(x - b)(x - c) \dots\dots\dots \phi x$$

If, here, a quantity greater than a be substituted, all the terms are positive,—if one between a and b, we have one negative factor, and all the rest positive,—if one between b and c, two negative factors, and therefore a positive result,—if between c and d, three negative factors, and a negative result, and so on. Hence the proposition. Again, suppose all these roots lie between α and β, and that these are all the real roots. Then

$$\frac{f \alpha}{f \beta} = \frac{(\alpha - a)(\alpha - b)(\alpha - c) \dots\dots (\alpha - l)}{(\beta - a)(\beta - b)(\beta - c) \dots\dots (\beta - l)} \cdot \frac{\phi \alpha}{\phi \beta}$$

Now, $\phi \alpha$ and $\phi \beta$ are both of one sign, because ϕx has no root between α. β. Also one set $(\alpha - a)$ $(\alpha - b)$, $(\alpha - c)$, &c., are all negative, and the other all positive. Hence, *the number of roots, a, b, c,……l,* which lie between α and β, is *odd*, if $f\alpha$ and $f\beta$ have different signs, and *even* if they have the same sign. This, however, supposes the roots to occur *singly*. It will be evident that in these propositions indications are given of a real, but very loose tentative approximation to numerical roots. If, indeed, one could find as

many quantities as dimensions of fx, which gave results alternatively positive and negative, we should evidently have these as limits between which the roots must lie. If, further, we can find limits beyond which we can look for no roots, other approximate quantities are obtained. We have seen, for example, that if p be the greatest co-efficient without regard to sign, $(p + 1)$ and $-(p + 1)$ are limits beyond which there are no roots. By the very same proof we may show that if p^1 be the greatest negative co-efficient, $p^1 + 1$ is a limit of positive roots, narrower, usually, than the preceding. One of the most convenient practical limits is obtained thus:—Each negative co-efficient taken positively is divided by the sum of all the positive co-efficients which precede it, and the greatest of the fractions thus formed is a superior limit of the positive roots. All these methods, however, give but very wide limits to the roots. What is called Newton's method of the Limits of Roots, depends on the principle that all values which make fx, and all the derived equations positive, are superior limits of positive roots. This will be evident by substituting $x+h$ for x, so as to have

$$f(x+h) = fx + f'x \cdot h + \&c., = o$$

where it is manifest, that if all the co-efficients be positive for any value x' of x, h must be negative,—that is, $x + h$ must become $x' - h'$, a quantity less than x'. What is called Waring's method, is also commonly used for equations of not higher order than the fourth.—The last of what we may call propositions of approximation, which we shall note here—is Des Cartes' rule of signs. It is, that no equation can have more positive real roots than it has *changes of sign* from + to —, and from — to +; and, again, no complete equation—that is, supplying void places as by $o\,x^m$—can have more negative roots than *continuations of the same sign.*—We pass now from these methods of approximation to such as give us at all events the accurate number of real roots, and the position about which each of these lies. Of these there are—Solutions of Cubics, as Cardan's Rule, and of Biquadratics by reduction to Cubics, by Euler's and Des Cartes' method. Each of these processes enables us to find complete solution of even purely symbolical equations; but the results come in such a form, that they are only of practical use when there are two impossible and two possible roots,—in the cases where three or all of the roots are possible, we should have symbolical expressions containing impossible forms, from which, however, it is impossible to disentangle them. But there is a theorem, Sturm's theorem, which gives a complete practical solution to any numerical equation of whatever order, so far as its real roots go. A still simpler *preliminary* principle, however, enables us to find, not merely in approximation but with complete accuracy, all the *equal* roots of any equation. Let

$$fx = (x-a)^2\,(x-b)\,(x-c)\,(x-\delta)-(x\,..\,l)$$

Then $$f^1 x = (x-a)^2 \left\{(x-b)\ \&c.,\right\}$$
$$+\ (x-b)\ (x-c\ \ c.\ (x-a)$$

Therefore, $f^1 x$ and fx have the common measure $x - a$. Hence, by taking the G. C. M. of fx and $f^1 x$, we obtain $(x - a)$, and we know that it is repeated twice in the equation $fx = o$. If we have

$$e.g.\ fx = (x-a)^m\,(x-b)^n\,(x-c)\,p,\ \&c.,$$

$$f^1 x = \text{A}\left\{x-a)^{m-1}\ (x-b)^{n-1}\right.$$
$$\left.(x-c)^{p-1},\ \&c.,\right\}$$

the G. C. M. of which will be

$$(x-a)^{m-1}\,(x-b)^{n-1}(x-c)^{p-1},\ \&c.$$

Similarly the G. C. M. of $f'x$ and $f''x$ will be

$$(x-a)^{m-2}\,(x-b)_{n-2}\,(x-c)^{p-2},\ \&c.,$$

and so on, until we reduce by successive divisions, to the last equal root, $(x - a)$. When, therefore, we have an equation presented, the *first* process is to detect and remove the equal roots. This presents no difficulty. Suppose $fx = o$, to be freed from equal roots. Then Sturm's theorem applies. Let $f_1 x$ be the first derived function from fx. Find the G. C. M. of fx and $f_1 x$, and *change the signs of the remainder.* Let $f_2 x$ be this remainder—proceed with it and $f_1 x$, so as to find their G. C. M.; and, again, change the signs of the remainder, and proceed in this way until we reach a number as the last remainder. Then if fx, $f_1 x$, $f_2 x$.........$f_n x$ be the series of modified remainders, Sturm's theorem is, *that the difference of the number of changes of sign in the results of the substitution of* a *and* b *for* x *in those quantities, expresses the number of real roots of* f x = o, *which lie between* a *and* b. This theorem we shall prove, and show how to apply. Call the successive quotients $q_1\ q_2$, &c., then, since the remainders are $-f_2 x$, $f_3 x$, &c., we have

$$\begin{aligned}
f\ x &= q_1\,f_1\,x - f_2\,x\\
f_1\ x &= q_2\,f_2\,x - f_3\,x\\
f_2\ x &= q_3\,f_3\,x - f_4\,x\\
&\ldots\ldots\ldots\ldots\\
f_{m-1}x &= q_m\ f_m x - f_{m+1}\,x\\
&\ldots\ldots\ldots\ldots\\
f_{n-2}\,x &= q_{n-1}\,f_{n-1}x - f_n x
\end{aligned}$$

Now, since fx, and $f_1 x$ have no common measure, the last remainder $f_n x$ is necessarily a number. Hence no two successive numbers, $f_m x$ and f_{m+1}, x can both become 0, for any value of x. Because in that case $f_{m+2}\,x = 0$, $f_{m+3}\,x = 0$ and so on, therefore $f_n x = 0$. But this can never be so, since *it* is independent of the value of x. Again, any one value which makes one of the functions $f_m x$ vanish, reduces the two adjacent to have different signs. Now, suppose the value $x = c$, to make one of the auxiliary functions $f^m x$ vanish, without making fx vanish,

then we shall seek the effect upon the number of changes of sign in passing through c, that is, for $c - h$ and $c + h$ where h is a quantity infinitely small. We need, evidently, only consider those $f_{m-1}x$, $f_m x$, $f_{m+1}x$, because no alteration in the order of signs will at all be made whichever value $c - h$, c, or $c + h$, is substituted in any other but $f_m x$. Thus, then, uniting $c - h$ in those three, we have

$$f_{m-} c, -h f_m c, f_{m+} c.$$

And since $f_m c$ vanishes, we have, as already shown, different signs for $f_{m-1} c$, and $f_{m+1} c$. Therefore there is *one* change of sign. Now, substituting $c + h$ for x, we find

$$f_{m-1} c, +h f^1_m c, f_{m+1} c,$$

which similarly has *one* change of sign. Therefore in passing through c, which is any root of any of the equations $f_m x = o$, except the original $f x = o$, there is no change of sign lost. Suppose, again, that $x = c$, makes $f x = o$, vanish. Then we need only consider $f x$, $f_1 x$. Substituting $c - h$, and $c + h$ successively, we have

$$-h f^1 c, f^1 c$$
$$+h f^1 c, f^1 c$$

In the first of which there is a change, and in the second a continuation of signs. Therefore, in passing upwards through any value c, which is a real root of $f x = o$, a real change of sign is lost. Thus, evidently, in passing regularly up from any limit a to any other b, there will be lost just as many changes of sign as there are real roots of the equation, a being the less and b the greater of the limits. Hence, then, the number of real roots between any two limits chosen arbitrarily can be at once found by means of Sturm's auxiliary functions. All real roots are between $-\infty$ and $+\infty$. Therefore, by substituting these, we can find the total number of real roots. It is evident that this is the same as forming the first terms, containing the highest power of x into a series, and then changing x into $-x$, and seeing how many changes of sign are gained. By means of this theorem, then, we can find—*the number of real roots altogether*—*the number of real roots between arbitrary limits.* If, for example, we find between $+10$ and -10, *three* real roots, we can interpose the other limit 0, and thus find, say, two real roots between 0 and $+10$, and one between 0 and -10. The latter we can treat by finding whether it is between 0 and -5, or -5 and -10, and so on, narrowing the limits until we obtain any degree of approximation desired. We see, then, that Sturm's theorem enables us certainly to find all the real roots of any numerical equation, to any degree of accuracy that may be desired.

Take, for example, $x^3 - 7x + 7 = 0$.

Here $f_1 x = 3x^2 - 7)\ 3x^3 - 21x + 21\ (x$

$$3x^3 - 7x$$
$$-14x + 21$$

$f_2 x = 2x - 3)\ 6x^2 - 14\ (3x + \frac{9}{2}$

$$6x^2 - 9x$$
$$9x - 14$$
$$9x - \tfrac{27}{2}$$

$f_3 x = +1 \qquad -\frac{1}{2}$

The leading terms are all + here, hence, substituting $-x$ for x, there would be as many as three changes of sign, therefore all the roots are possible. Also $+2$, gives the same sign as $+\infty$, hence we need not go higher.

	$f x$	$f_1 x$	$f_2 x$	$f_3 x$
$x = 2$	+	+	+	+
1	+	—	—	+
0	+	—	—	+
— 1	+	—	—	+
— 2	+	+	—	+
— 3	+	+	—	+
— 4	—	+	—	+

and since -4 gives the same series of signs that $-\infty$ does, we may stop here. Hence, looking at the table, we see that *two* real roots lie between $+2$ and $+1$, and one between -3 and -4. To separate the two between 1 and 2, try $x = \frac{3}{2}$, then we have

$$- \quad - \quad 0 \quad +$$

in which there is one more change than in the first, and one less than in the second line. Hence, one root lies between 2 and 1·5, and one between 1·5 and 1. It is easy to see how to carry the process further to any approximation. In proceeding, then, to the solution of a numerical equation, we should first find the equal roots and divide by them, so as to have the equation as $f x = (x - a)(x - b)(x - c)$ &c.,...... $= o$ Next proceed to form Sturm's auxiliary functions, $f_1 x$, $f_2 x$, &c. Then apply some of the principles of approximation, which give the limits of roots, which have been noted above. Substitute these limits, and any number of numbers between them in Sturm's auxiliary functions, and note where changes of sign occur, and how many there are. Where there is more than one change of sign between two consecutive values choose intermediate ones, until the separation is completely effected.—There is another equally general, but perhaps more troublesome process, called after its discoverer, Fourier; and many processes of quite special but important application for *reciprocal* equations—approximations by means of continued fractions, &c. For full exposition we must refer the reader to Hymer's or Young's *Theory of Algebraical Equations.*

Rotation. We can conceive of a body as moving round a *fixed axis*, or definite line. In this conception all is distinct—we see that one rate of motion belongs to the whole body, that

all points at equal distances from the axis move in equal circles, and with uniform velocities. We can picture the motion of every single point of the body, as a simple course with whose whole character we are familiar, and whose position in space we can at once determine. Again, we can conceive of a body as moving round a *single point*, fixed in the body. Here there is by no means the same simplicity—what paths the body may describe in space, it is very difficult to grasp, so as to be able to follow completely every single particle in its course. Both these conceptions are purely *Cinematical*—connected with the geometrical theory of *motion per se*, apart from all discussions of its cause. But evidently the element of cause may be added, and we may have given us to discuss the character of the motions which will result from the application of certain definite forces to the body, which is made to rotate. This is the *dynamical* part of the subject. We shall try to exhibit briefly the more important propositions in both. It is evident that rotation round an immovable axis in no way changes the figure of the body. Internally it remains quite the same, if the body be perfectly rigid. We can readily see what is a fit measure of the amount of rotation. All the points describe in the same time arcs of a circle in a plane perpendicular to the axis of rotation, which are proportional in length to their radii. Hence the standard of velocity may be taken $= \frac{Arc}{Radius}$, which is constant for the whole body. This is the same, then, as the measure of actual velocity of a point for which the radius is 1. Hence if $\theta =$ *angular velocity* $= \frac{Arc}{Radius}$, $\theta r =$ absolute velocity.

Suppose two movements of rotation round axes o p, o q, which are lines in proportion of the angular velocities p and q. Then these two rotations can be destroyed and completely replaced by one rotation, the magnitude and direction of whose axis are represented by o o′, the diagonal of the parallelogram on o p, o q. Suppose any point m, of the body in the angle which is supplemental to p, o, q. Draw from m perpendiculars x, y, h, on o p, o q, o o′. Then, in consequence of o p—supposing the rotations in the same direction—the point m will be raised, let us say, from the plane of o, p, q, with a velocity $p\,x$. Again, in consequence of the rotation q, it will be raised with a velocity $q\,y$. Therefore it is raised with velocity $p\,x + q\,y$. But, by a well known geometrical property, o $p \,.\, x +$ o $q \,.\, y =$ o o′. h, or $p\,x + q\,y = $ o′ h, and o′ h is the velocity with which a rotation round o o′, with angular velocity θ, would

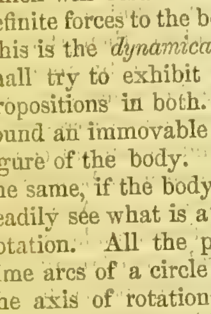


Fig. 1.

lift the body. Hence the two rotations as respects m, that is, any point in the plane o p q can be replaced by the rotation θ. But the whole body moves with this plane, so that the proposition holds true generally. We must lay down here a convention, however, for the geometrical representation of rotatory movement—the line which represents it is uniformly along the axis of movement—proportional to the angular velocity, and drawn in that one of the two directions of the axis from any given point, which lets one see the rotation as one would see the hands of a watch. Thus, for a rotation which to one looking down on a watch held in his hand, appears in the same direction as the hands, the line which measures rotation would have the point o at the watch face, and o p would be measured outwards *towards* the holder of the watch, proportional to the angular velocity. Another illustration—and, in fact, the real origin of the convention—is, that the apparent rotation of the sun would be measured by a line drawn from the ecliptic plane *towards* spectators in our northern latitudes. Motion in the opposite direction is measured by lines drawn from the ecliptic plane away from the spectator. Using this convention, we see that o p, o q, represent rotations which move m *up* from the plane of the paper. Consider now a point in the angle o p q itself. Then the rotation o p will move it up and o q move it down. Hence $-p\,x + q\,y$ is the resultant rotation. Also, o o′ moves the body up or down according as m is in p o o′, or in q o o′. Hence, according to these suppositions—$p\,x + q\,y$ ought to be equal to $+$ o′ h, or $-$ o′ h. We know this to be the case from the geometrical proposition aforesaid. Hence in all cases, two rotations round axes passing through one point, can be compounded into one, upon identically the same principle as forces acting at a point are compounded. There is therefore a *parallelogram of rotations*. It is an immediate consequence that there is also a *parallelopiped of rotations*, that is, if any rotations o p, o q, o r, be replaced, the diagonal o o′ of the parallelopiped which they form is their resultant. Hence, indeed, any number of rotations whose axes all pass through a point can be compounded according to the same laws as the same number of forces proportional to them. We indicate here an immediate geometrical inference from the cinematical principles just proved. If any point m, not in the plane, o p k, be taken, and x y h drawn, then $p\,x$, $q\,y$, o′ h are again proportional to the rotations for m. Also, these are proportional to the triangles m o p, m o o′, m o q, and the direction of motion is perpendicular to these planes, so that the lines of motion for each, form the same angles as the

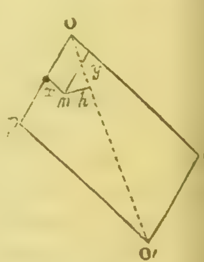


Fig. 2.

planes. But these lines of motion form the sides and diagonal of a parallelogram. Hence the three triangles stand to one another in the same relation as the sides and diagonal of a parallelogram, whose sides and diagonal contain the same angles as the triangles do. This proposition can be proved quite independently; but it comes out as a very beautiful consequence of the principles of rotation.—Proceeding, then, according to the analogy of the theory of forces, which is now suggested, consider two rotations, p and q, round parallel axes, O p, O q. Suppose m a point in the plane of rotation. Then let $m = x$, A B $= a$ (the perpendicular distance of the axes.) Here m will be lifted $p\,x$ by p and $q\,(a + x)$, by B. Hence altogether m will be lifted with velocity $p\,x + q\,a + q\,x$. One rotation $p + q$, at distance m C $\dfrac{p\,x + q\,a + q\,x}{p + q}$, will then produce the same effect upon this one point m.

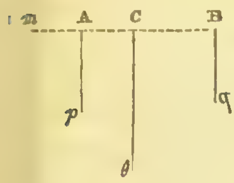


Fig. 3.

Now A C $= m$ C $- x = \dfrac{p\,x + q\,x + q\,a}{p + q} - x = \dfrac{q\,a}{p + q}$. And C B $= a -$ A C $= a - \dfrac{q\,a}{p + q} = \dfrac{p\,a}{p + q}$. Hence C is the point where A B is divided in inverse proportion to the adjacent rotations p and q, and is therefore the same for all points m in the plane. Hence the two rotations may be replaced completely (since, as before, with the movement of the plane p A B q, all the body moves) by $p + q$, acting at C. That is, we may compound rotations round parallel axes by the same rules as we compound parallel forces. Keeping this analogy in mind, or proceeding from first principles, the student will very easily apply the proof to the case where the point m is *within* the space p A B q. Let us follow out the analogy already so productive, to the case of rotations round parallel axes in different directions. The figure will show to any one acquainted with the demonstration of the corresponding statical problem, that it still holds. Where the two forces are equal in statics, we get as the result an infinitely small force applied at an infinite distance. Here also an infinitely small *rotation* applied at an infinite distance. Of course either result, in itself nugatory, sends us back to first principles. Let us see what will be the effect of two such opposed rotations. Consider any point m in the plane, distant x from the first axis p, and $x - a$ from the second—a being the perpendicular distance between the axes. Then the velocity with which m rises from the plane is $p\,x + (-p)\,(x - a) = p\,a$, a quantity constant wherever m is taken. What is the meaning of this result, except that the whole plane, and therefore the whole body, rises evenly and simultaneously, with a uniform velocity. Hence we see, then, that the *couple* of *rotations* gives a motion purely of *translation*. It is sufficiently clear from the analogy, now wrought out in all fundamental points, that we can compound and decompound rotations in the very same manner as forces. Hence, if we have any number of rotations, $p\ q\ r$, &c., anywhere in space, we can take any point O, place these two equal and opposite rotations p, and $-p$, and thus have, instead of the original p, p at O and a couple of rotations $p' - p$. It is evident, then, that we can decompose any motion given by rotations round fixed axes anywhere in space into two final results—a resultant rotation θ (corresponding to the resultant force), and a resultant couple of rotations—which we have seen to be equivalent to a motion of pure *translation*. As we can for the case of decomposed forces make a final reduction, and obtain a central axis of resultants, and a minimum couple in a plane perpendicular to it—so here, as we see from the analogy, we can reduce any system of rotations to one where there is a central axis of rotation and a minimum couple of rotations, whose axis is the central axis—that is a minimum motion of translation along this line. This final reduction, then, enables us to lay it down as a proposition—that we can reduce all movement of a body in virtue of any number of rotations, wherever situated, to a motion of turning round a given right line, and at the same time slipping along it. This is also the final reduction of motions of translation, for we can bring *forces*, which produce them, to have the same effect as a force and a couple. Hence we might already infer, that since all motion is either one of *translation* or one of *rotation*, and since each of these can be reduced—according to independent proofs—to this motion of turning round a given line, and at the same time slipping along its axis—that is, in fact, to the motion of a point along the thread of a screw, that all motion could be reduced to that elementary motion. We shall see immediately what proposition we anticipate and assume here. The analogy between *rotations* and *translations*—their mutual convertibility—we may best exhibit in the words of M. Poinsot:—

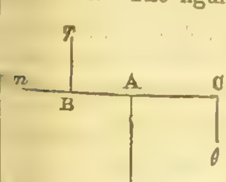


Fig. 4.

"We may now see the perfect symmetry of this composition of rotations and of forces; they are almost identical; for, had we originally called *force* the cause which is capable of making a body turn on an axis, we should have had a system of statics perfectly like what we now have for these new forces. Only in it simple forces (considered always as transported to the centre of gravity of the body), would have answered to our couples in

ordinary statics, and couples to our simple forces. Indeed, the same treatise, written on the science of forces, might be perfectly exact, and might treat the science completely, although the word force in it should be understood in these two completely different senses."

II. In the ultimate reduction of all motion to that along the thread of a screw, given above, we had not taken into consideration at all the motion round a point, instead of a fixed line. We proceed to show that we may reduce any motion of that kind whatever, into a series of successive motions round fixed axes. From that of course follows the complete generality of the reduction spoken of. Suppose the point O fixed.

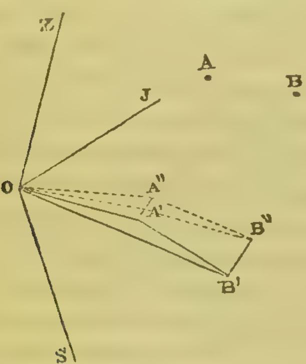


Fig. 5.

Suppose A, B, two points of a body, to take, after an instant's rotation round O, the position A'' B''. Draw O A B, O A'' B''. Then if O S be the line of section of these two planes, evidently, rotation round it, will bring A B into A B', in the plane O A'' B''. Also, all the lines O A, O B, A B, are unaltered, because O is fixed, and the body rigid. Hence all possible positions which A B can take in the plane O A'' B'' will be taken by sliding the permanent triangle O A' B' (= O A B) round in that plane. O A'' B'' will therefore be one of these positions. But this sliding is actually a rotation round an axis through O perpendicular to the plane. Therefore two rotations round axes through O will bring the points A B into any position A'' B'' which is possible for them. But these two may be compounded into one O I. Hence we may consider any motion round a point as produced by rotation for an instant round an immovable axis. Next instant, the body may be considered as moving round another immovable axis, differently situated, and so on. Hence any total movement round a fixed point is representable by an infinite number of movements round axes fixed for the moment, but momentarily changing in position. We are not to conceive the INSTANTANEOUS AXIS as passing continuously along from O I, but as successively occupying the positions—perfectly immovable for the instant—O I, O I', O I'', &c. The instantaneous axis then always passes through the fixed point, O. It is to be confessed, however, that we have not yet a very clear idea of the mode of motion round a fixed point. The little elementary rotations round instantaneous axes cannot be graphically represented like the little elementary polygon's sides, which approximate to a curve. Let us try to gain yet a clearer idea of it. Conceive a body rotating round O I. The instantaneous axis will take a certain path in the body itself, and a certain path in space. If we suppose the body cut by a sphere whose centre is O, the instantaneous axes may be represented for the sake of distinctness of conception, as radii. Now the end I has properly no path either in space or in the body; but yet since it has an infinite number of successive positions, we may evidently group them best together, by considering them to constitute a continuous path. Suppose that marked out in the body, before the commencement of rotation. Let it be the curve ς, and suppose the first instantaneous axis to be

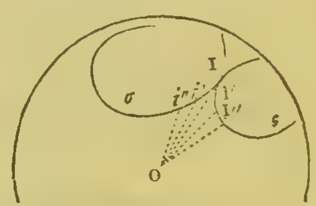


Fig. 6.

O I. The motion round O I will bring the next elementary position I', where the next instantaneous axis cuts the sphere, suppose to i', and would alter the position of the rest of the curve ς in space. Rotation round O i' would bring I'' to i'', and so on. Thus we should have the curve σ, which represents the path *in space*, of the extremity of the instantaneous axis. This axis may be conceived, then, as describing *in the body*, and *in space* two different *cones*, whose bases are ς and σ, where they are cut by the sphere, because they always pass through O. *We have thus succeeded in representing the whole motion to ourselves; as consisting in the rolling of the cone whose base is ς, upon that whose base is σ, without any slipping being permitted.* It follows that in whatever way a body moves round a fixed point, the movement may be represented by that of a definite cone rolling upon another. The instantaneous axis is the line where the two conical surfaces meet. This is perhaps the clearest way in which it is possible to present to our conception motion round a point. Let us consider it further. We note the following quantities, which present themselves in thought, as in connection with this idea. The angular velocity θ of rotation round O I—the angular velocity ω with which the extremity I describes ς or σ, for evidently both are described in the same time, the one curve being just the other unrolled. The radii of curvature r and ϱ, of ς and σ—the angular movements p and ϖ of the pole I, round the axes O P, O Π of the oscillating cone for the two curves. If ϵ, e be the two angles of the ele-

mentary polygon, which constitutes r, ς, and therefore the dihedral angles for the sides of the pyramid which makes the cone, we have $\theta\, d\, t$, the angle described in dt, $= d e - d \varepsilon$. And $d \varepsilon = \frac{d \sigma}{\varrho}$, $d e = \frac{d \varsigma}{r}$. Hence $\theta = \frac{d \varsigma}{r\, d t} + - \frac{d \sigma}{\varrho\, d t}$. And $\frac{d \varrho}{d t} = \frac{d r}{d t} = \omega$. Therefore $\theta = \omega \left(\frac{1}{r} \pm \frac{1}{\varrho}\right)$. Again, if the radii vectores $\text{I P} = r$, $\text{I }\Pi = \varrho$ are drawn, O P, $\text{O }\Pi$ are axes of the right osculating cones. If O I be taken $= \frac{r^2}{\text{O I}^2} = \frac{a^2}{\text{O I}^2 - a^2}$, where $a =$ the radius of the base of the osculating right cones—that is $r^2 = \frac{a^2}{1 - a^2}$. So also $\varrho^2 = \frac{\alpha^2}{1 - \alpha^2}$. Hence $\theta = \frac{\omega}{a} \sqrt{1 - a^2} \pm \frac{\omega}{\alpha} \sqrt{1 - \alpha^2}$.

But $\frac{\omega}{a}$, $\frac{\omega}{\alpha}$ are p, ϖ respectively and $\frac{a}{1} = \sin$ semi-angle of one cone, and $\frac{\alpha}{1} = \sin.$ semi-angle of other. Therefore $\theta = p \cos \varepsilon + \varpi \cos \varepsilon'$. Also $p ; \Pi :: \sin \varepsilon' : \sin \varepsilon \therefore \theta = \text{K} (\overline{\sin} \varepsilon' \cos \varepsilon + \sin \varepsilon \cos \varepsilon')$. Hence p, ϖ, θ are sides and diagonals of a parallelogram, whose angles are ε, ε', $\varepsilon + \varepsilon'$. —We shall consider finally the most general idea which we are able to form of the movement of a body in absolute space. Evidently it may be reduced to a simple translation, starting from a point O, and a simple rotation θ, round an axis O I. If the direction of translation is in the plane of rotation, we can reduce the whole movement to a simple rotation. For the translation may be represented by a couple of rotations, one of whose elements is equal and opposite to that which causes the rotation and which destroys it; and the other, equal of course to this, is in the same plane parallel to it. The axis O′ S of this rotation is called the *spontaneous axis.* If, however, the direction of translation is not in the plane of rotation, we may transfer it into two couples nevertheless, one of which will be, as in the last case, in the plane of rotation, and the other not. This former may be compounded with the original rotation, as before, so as to leave of it and the original rotation only one rotation. There is thus one rotation and a couple of rotations in a plane perpendicular to it—that is, a translation along the axis of rotation. This is, as we have already seen, a motion representable by the motion of a particle in the thread of a screw. All motion, then, of a rigid body—the most general it can have in space—may be represented *instantaneously*, by the motion of a point in the thread of a screw.

III. We come to consider the purely dynamical part of the subject—what are the forces capable of producing a given movement? It may be laid down, in the first place, that there are always forces capable of producing a given movement. Thus, every particle has a certain velocity u, and a force $u\, d\, m$ would therefore produce this. Hence a set of forces $u\, d\, m$, applied to every particle of the body, may be conceived capable of producing the movement in question. These forces can be reduced to P, Q, R, &c., by ordinary methods of reduction. We must note, however, the fundamental difference, that these reductions presume, and imply connections between the different parts of a body, of which the elementary forces $u\, d\, m$ are quite independent, in the first instant, when they are applied. The forces P, Q, R, &c., imply these connections then also. If we do not consider these internal connections, but think only of the external forces, we may compound $u\, d\, m$ into such forces as P, Q, R, &c. This we shall do in the first instance. Afterwards we shall come to what the effects of centrifugal force are. A pure movement of translation, in which all the particles advance with one velocity u in parallel lines, may evidently be compounded into the action of one single force $\int u\, d\, m$, passing through the centre of gravity. In the same way a single force R, passing through the C. G., may be decomposed into parallel forces, applied to the molecules, and proportional to their masses. The uniform velocity will be $\frac{\text{R}}{m}$, where m is the whole mass. These consequences would not obtain if R, always passing through the C. G., changed occasionally or continuously in magnitude and direction. They would hold for each successive instant, and therefore altogether, though the particles would move not in straight but in curve lines. Next, consider a pure movement of *rotation.* Let a body turn round O Z with angular velocity θ. Then taking two arbitrary rectangular axes O X, O Y, and reducing the forces for every element, we find

$\text{X} = \theta\, y\, d\, m$, $\text{Y} = -\theta\, x\, d\, m$, $z = o$ $\text{L} = -\theta\, x\, z\, d\, m$, $\text{M} = -\theta\, y\, z\, d\, m$, $\text{N} = \theta (x^2 + y^2)\, d\, m = \theta\, r^2\, d\, m$. Hence summing up we have 1°·2 forces X, Y, which are $\theta \int y\, d\, m$, and $-\theta \int x\, d\, m$ respectively—or their resultant

$$\text{P} = \theta \sqrt{\left(\int x\, d\, m\right)^2 + \left(\int y\, d\, m\right)^2}.$$

Or since if $a =$ the perpendicular from the centre of gravity of the body on O Z,

$$a\, m = \sqrt{\left(\int x\, d\, m\right)^2 \times \left(\int y\, d\, m\right)^2};$$

we have $P = \theta\, m\, a$. 2°. Two couples $L = -\theta \int x\, z\, d\, m$, $N = -\theta \int y\, z\, d\, m$, or a single couple K, round an axis perpendicular to O Z, whose moment is

$$K = \theta \sqrt{\left(\int x\, z\, d\, m\right)^2 + \left(\int y\, z\, d\, m\right)^2}.$$

3°. And, lastly, a couple N, whose axis is O Z, and moment

$$N = \theta \int \left(x^2 + y^2\right) d\, m = \theta \int z^2\, d\, m.$$

If O Z pass through the centre of gravity $\int y\, d\, m = o$, $\int x\, d\, m = o$ $\therefore$ $P = o$, and there are only left two couples, which are reducible to one. Hence forces which turn a body round an axis, through its centre of gravity, can be reduced to a single couple. If O Z be one of the principal axes, $\int x\, z\, d\, m = o$, $\int y\, z\, d\, m = o$. Hence $K = o$. Therefore if the forces turn the body round a principal axis, they can be reduced to a couple whose axis that is. And inversely, if a couple be applied to a free body in a plane perpendicular to a principal axis, its effect will be to make the body turn round on this axis with an angular velocity equal to the moment, divided by the moment of inertia $\left(\int r^2\, d\, m\right)$ of the principal axes. It is easy to see that these principles will apply also to the case in which there are both translations and rotations.—We pass to centrifugal forces. To have a clear idea of these, let us note that the particle $d\, m$, under the tangential force $u\, d\, m$, could not describe an arc of a circle, but a line. But it does describe such an arc, and therefore there must be some cause to bend it inwards. This centripetal force f is of the same kind as gravity. Hence $\frac{1}{2} f \,.\, d\, t^2$ is the fall for a time, t. But $u\, d\, t$ is the space moved through along the tangent in $d\, t$. Hence (*Euc.* III., 36), $\frac{1}{2} f\, d\, t^2 \,.\, (2\, r + e) = u^2\, d\, t^2$, $\therefore$ $f = \frac{u^2}{r}$, neglecting e, which is the excess of the secant of an infinitely small angle over 1. This gives us the magnitude which f must have to produce its actual effects. Of course this result applies to all curvilinear movement. Since $u = \theta r$, $f = \theta^2 r$. We may therefore represent the physical circumstances as follows:—There is no centripetal force among the data. If such be evolved, there must be evolved along with it an equal and opposite centrifugal force, so that the circumstances of the problem do not change. Thus, the centripetal force being $-\theta^2\, r\, d\, m\, d\, t$ (—because it tends to diminish r), we must have a centrifugal force $+\theta^2\, r\, d\, m\, d\, t$ opposite to the centripetal—that is, in the direction of the radius, and tending to increase it. And this gives us the elementary idea and measure of centrifugal forces. Let us see how these reductions can be operated. $\theta^2\, r\, d\, m\, d\, t = \theta\, d\, t \,.\, \theta\, r\, d\, m$, that is, the centrifugal force bears a constant ratio to the tangential, and is perpendicular to it. Reduce them just like the tangential. Then Π = resultant force

$$= \theta^2 \sqrt{\left(\int x\, \delta\, m\right)^2 + \left(\int y\, \delta\, m\right)^2}$$

$= \theta\, P$. And since all the forces that make up this are perpendicular and proportional to those which make up P, $\theta\, P$ is perpendicular to the direction of P. Similarly

$$\chi = \theta^2 \sqrt{\left(\int x\, z\, d\, m\right)^2 + \left(\int y\, z\, d\, m\right)^2}$$

$= \theta\, K$, and the axis of χ is perpendicular to that of K. But it is to O Z also, that is to the axis of N. Therefore it is to the axis of the resultant of N and K which is G. Hence the axis of the couple due to centrifugal forces is perpendicular to the axis of rotation and to that of the couple of impulsion. We may repeat the same suppositions about O Z passing through the centre of gravity or being a principal axis with quite analogous results. The last question which we shall consider we now come to. Suppose a body in rest, moveable round a fixed axis O Z, to be struck by a couple N perpendicular to it—find 1° the angular velocity of the motion θ 2° the percussion which the fixed axis receives 3° the continual pressure which, in consequence of the centrifugal forces, it has to support. Take on O Z a line O N, representing at once the magnitude and direction of the couple of impulsion. Suppose now two forces P, P′ equal and opposite, and two couples K and K′ so also, P and K being the force and couple which, with N, make the body rotate as if the axis were perfectly free. N, P, K, then are capable of making the body move quite freely. But they produce an angular velocity $\frac{N}{\int r^2\, d\, m}$; and the other two quantities are destroyed by the axis. Hence $\frac{N}{\int r^2\, d\, m}$ is the value of the angular velocity. Again, P′ is the percussion on the axis, and K′ the continual pressure, and $P' = -P =$

$$-\theta \sqrt{\left(\int x\, d\, m\right)^2 + \left(\int y\, d\, m\right)^2}$$

$K' = -K = -\theta$

$$\sqrt{\left(\int x\, z\, d\, m\right)^2 + \left(\int y\, z\, d\, m\right)^2}$$

—We have given only the elements of this very important subject. The fullest and clearest information respecting it is to be had in Poinsot's *Theorie Nouvelle de la Rotation des Corps.*

Rotation of the Planets. See SOLAR SYSTEM.

Rotatory Polarization. See POLARIZATION.

Safety Lamp. See LAMP.

Safety Valve. In locomotive engines, the safety valves are two valves loaded with certain pressures, and placed on the boiler for the escape of steam of pressures beyond those. The one is beyond the engineman's control, and is called the lock-up valve: the other, at a little lower pressure, is under his power, and regulated by a lever and spring balance. It is only necessary to have the aperture sufficiently large to let off the steam as fast as it is generated, when the engine is at once put out of work. The valve is sometimes loaded by a heavy weight laid on it—sometimes by means of a lever, with a weight to move along to suit the required pressure.

Sagitta. *The Arrow.* One of the old constellations, near the back of AQUILA. It contains no stars higher than the fourth magnitude.

Sagittarius. *The Archer.* One of the zodiacal constellations. It is situate below Aquila, between Scorpio and Capricornus. There are no stars above the third magnitude.

Saros. An Egyptian period. See CYCLE.

Satellites. *Secondary* Planets, or those which, like our Moon, accompany *Primaries.* It seems remarkable that while all the known planets beyond the region of the Asteroids are accompanied by systems of satellites, our globe is the only one within that region which has a companion or attendant. The elements of all known satellites are given under ELEMENTS. Regarding the satellites of Uranus, there is still a puzzle. Some astronomers relying on the unsurpassed accuracy of Sir William Herschel, are inclined to accept it as probable that this planet has EIGHT attendants of the periodic times as below:—

No.	Periodic Times.		
1st Satellite, ...	2d.	12h.	— —
2d Satellite, ...	4	—	— —
3d Satellite, ...	5	21	— —
4th Satellite, ...	8	16	56m 31
5th Satellite, ...	10	23	— —
6th Satellite, ...	13	11	721·6
7th Satellite, ...	38	2	— —
8th Satellite, ...	107	12	— —

It cannot be doubted that some such scheme is necessary to reconcile all observations; and that such an one does so. We shall soon have additions to Neptune's satellites.—It has been alleged, regarding the rotation of these satellites on their axes, that, like the Moon, they always present the same face to their primaries. This, however, as it seems to the writer, on no substantial ground. Had not the condition of the Moon in this respect been previously known, the observations in question would never have been held adequate to sustain such an inference. But the case of the Moon constitutes no ground of analogy. Quite as likely, her state is a peculiar one—depending, for the most part, on her own internal structure.

Saturation. When a soluble substance is put into a solvent, there is in many cases only a definite proportion of the substance which will dissolve. The solvent is then said to be saturated with the substance. Sometimes a solution saturated with one substance will yet dissolve a different one.

Saturn. Without doubt the most interesting of all the planets belonging to our solar system. It is not so large as Jupiter, its diameter being only 79,160 miles, while that of Jupiter is 87,000; but instead of four satellites it has eight, and it is distinguished besides by that extraordinary appendage of Rings. Of the planet itself little can be said. Its surface is marked by a few bands of various shades passing across it, although with the exception of a bright one near its equator—(a band clearly belonging to the planet itself, and having no relation to reflexion from the rings)—these are not so well marked as in the case of Jupiter. It is purely elliptical in form, not irregularly so, as Sir William Herschel originally supposed. It rotates on its axis in 10 hours, 29 minutes; and its period of revolution in its orbit is 29 years, 5 months, and 24 days; or, more exactly, its periodic time is 10,763 days. The specific gravity of Saturn is about the lowest among the planetary bodies, being only 0·14 of the specific gravity of the Earth; so that this gigantic globe must be composed of materials not much heavier than cork. The celestial landscape from the surface of Saturn must indeed be magnificent; near it, its Satellites, Mimas, Enceladus, Tethys, Dione, Rhea, Titan, Hyperion, Japetus, shining like Moons; and that wonderful ring now shading or eclipsing large bands of the planet, and again illuminating them by the light its broad surface reflects. The relations of the rings to Saturn, in this latter respect, have recently been fully and exactly discussed by Dr. Lardner, in a memoir in the transactions of the Astronomical Society, to which we gladly refer the curious student. Concerning the satellites, whose distances, periodic times, &c., have been given in ELEMENTS, nothing further need at present be said, but the habitudes, history, and probable development of the Rings are connected with facts so varied and novel, that we are constrained to devote a separate article to the subject.

Saturn's Rings. The general aspect of the planet and these appendages, is given in fig. 4, plate II., copied from a sketch by Captain Jacob, from his fine achromatic at Madras.

It will aid the reader as he follows our discussion of details.

(1.) *General Description of the Rings.*—Soon after his discovery of the satellites of Jupiter, Galileo became aware that something peculiar attached to the planet Saturn: but, as his telescope had not power to discern the actual phenomenon, he merely placed on record the facts which puzzled him. The ring being of course best seen at the sides of the planet, presented the appearance of two *ansæ* or *handles* attached to the central orb; nor was the reality ascertained until the time of Huyghens, who gave publicity to his discovery in the following sentence (the letters of which, after the fashion of the time, he transposed), "*Annulo cingitur tenui, plano, nusquam cohærente, ad eclipticam inclinato.*" Not long after the discovery by Huyghens, the chief division of the ring was noticed by Mr. William Ball in England; and it was speedily placed beyond doubt by Cassini, that Saturn has two concentric rings of different breadths, separated all round by an interval easily distinguishable. These are the rings we shall refer to, as A and B; the latter the one nearer to Saturn. Subsequently, from Sir William Herschel's time downwards, statements have been made of visible divisions in the midst of these two rings. One in the middle of A, as represented in our plate, appears pretty well established; but various others have been reported by Captain Kater, the Astronomers of the *Collegio Romano*, Encke, and several more. These divisions, however, have not continued visible, and the solution apparently accepted was, that they do not exist; but that through defects of his telescope or the condition of the atmosphere, the Observer reporting them was deceived. This solution, it need scarcely be remarked, was far from satisfactory. Besides the apparent divisions or black lines, broad mottled and dark *stripes*, especially within the ring B, had been noticed, by Mr. Dawes among others.—But while the question as to the significance of these various appearances still remained an unsettled one, Astronomers were startled by the discovery of a third ring C, within the ring B, of a slaty colour and therefore comparatively dark, but nevertheless sending out so much light that the marvel was it had not been discovered before. This third ring was first detected by Mr. Bond at Cambridge Massachussets, on November 15, 1850; and again, without any knowledge of what Mr. Bond had noticed, by Mr. Dawes, on November 29; and a few days later by M. Lassell. The third ring C is represented in our engraving. It, too, appears to present puzzling phenomena akin to those noticed above. Various accounts have been given of its breadth, which seems augmenting. Mr. Dawes feels persuaded that he has seen at least one division in it; and at times it has looked as if it were separated from ring B. This ring is manifestly no new or very recent formation. Appearances were seen by Gallé at Berlin,—that can alone be explained by its existence—in the year 1838: and in some of Sir William Herschel's drawings, a dark belt is found across Saturn, attributable to no other cause. Yet had it been as fully developed as now, why did so many admirable observers miss it? It can now be seen in clear nights through a good achromatic of four inches diameter; and we know that telescopes very greatly more powerful, had often been intently directed towards Saturn. What can be made of this accumulation of perplexing circumstances? Is there an *actual instability* in this singular system?—We shall attempt the solution in the remainder of this article; but, in the meantime, let us realize the *dimensions* of the formation, concerning which we are speculating. The best average measures of the rings, &c., are probably the following:—

		Miles.
Exterior diameter of A,		176,418
Breadth of do.,		10,573
Interval between A and B,		1,791
Exterior diameter of B,		151,690
Breadth of do.,		17,175
Breadth of C,	about	6,350
Distance of inner edge of C and the limb of Saturn,	about	7,460
Breadth of entire System of Rings,		35,889

It is difficult to ascertain the exact *thickness* of this system. A bright line so thin, is scarcely measurable; and besides, there is no likelihood that *this* thickness is measurable. Bessel of Konigsberg determined the *mass* of the system of rings by their effect on the apsides of the satellite *Titan*, to be the $\frac{1}{118}$th part of Saturn's mass. Combining this determination with the best measures we have, which are probably those of Mr. Bond of Cambridge, it may be concluded that the thickness is somewhere about 100 miles,—that is, about the three thousandth part of the breadth. Conceive a cake *one inch* in thickness, and *two hundred and fifty feet* in breadth, its length being indefinite;—that is a similitude of the mechanical condition of this Ring-appendage of Saturn!

(2.) *The Stability of the peri-Saturnian Rings.*—The character of an arrangement, apparently so unlike any other within our system, being realized, the question immediately arises as to the mode or the combination of forces, by which it is secured in its position, and protected from the influence of disturbing causes. The theorems of Celestial Mechanics, show distinctly enough how a planet or satellite suitably placed, must revolve for ever around its central Orb; but we cannot infer that the same stability may be extended, by the same agencies or any combination of them to systems of Rings.—The question now proposed is divisible into two parts, the discussion of which leads directly to very unexpected conclusions.

1. The subject was, earliest of all, taken up by Laplace. That great philosopher looked at it under two of its phases. The one case he

exposed to a searching inquiry, and observation soon confirmed his results: respecting the other he expressed an *opinion* only, but left no investigation; unfortunately this opinion was wrong. Reflecting on the immense attractive power of so vast a globe as Saturn, Laplace discerned that with such a force in its interior, any description of ring would assuredly be broken and dragged inwards piecemeal, if its particles were not endowed with what is ordinarily called a centrifugal force capable of resisting that attractive central energy. The ring, therefore, he alleged, must *revolve* in about the same time that a satellite should revolve at its mean distance;—a conclusion quite irresistible, and speedily confirmed by the scrutiny of Sir William Herschel, who, by watching the progress of certain dusky spots on its surface, established the period of revolution as 10 hours, 29 minutes, 11 seconds. Laplace also saw farther. He found that as the outer and inner circumferences of so broad a ring, rotating *as a whole*, would necessarily have conflicting velocities that might endanger its security, it was necessary that it consist of separate rings,—and that this was the mechanical significance of the permanent division between A and B, confirmed by Cassini. An inference subsequently impugned by Plana, on the ground of a most erroneous conception of the *mass* of the rings: so far from Laplace's theory being invalidated, we shall soon see that it may be pushed very much further.——
Another question however, cognate although essentially different, came up next before the great Geometer. If Saturn's mass could be presumed to remain *invariably in the precise centre of the system of rings*, that system—protected by the foregoing motion of rotation—might continue stable. But it is impossible to conceive this rigorous coincidence of the two centres. The ring, affected by the varying positions of the satellites, must change its place by small quantities; and observation itself appears to have sometimes detected a slight eccentricity in the position of the planet. Now, if the ring be uniform around its circuit, the occurrence of the smallest eccentricity of position would be fatal to its existence. The rim nearest Saturn would indubitably be more attracted by the planet; the eccentricity would increase,—the energy of the destructive force increasing along with it; *and the Ring must speedily fall upon the Planet.* The occurrence of eccentricity being certain, how then is *stability* maintained? It was here that Laplace satisfied himself with offering an *opinion*, the authority of which, it is not too much to say, has retarded or rather prevented subsequent inquiry into the constitution of Saturn's Rings. The opinion or conjecture of this great Geometer was as follows:—suppose the rigid ring not uniform, but *loaded* in one or more parts,—this irregularity will prevent its destruction. For instance, if the part of the rim which is farthest away from Saturn on the occurrence of an eccentricity, is the loaded one, or contains more matter than the opposite portion or that nearer the planet, then will the superior energy of attraction arising from *that additional matter* counterpoise the increment of force acting on the opposite rim as arising *out of its greater proximity*, and equilibrium may still endure. But supposing the eccentricity originating the disturbance, should continue for a period of time, is it not evident that in the course of *five* hours—the period of a semi-revolution of the ring—this loaded or massive part would be brought nearest the planet, and that the very arrangement which formerly acted as a conservative force, would now become a powerful destructive force, against which nothing could possibly avail? The eccentricity, indeed, might not remain steadfast—we know it does not remain steadfast—but, to avert the effects now spoken of, it would require to change in *exact conformity with the period of the ring's revolution;*—a period with which, if we judge from the nature of its producing causes, *it has nothing whatever to do.* The question may be put in other forms and encumbered with various refinements; but accurate inquiry leads in every case to the same conclusion, that no analogous arrangement seems capable to secure the stability of a Rigid Ring which is subject to shiftings of its centre. It may not be unsuitable to add, that such Hypotheses have generally the air of being too ingenious, worthy of every admiration amidst a display of mechanical toys, but rarely constituting an essential part of the vast and simple arrangements of Nature.

2. Reverting to Laplace's first conception, viz., that *one* division of the ring is necessary, so that it be not destroyed by its motion of revolution, let us pursue it to its full consequences. It is easy to obtain an exact idea of the ground of the alleged necessity. For instance, the Earth moves around the Sun in one year, and Mercury in three months,—which periods are necessary to the stability of planets at their respective distances from the Sun. But suppose the two planets bound together—say by a material rod—so that they must both revolve in the same time. If Mercury were thus constrained to revolve as slowly as the Earth, the Sun would pull it towards it with an attractive force so vastly under-counterpoised, that the rod would in all probability break, and Mercury would rush nearer the Sun. If, on the other hand, the two revolved as quickly as Mercury does, the Earth—through its excess of centrifugal force—would also break the rod, and fly off to a great remoteness. In any such case, the rod would be subjected to a heavy strain; and the amount it would bear, would evidently depend on its own inherent toughness or strength. Now, a broad rigid ring rotating around a central body is precisely in the foregoing position: the danger to it depends on its *breadth;* and its power to resist the destructive force, on its *thickness* compared with its *breadth*, as one main element. Acquainted as we now are with the general character

and leading dimensions of that peculiar formation, a larger and more real question than Laplace proposed has become soluble;—the question, viz.: *what is the extreme breadth of a ring of such sort, which could be expected to endure?* It is to two American Men of Science that the first, and apparently the full and satisfactory solution is due. Mr. George Bond, of Cambridge Observatory, broached the inquiry, which was taken up and completed by Professor Pierce, with his well-known acuteness, and tried analytic skill. There appears no dubiety about the results, for the conditions are unchallengeable, and the course of investigation plain. These results are, *first*, that, to produce a chance of stability or to prevent the rings being torn asunder, their breadths must be greatly less than the breadths of A or B—in short, that a stable formation can be nothing other than a very great number of separate narrow rigid rings revolving with their own *suitable* velocities. If this, or anything like it, were real, how many new conditions of instability do we introduce! Observation tells us, that the division between such rings must be extremely narrow; so that the slightest disturbance by external or internal causes would cause one ring impinge upon another; and we should thus have the seed of perpetual catastrophies! Nor would such a constitution protect the system against that mode of dissolution which Laplace afterwards scrutinized. There is no escape from the difficulties therefore, but through the *final rejection of the idea that Saturn's rings are rigid, or in any sense a solid formation.* Mr. Bond at first indicated, as an available resource, the hypothesis that the ring might be liquid, or a vast isolated and flat ocean; and while certain grave difficulties would thereby be removed, the conception enables us to recognize the possibility of fluctuating rings—of temporary separation of currents—thereby in so far explaining those perplexing and varying statements of our best Observers, regarding the number of such divisions. But Mr. Pierce has pushed his analysis farther, by a process through which we cannot now follow him, to a conclusion highly unfavourable also to this new form of the hypothesis. There is manifestly but one other resource—viz., *the peri-Saturnian system may be a mass of meteorites analogous in all respects to those streams of meteors occasioning an intermitting periodicity in the shooting stars; and, probably also, to the Zodiacal Light.* The conclusion is far from being free from difficulties; but it is subject to fewer than any other. It disposes of all difficulties regarding Stability, for each minute mass is virtually a free satellite; it enables us to understand changes in the rings; and the dark ring itself loses its enigmatical character. This ring is semi-transparent; the body of the planet is seen faintly through it—a fact wholly reconcileable with the idea that it too is composed of meteorites, although they must be very sparsely strewn.—Waiting for further elucidation from the probable results of the competition for the Adam's Prize, it may at present be permitted us to remark, that should the foregoing result be verified, it will merely be another proof of the importance within our system, as indeed within the whole universe, of the minutest objects it contains.

(3.) *History of the Rings.*—One other inquiry has recently been started, of a still more singular kind:—unfortunately our limited space will receive nothing more than its heads. Discrepancies in the measures of the breadths, and relations of the rings A and B,—results taken after intervals of a few years, by the best instruments and the best observers,—stirred the question whether the breadths of the rings are really fixed, or whether their dimensions are evolving? These discrepancies are quite too great to be accounted for by errors of observation; so that they must have some *physical cause.* Every fact bearing on this curious subject has been collected by the industry of Otto Struve, and discussed with his hereditary sagacity. Resting first on the drawings of the rings made by the earlier observers; converting their measures, by an acute critical process, into the nearest attainable relation to our modern micrometrical determinations, and completing the series by these latter results classified and rendered comparable, he has felt authorized to present a history of the Rings from the year 1657 downwards. Measurements directly and absolutely reliable cannot be said to have begun before the times of Bradley, in 1719; nevertheless these older indications must be allowed a certain positive weight. Struve's essay, containing fullest details, is in the *Recueil de Memoires des Astronomes de l'Observatoire Central de Russie;* and merits well on several accounts the earnest attention of the student. The following are his general conclusions:—(1.) *The interior edges of the rings have been gradually but uninterruptedly approaching Saturn.*—(2.) *The approach of the interior edge, has been accompanied by an increase in the total breadth of the rings.*—(3.) *In the interval between the observations of J. D. Cassini, and Sir William Herschel, the breadth of* B, *has increased at a higher rate than the breadth of* A.—The phenomenon, then, is this: the exterior diameter of A not being appreciably altered, it is clear that the matter of the rings is stretching out inwards or towards the body of the planet,—A conclusion unexpectedly confirmed by the recent greater development of the dark ring C, and by the startling fact, that the augmentation of its breadth has already been traced. It were premature to condescend on actual numbers; but the rate of increase is manifestly so great, that no great space of time shall elapse ere these formations reach the body of the central globe.—How closely does this new result harmonize with the conclusions of our former section! And it adds great probability to the speculation, that within our system itself, there are causes of change which will gradually bring all secondary bodies into contact with their

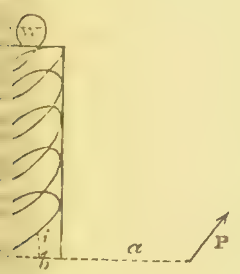
central Luminary.—Apart from these general views however, the history of this inquiry contains a fact alike interesting and instructive. It began with fair hypotheses explanatory of the *stability* of Saturn's *rigid* Rings: it has ended in a proof, that *they are neither rigid nor stable!* It is ever wisest to make sure in the first place, of the facts whose causes we undertake to assign!

Scales. See THERMOMETER.

Scintillation. See TWINKLING OF STARS.

Scorpius, or Scorpio. A zodiacal constellation lying between Libra and Sagittarius, and between Ophiuchus and Lupus. It has one star (α Scorpionis) of the first magnitude, which, along with Spica Virginis and Arcturus, form a conspicuous triangle.

Screw. A screw may be conceived as made in two ways,—either a solid cylinder is rimmed round with a spiral thread which keeps a constant inclination to the vertical—describing thus the curve called the *helix*, and thus constituting the convex screw. Or another hollow cylinder, just capable of containing the thread of the screw is attached, so that there are really hollow tubes where grooves were. This is the *concave* screw.



When the screw is employed to raise weights, or as a mechanical power, we may suppose equilibrium produced between a force P, acting at a distance a, a weight W at the top, and the reacting forces of the screw. Call any one of those R, then resolving the total forces, and let i = angle of inclination of the thread to the horizon; 1° vertically, we have

$$W = \Sigma \, R \cos i.$$

When, as the axis of the screw is supposed fixed, horizontal revolutions would only determine the *pressure* upon that. But take moments *round the axis*.

$$P \, a = \Sigma \, R \sin i \, . \, \bar{b} \, .$$

$$\text{Hence } \frac{P \, a}{W} = \tan i \, . \, \bar{b}$$

$$\frac{P}{W} = \frac{\bar{b} \tan i}{a} = \frac{2 \pi b \tan i}{2 \pi a} =$$

$$= \frac{\text{distance between the threads}}{\text{circumference of circle whose radius is } a}$$

If we consider friction as operating, we have R, R in the limiting case.

Hence resolving

$$W = \Sigma \, R \cos i \pm \Sigma \, \mu \, R \sin i$$

Taking moments

$$P \, . \, a = \Sigma \, . \, R \sin i \, . \, b \pm \Sigma \, \mu \, R \, b \cos i$$

$$\therefore \frac{P \, a}{W} = \frac{b \sin i + \mu \, b \cos i}{\cos i \mp \mu \sin i} \, . \text{ Let } \mu = \tan \alpha$$

$$\therefore \frac{P \, a}{W} = \frac{\bar{b} \, . \sin (i \pm \alpha)}{\cos (i \mp \alpha)}$$

$$\frac{P \, .}{W} = \frac{b}{a} \, \frac{\sin (i \pm a)}{\cos (i \mp \alpha)}$$

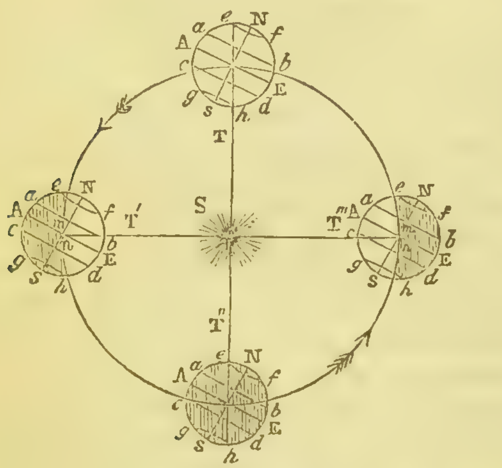
We have taken the force P as figured, because always in practical use of the screw, the power is applied in this way by means of a horizontal lever. An endless screw consists of two or more spiral fillets on a rod capable of rotation round its axis. These threads work in teeth on the circumference of a wheel, so that while the revolution of the rod continues the screw keeps moving on its own axis. For examples of very delicate screws, MICROMETER is referred to. Where a low ratio of power to weight is wanted, the method, as we see from the first result, is to lessen the distance between the threads. But this has a limit, because the pressures opposite to R, on the threads, would break them in a too fine screw. To obviate this a screw is used within a screw, the two turning in different directions. If the distance between the threads in the two are nearly equal, there may be a very low ratio, indeed, of power and weight, and yet both of the screws may be sufficient against the pressures R. It is a purely Kinematical result, as seen in ROTATION, that the most general motion that can be given to a body, may be represented *instantaneously* by motion along the thread of a screw.

Screw in Ships. See Appendix.

Sea. See OCEAN and TIDES.

Seasons. The phenomenon of the seasons can be explained in a very few words. Let the reader take any ball, with a pin through its centre, and carry it in a horizontal plane round a centre, keeping it constantly at a given oblique inclination to the horizon. Let him suppose, now, a candle in the centre, and so consider the effects of its light in illumining the ball. Further, let him take one point upon the ball—nearer the upper pole or end of the axis of rotation in the ball, than the bottom—corresponding, say, to the position of Glasgow on the Earth. Now, suppose the candle joined in every successive position of the ball with its centre. If, then, 67° be the constant inclination of the axis of the ball to the horizon, there will be a position where the line will make with the axis an angle 67°, another where the angle is 180°—67° = 113°; and, in passing round from 67° to 113°, and from 113° back again to 67°, the angle will pass through all values, and be at 90° twice. At the place for which it is 67°, the illumined space will be limited by a circle which includes and goes up behind the upper pole; and for the point P considered, if a circle be drawn through P, perpendicular to the axis, this circle will have its *greater part* in the illumined space. It will continue so,

but decreasingly so, till 90°, where its half is in the illumined space,—after 90° less than its half will be so, and so on to 113°, where it has the least part of it that it can have in this space. Passing on from 113° to 90° we shall have still the least half, but a half always approaching an equal half in this space. At 90° this equal division takes place, and in passing from 90° to 67°, the greater half is so, and increasingly so, until, at 67°, it begins to diminish. Now, in the case of the earth moving round the sun, we have exactly this description fulfilled. Evidently, while the *greater* half of the circle through P is in the illuminated space, the days will be longer than the nights, for the earth turns round her own axis at a rate perfectly uniform; and though P does not describe this circle on the earth's surface, yet, if this circle were merely laid round the earth—which would not alter the circumstances of the illumination at all—P would pass round this circle, and the proportion of the time of daylight to twenty-four hours, would be just the proportion of the illuminated part to the whole circle. We represent the whole arrangement by the figure. The reader will readily understand how, the vernal equinox — corresponds to the 90° for which the angle is diminishing, and the proportion of the circle which is illuminated increasing—how on from that to the point corresponding to 67°, the days lengthen and the nights shorten; and at that point there is the summer solstice, and so on through the year.

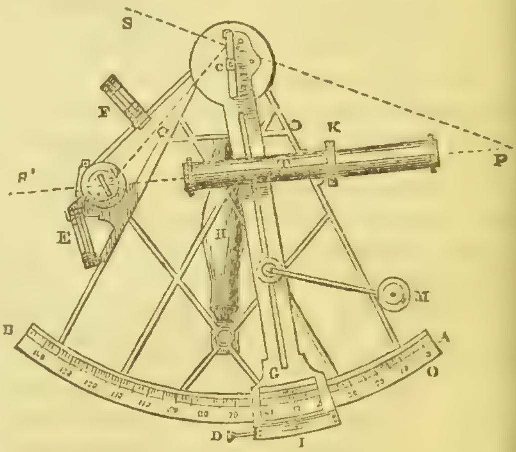


We discover the cause of the seasons to be, then, the oblique inclination of the axis of the earth's rotation, to the plane of its motion round the sun, or the ecliptic. Were it perpendicular to the plane, there would be no seasons. It is easy, by fixing the point P in different positions, to see what will be the phenomena of the seasons for any point on the earth's surface. The maximum inequality of day and night will be seen thus to extend within 23° on a meridian, all round both poles, there being no night; and, again, no day there at certain times of the year. Again, for the equator, the days and nights are always equal, and the greatest inequality increases as we recede from the equator, and pass in either direction towards the poles.

Serpens. A constellation near Aquila, Ophiuchus, Libra, and Hercules. On the maps it is represented as held in the hand of Ophiuchus. Its largest star is of the second magnitude.

Sextans. One of Hevelius' constellations. It is placed on the back of Hydra, at the feet of Leo. It contains no star above the fifth magnitude.

Sextant. The instrument called sextant is represented in the figure. H is the handle by



which it is held—the other hand while observing moves the index G. The arc A B, which consists of 60°, is divided into 120°, and each into parts of 10′ each. The vernier shows divisions of 10″. The microscope M is adjusted for the sake of reading off the divisions and adapting to the employment of the vernier (VERNIER). There is a tangent screw D to enable one to make the adjustments that are necessary. The upper end terminates in a circle, across which is fixed a silvered glass C over the centre of motion, and perpendicular to the plane of the instrument. To the frame is attached at N the horizon glass, the lower half of which is silvered, and the upper not. This also is perpendicular to the plane of the instrument, and so fixed that it is parallel to C when the index G moves to zero. The telescope T is carried by a ring K, and can be raised or lowered by a smaller screw. This is meant to place the telescope so that the field of view may be bisected by the line of separation on the horizon glass N. Two dark glasses are placed at F so as to moderate the intensity of light coming from a bright object like the sun. We shall briefly describe the method of observing an angle by the sextant. The telescope is directed to see the one object S′ at the line where the silvered and unsilvered parts of N meet. Then the index G is turned so that the ray reflected from S the other object upon C, reflected back again from the silvered part of N, should form an angle of S in coincidence with S′ seen directly.

It is clear here that S C N = 180°—2 N C G
and C N P = 180°—2 N C O,
if C O be the line to zero

$\therefore$ SCN — CNP = P = 2. GCO.

Hence if, as in the sextant, for every actual de-
ree of angular measure, two be marked on the
ale, the number marked on the scale will re-
resent the angle P. In the ordinary case, the
titude of a sun or star is to be taken, and the
orizon answers to the object s′. Where the
orizon is seen dimly a mercury trough can be
sed, and the reflection of the trough in it em-
oyed as s′. The altitude will evidently be half
the actual reading. For all instrumental de-
ils, and for a description of a convenient variety
this ordinary form of Hadley's sextant, by Pis-
r and Martins of Berlin, see Loomis' *Practical
stronomy*, pp. 96—102.

Sidereal. The sidereal day is the interval
tween the successive transits of the meridian
any star. The sidereal year is the interval
tween the sun's being in any particular position
mong the stars. See CALENDAR.

Signs. See ZODIAC.

Simoom. See WINDS.

Siphon. A very simple instrument of great
e in the arts. It has been sometimes also em-
oyed with advantage to turn aside the course of streams, when that was required for the construction of works of hydraulic engineering. In its simplest form, it consists of a bent tube A B, with unequal arms. The short arm A is dipt in a vessel of water until the top C becomes level with the water, which then flows over down the arm C B. The tube can then be raised until A is just below the water, all the rest of the tube being out of it. The flow will still continue.

Siren. An instrument invented by Cagniard
Tour, for ascertaining the number of vibra-
ns corresponding to any specified musical
nd. Its principle is this: suppose two circu-
plates, perforated by a corresponding number
small holes disposed along the circumference
circle smaller than their external diameter.
; the one plate be fixed in a horizontal posi-
, and the second plate laid over it (the two
faces being in contact) but connected with
chinery that can make it revolve with any
en speed. It is manifest, that, according to
speed with which the upper plate revolves,
l be the frequency with which, in a given
e, the perforations are in contact, so as to
blish a perforation through both plates. Below
lower plate is a tube, through which a con-
nt current of air is being forced; and, above
upper one, a suitable vessel. When the
er plate revolves, puffs of air will pass into the
er vessel, with a determinate frequency. As
velocity of revolution increases, these puffs
begin to produce a musical sound, rising in
h with the rapidity of that revolution. But
this rapidity of revolution is measured by
apparatus itself, the instrument will clearly enable us to state with precision the number of aërial vibrations required to produce the various notes. Other instruments, with the same object, have been proposed; none superior to the Siréne.

Sirius. The brightest star in the sky. It is α *Canis Majoris.* For its parallax, see STARS.

Sirocco. See WINDS.

Skew Arch is an arch whose face is oblique to the axis of the archway. Its figure is derived from that of a symmetrical arch by distortion in a horizontal plane, and is usually an arc of a circle or of an ellipse. The elevation of the face of a skew arch, and every vertical section parallel to its face, being similar to the corresponding elevation and vertical section of a symmetrical arch, the forces which act in a vertical layer or rib of a skew arch with its abutments, are the same with those which act in an equally thick vertical layer of a symmetrical arch with its abutments, of the same dimensions and figure, and similarly and equally loaded. Fig. 1 represents a plan of a skew arch, with counterforted abutments; and fig. 2, an elevation of the same arch, in a plane parallel to its faces. The *angle of skew*, or *obliquity*, is the angle which the axis of the archway, A A, makes with a perpendicular to the face, B C A B, of the arch. The span of the archway, "*on the square*," as it is called (that is, the perpendicular distance between the abutments), is less than the span *on the skew*, or parallel to the face of the arch, in the ratio of the cosine of the obliquity to unity. It is the span *on the skew* which is equal to that of the corresponding symmetrical arch. The span *on the square* being the width of the passage which a skew arch allows for the roadway or stream over which it is built, it is obvious that in arches of different degrees of obliquity, built across the same road or stream, the span *on the skew* varies proportionally to the secant of the obliquity; and if the breadth of the roadway above the arch, and the depth of the arch stones be both constant, the quantity of material in the arch varies as the secant of the obliquity also; and the horizontal thrust varies, as the square of the secant of the obliquity, according to the principle of the "transformation of structures,"— as to which, see *Proceedings of the Royal Society* for February, 1856, and Rankine on *Applied Mechanics.* It is in general advisable, however, to make the depth of the arch-stones of a skew arch increase proportionally to the span on the skew. The best position for the bed-joints of the arch-stones is perpendicular to the

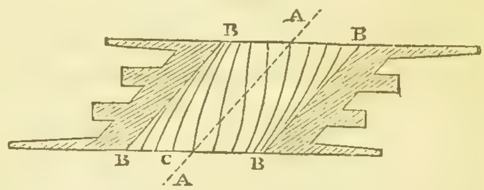


Fig. 1.

thrust along the arch. If, therefore, there be drawn on the soffit of a skew arch a series of parallel curves, made by the intersections of the soffit with vertical planes parallel to the face of the arch, the best forms for the bed-joints will be a series of curves drawn on the soffit of the arch, so as to cut the whole of the former series of curves at right angles, such as C C in figs.

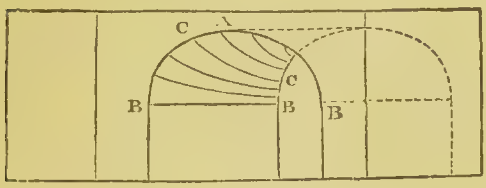


Fig. 2.

1 and 2. In order to draw the figures of the arch-stones, a drawing is made, representing the *development* of the soffit of the arch; that is to say, the appearance which the under side of the arch would present, if it were flexible, and were spread out flat on a plane surface. On this drawing are laid down a series of curves, parallel to the curves representing the developments of the lines of intersection of the faces of the arch with the soffit. A second series of curves, drawn with the free hand perpendicular to the first series, show the development of the figures of the bed-joints of the arch-stones, when these are made of the best form. The projections of those two series of curves on a plane parallel to the face of the arch, as well as their developments, cut each other at right angles. The execution of the best form presents some practical difficulty, owing to the arch-stones being of different thicknesses at different points. To avoid that difficulty, it is a common practice to use spiral bed-joints. These are laid down on the development of the soffit, by drawing a series of parallel straight lines as near as may be to the curved lines, which are the development of joints of the best form. The weakest parts of a skew arch are the stones at and near the acute angles of the abutments. Skew arches of great obliquity ought to be avoided as far as possible by the engineer, as being more expensive, more difficult to build, and less strong and stable than symmetrical arches.

Skew Bridges are those in which the passage beneath the bridge (whether for a road, railway, or water channel) and the passage over the platform of the bridge cross each other at an oblique angle. A skew bridge may contain one or more *skew arches*, of masonry or brickwork (as to which, see the preceding article); but it may also be constructed without skew arches, by supporting the platform on a series of arched ribs of brick, stone, iron, or timber, or of beams of timber or iron, each of which ribs or beams is symmetrical in itself, although their arrangement is oblique; or otherwise, by building a symmetrical archway of length sufficient to allow the oblique roadway, channel, or other passage above, to traverse it diagonally. This last method obviously involves a waste of ground, materials, and workmanship, unless some use can be made of the two triangular spaces which are left on the platform of the bridge at either side of the roadway.

Snow. When the temperature of the atmosphere approaches zero, snow falls instead of rain; the colder the air is, the less considerable is the fall, in consequence of the very small quantity of aqueous vapour that can exist at low temperatures. Snow has a very great variety of forms, all referable, however, to five leading types—*thin plates; a spherical or plane nucleus, bristling with radial spikes; fine spikes or prisms, with six edges; pyramids, with six faces; spikes or needles, terminated at either extremity or both, by a thin plate or leaf.* The fall of snow taking place only in regions where the temperature sinks below zero, it never snows at the equator except at great heights on the sides of mountains. In reference to this hydrometeor, Europe may be divided into three regions,—viz., the *first*, comprehending the South, where snow melts as it falls: the *second*, comprehending Northern France, Belgium, and Southern England, where snow showers do fall, and the snow lies: and the *third*, in which snow is found during great part of the winter; this comprehends all the Northern countries, and a large continent to the East, from Franconia and the eastern limits of the Black Forest as far as the plains of Hungary.—For line of perpetual snow, see TEMPERATURE.

Solar System. The name given to that assemblage of material orbs—the Earth being one—which revolve around our vast luminary the Sun, receiving from him light and heat, and retained in their orbits chiefly by his attraction. In all respects it is fitting that the system should receive its appellation from that central orb, seeing that, irrespective of the importance of his influences, his mass is so great as to exceed the masses of all his attendants nearly seven hundred and forty times. Various Inquiries regarding the Sun and his several Planets have been undertaken in other parts of this Cyclopædia: nevertheless there remains something to be stated concerning the system and its arrangements as a whole.

(1.) *The Bodies of which our System is Composed.*—These are exceedingly various. In former times we reckoned a few planets alone; but these planets are only the class of larger masses circulating round the sun; and, as such, the earliest seen. They are, as is well known, Mercury, Venus, the Earth, Mars, Jupiter, Saturn, Uranus, and Neptune,—some of them accompanied by satellites or moons. The breadth of space occupied by their orbits is enormous. Taking the earth's distance from the sun as unity, the mean distance of Mercury is ·3871, while that of Neptune is 30·0368. In other words, between the orbits of Mercury and Neptune there is a space

of the breadth of our distance from the sun—say ninety-five millions of miles—multiplied by 29·6497! Nor can it be strictly said that our system is confined even within that immense sphere. There may be planets beyond the orbit of Neptune; certainly there are material portions of the system within the circuit of Mercury: and who shall fathom the abysses through which Aristotle's comet may have penetrated—remaining all the while subject to the power of the Sun! The sphere of our system indeed, cannot be safely alleged to terminate until it reaches a remote limit, where the energy of its central Orb is balanced by that of some other fixed Star.—The discovery that first dispelled these ancient ideas concerning the simplicity and uniformity of the Solar System was that of the ASTEROIDS, of which, under the appropriate article, we have already given an account. The position of these curious bodies may best be indicated by the diagram below—copied from one in Galbraith and Haughton's valuable manual. The breadth of the sphere they occupy is very great—not less certainly than the distance between the Sun and the Earth. But then what numbers of orbs

swarm within that space! How strangely the orbits of many of them interlace! How infinitesimal they are, and in all respects how contrasted with a globe of the habitudes of Jupiter or even of the Earth! Yet *there* these bodies lie, occupying a distinct position, and doubtless with important functions of their own. Olbers, whose labours led in this singular course of discovery, imagined them the fragments of some orb that had burst asunder;—a hypothesis at once unphilosophical and untenable. Physical science recognizes *catastrophes* only in the last resort. Assuredly we have no title to ask their aid merely because some event occurs, or some object is descried unlike everything that had entered among our former knowledge. Nor is this hypothesis less objectionable in its relation to the principles of mechanics:—if a Planet were shattered by any cause, each fragment would in its new orbit return periodically to the spot where the catastrophe originally occurred,—a condition in no respect fulfilled by the Asteroids. Whatever the claims of the NEBULAR HYPOTHESIS (*q.v.*), it is much more likely that the formation of these minute worlds belongs to the manner and causes of the origin of the entire system.—The Asteroids however, are only our first step down amidst the variety constituting the scheme within which we are. We do not speak of the COMETS (*q.v.*), because, all things considered, they seem fitter to be regarded as chance visitors than as integrants of the planetary system: but, observe these multitudes of METEORS (*q.v.*), now flashing out in our midnight skies as separate *falling stars*, and again revealing, by periodic showers, the high probability that streams of them exist which, during certain intervals of years, intersect at regular seasons the orbit of the earth! Farther yet, we have that strange ZODIACAL LIGHT (*q.v.*), and, it may be, the still more evanescent interplanetary Ether, belonging probably to the fundamental conditions of our system, and seriously affecting its fates.—How utterly, in presence of such Variety, has all fancied simplicity disappeared! and how many ages must pass over Inquiry, ere the boast shall be realized that even the volume of our Planetary Astronomy may be closed! Not one of these phenomena is isolated or disparate. Every orb and atom is essential to the harmony and development of the whole; but, as yet, we have discerned the connections only of the ruder and more visible parts.

(2.) *Disposition or Arrangement of the Bodies composing the Solar System.*—Beside the general fact that the whole of this planetary matter of whatever form, appears to revolve around the Sun, there are a number of peculiar laws or marks of arrangement manifestly entering into the intimate or primal constitution of our System, and of which satisfactory theory will not fail to take account. Chiefly, these are the following:—1. Bode's law, or rather the law of Titius—a law founded on no known principle, but expressing actual relations so curious and numerous, that it is impossible to conceive it devoid of rational foundation. This principle has already been discussed under the suitable article. 2. Those strange Asteroids seem to constitute a marked division or boundary between two contrasted sets of planets. All within the asteroidal ring or zone, ought to be termed the *interior* planets, and those beyond it the *exterior*. The two classes are remarkably distinguished; while the individuals of each set are closely correlated in regard of their physical attributes. The contrasts and correspondences are mainly as follows:—The average specific gravity of the four inner planets is almost exactly the specific gravity of the earth, while that of the four outer ones is only one-fifth part of this quantity. The average diameter of the outer planets is 10·54 times that of

the inner ones: the period in which the external planets rotate on their axes is, for each, nearly ten hours; while the corresponding period in the case of the inner planets is very nearly the length of our own day. It is beyond the ring of the Asteroids that we find that grand display of secondary planets or moons; within the ring, our Earth alone has a satellite. Assuredly we must agree with the illustrious Humboldt, that the existence of two systems or groups of orbs, so separated and so strongly contrasted, is not without its significance and relation to the general harmony of the scheme. 3. The following facts are so important that they have long attracted profound attention:—the mere action of gravitation as a sustaining force explains none of these. *First*, Although the orbits of all the larger masses within the system (including the Asteroids), are *ellipses*, the *eccentricities* of these ellipses are so small, that, for general purposes, the orbits may be taken as *circles*. *Secondly*, These orbits lie almost in the same *plane*—the plane of the equator of the sun. Some of the Asteroids deviate by comparatively large quantities, but it is unquestionable that the Sun's equator plane is the normal plane of the system. *Thirdly*, The planets all revolve around the Sun in the same direction, and, in so far as is known, they rotate on their axes, likewise in that direction. *Fourthly*, The satellites are subject to all these arrangements, except those of *Uranus*, the orbits of which are nearly at right angles to the ecliptic, and whose motions are retrograde. *Fifthly*, The existence of these conditions is an essential element towards the stability of the planetary orbits, as already stated under PERTURBATIONS (*q. v.*)—These are the conditions on which Laplace founded his memorable NEBULAR HYPOTHESIS (*q. v.*)

(3.) *Motions of the Bodies in our System.*—These motions are those of *revolution* in an *orbit*, and *rotation* around an *axis*. The nature of the motion of *revolution*, was first laid down in KEPLER'S LAWS (*q. v.*) On these laws the immortal theory of GRAVITATION was built; and from that theory we have deduced the character and laws of the PERTURBATIONS,—subjects all already discussed under suitable heads in our Cyclopædia. As to the motion of ROTATION, one consideration alone remains worthy of notice. Whence the variety of the periods of this motion? Why is it that Jupiter performs his rotation in nine hours, fifty-six minutes, while the Earth occupies nearly twenty-four hours? Doubtless there is a law at the root of these varieties also; but, as yet, it has not been clearly discerned. A very ingenious American however, has recently started an idea meriting all consideration, and by which he attempts to attach these *specific periods*, to the Nebular Hypothesis. For clearer illustration, imagine the planets all ranged in a straight line from the Sun, and let us confine our thoughts to the three bodies, *Mars*, the *Earth*, and *Venus*. It is evident that a point must exist between Venus and the Earth, such that any atom of matter placed in it would be equally attracted towards either of these two orbs; and, in the same way, a similar *point of indifference* could be found between the Earth and Mars. Now the space between these two points may, correctly enough, be said to be the breadth of the celestial space which (among existing planetary arrangements), is controlled by *the attractive power of the Earth*; in other words, an atom, placed anywhere within that space, would—all other conditions being suitable—be *drawn* from a state of rest towards the EARTH, and not either to VENUS or MARS. Advancing a step farther, a RING, whose breadth is determined by the distance between the foregoing two points, may be supposed to environ the Sun—the earth's orbit being within it; and this ring may, for manifest reasons, be termed the *Sphere of the Attraction of the Earth*. Finally, arising in precisely the same manner, *every* planet may be conceived accompanied by its ring, the breadth of which will represent its *weight* in the system, or its relative attractive power: and these rings may be termed, as in the former case, the *Spheres of Attraction* of the several Globes.—It would be rash, indeed, to allege *à priori*, that the original primitive rings were, *as a matter of fact*, framed on the foregoing principle; and certainly the Inquirer would egregiously mistake the logical position of the nebular hypothesis who could venture to say that they *must* have been so formed. No demonstrative result so minute or satisfactory, can possibly follow, from any knowledge attainable by man, regarding that remarkable speculation; and the attempt to detect such results, as well as the dogmatic demand that they be detected, are alike inconsistent with the only practicable position we can occupy in regard to it: nevertheless this much may be said: "*The partition of the original nebula into a number of rings, with such breadths or nearly such, is perfectly consistent with the fact, that the said rings have ultimately resolved themselves into planets of the relative order and magnitude, distinguishing those now existing.*" Not only is this new supposition not hostile to the general tenor of the Nebular Hypothesis; but, on the contrary, it is altogether consistent and congruous with it.—We reach the remarkable and conclusive result. Mr. Kirkwood of Pennsylvania, who first started the idea of *these spheres of attraction*, has found in them the *periods* of the *rotations* of the several PLANETS. The law announced by him is this, —viz., "*The* SQUARE *of the number of times that each planet rotates during one revolution in its orbit*, is proportional to the CUBE *of the breadth or diameter of its sphere of Attraction.*"

Solid. A body whose particles are held together, so that an appreciable force is required to

separate them. Also a geometrical term for a body with length, breadth, and thickness. Solids are divided into irregular, as most of those in nature are—and regular—as many crystalline forms. What are called regular solids are terminated by regular and equal planes. There are only five *regular* solids,—the tetrahedron, bounded by four equal triangles,—the cube or hexahedron, by six equal squares,—the octahedron, by eight equal triangles,—the dodecahedron by twelve, and the icosahedron by twenty.

Solstice. For the meaning of solstice, see SEASONS.

Sothiac Period. See CYCLE.

Sound. Under ACOUSTICS, all the general phenomena of sound have been adverted to. Had space permitted, we should have occupied the reader in this place with some late controversies regarding the mode of propagation of sound. But we simply refer to the recent numerous papers by Professors Stokes, Rankin, Haughton, and Potter, in the *Philosophical Magazine.* The common and now *old* theory, as delivered by Laplace, has certainly not been invalidated; and that the heat evolved during the wave motion, is exactly adequate to bring up Newton's velocity to the observed or measured velocity, has been experimentally established by M. Joule. One omission in ACOUSTICS, is noticed in our Appendix.—It may just be stated, that as it has been long known that waves of sound are subject to reflection, and further, that these *interfere* with each other, so there now remains little doubt that they are subject to *refraction* also.

Specific Gravity. If a given mass of a body weigh m times as much as the same mass of water, m is called the specific gravity of the body. Air is taken as the unit for gases, and a gas of which, a given volume under any definite pressure weighs m times as much as the same volume of air under the same pressure, is said to have the specific gravity m. The readiest method of finding the specific gravity of a solid is by means of the HYDROSTATIC BALANCE,—that for a liquid by means of a flask, which is filled successively with water and with the liquid, and gases may be collected and compared in a similar way.

Specific Heat. Where a given amount of heat raises a substance m degrees, and raises water n degrees, the specific heat of the substance is $\frac{n}{m}$. Hence, evidently, the specific heat is a measure of the comparative amount of Heat required to raise a substance through one degree of temperature.

Spectacles; consist simply of lenses so arranged as to be conveniently applied to the eyes, the purpose of which is to aid distinctness. The properties of these LENSES are discussed under that article. See PERISCOPIC.

Spectrum. The name given to the contents of a Solar beam, in so far as they can be separated and the beam resolved into its simple elements.—This beam or ray produces three distinct species of effects, and when the constituents of the beam are dispersed by Refraction or by the Prism, these effects are found to proceed from different portions of the longitudinal space filled by that dispersion. The effects in so far as they are known to us, are,—the Actinic effects, or the power to produce chemical changes—the excitation of the sensation of Light and Colour—and the power of Heating. The light-giving portion of the Spectrum lies between the other two. The actinic power, beginning within the visible spectrum, stretches quite beyond it at its violet extremity: the heating power, on the contrary, likewise beginning within the visible spectrum, also stretches beyond it, but towards the red or opposite extremity. We shall briefly study the distribution of these three effects, taking them in the following order—the Actinic, the Thermal, and the Light-giving.

I.

The reality of a prolongation of the spectrum beyond the upper or most refrangible rays had long been suspected. Very acute eyes seem to have detected shades of colour in certain circumstances outside the violet colour, very faint indeed, but of a lavender shade. Recently, however, the existence of such rays has been placed beyond doubt by two classes of discoveries.—(1), Photographic changes can be produced by influences flowing from that extra or invisible space, as well as by the more refrangible visible rays: and (2), Rays whose position is in that same portion of space may, as Mr. Stokes has shown, be in a certain sense rendered visible. The remarkable interpretation put by this very eminent physicist on the curious phenomena named Fluorescence by some, and Epipolic Dispersion by Sir John Herschel, has already been explained under article DISPERSION EPIPOLIC; nevertheless, it may not be unacceptable that we briefly sum up the discovery in this place. When the violet ray and those higher and visible rays enter certain liquids near their surface—for instance, a solution of the sulphate of quinine—a pale blue colour is seen about the surface of the solution. The explanation is, that the fluid into which these rays enter is caused by them to emit a mixed light of lower periods. The violet, or higher ray, does not pass through the liquid and issue a pale blue; it merely, as we have just said, causes a thin layer of the liquid near the surface to emit a faint blue light, seen in all directions, even through the liquid itself, which is transparent for this pale blue light, although not for the violet light in which it originates. It can scarcely be said that the rigorous analysis of this supra-violet space is more than entered on; nevertheless, the probability is, that the ultimate result will coincide with what is established beyond doubt in the case of Thermal effects—viz.:

that it is filled with the same Ether which gives rise to Light and Colours; only vibrates more rapidly, and diffuses itself by means of shorter waves. In an interesting memoir in *Poggendorf's Annalen*, part xcviii., Esselbach thinks he has established that the undular length of the extremest fluorescent rays yet known to us is 0·0003 millimeters.—Our next inquiry, however, is at length freed from all uncertainty and vagueness.

II.

Under articles **Radiant Heat**, **Diathermancy**, &c., an account has been given of the memorable researches of Melloni, confirmed, and in some cases anticipated by Professor Forbes.—The closest similarity between what were then termed the Heating rays and the Light-giving rays, in all their affections, was placed beyond doubt by that brilliant series of discoveries. The rays of Heat, whether in the Solar Beam or rising from some *obscure* heated source, can be reflected, refracted, dispersed, and polarized precisely as a ray of Light. But a vital question still remained at issue. Is the ray of Heat an *independent* although *analogous* ray? Is the spectrum merely a compound spectrum, consisting of a Light-giving spectrum and a Thermal spectrum, distinct although partially superposed? or is it one single spectrum, merely varying at different parts of the space over which it is dispersed, by the fact of a slower or more rapid vibration of one and the same Ether? In short, are we entitled to lay down the general proposition, "*All effects, Actinic, Light-giving, Thermal, flow from one and the same physical agency—viz., from the propagation of vibrations having different velocities or periods, through the same elastic Ether?*" If the affirmative be established, the following proposition may be laid down:—"The Light-giving effect is confined within limits in the dispersed ray, because—as with the ear—the eye can be affected only by vibrations within certain limits of rapidity: some vibrations do not affect the eye, but they affect our other senses through their heating power; while vibrations of the higher range appear chiefly through their chemical influences." Melloni, in the earlier stages of his inquiries, was disposed to accept the theory of two superposed spectra; but he subsequently gave a modified assent to the doctrine now almost universally received. The truth of the doctrine seems indeed to be placed beyond reach of question by the recent elaborate researches of Masson and Jamin—these eminent physicists having demonstrated that, *if on any part of the visible spectrum any physical change be impressed affecting its* LIGHT-GIVING *power, precisely the same change will be found to have passed over its* HEATING *power.* For instance, take the method of absorption. Destroy by the intervention of coloured media, the light of any of the colours of the spectrum; the heat belonging to that colour is destroyed also. Or inversely, take any coloured medium—such as red glass, certain green glasses, a solution of sulphate or bichromate of copper in ammonia—which can be traversed by one colour only, every such medium permits the heat belonging to the special colour to pass, and it extirpates or intercepts all other heat. Again, employing a glass of blue cobalt, which is known to divide the visible spectrum into bright and obscure strips or bands—the same bands exist as to its heating effects. The same Inquirers have however gone further, and occupied themselves with still more delicate researches. They polarized the coloured ray on its issuing from the prism; they caused it to pass through quartz of different thicknesses and through solutions of sugar, and they found invariably that the planes of polarization of Light and Heat deviate in the same direction, and by the same amount. Also, three thin plates of $\frac{1}{4}$, $\frac{1}{2}$, and 1, the length of a wave, presented the same interferences for both classes of phenomena; the first giving a circular polarization, the second a plane polarization in a plane at right angles, and the third leaving the emerged ray polarized as the radiant ray.—Repeating the foregoing general proposition in terms more specific, Masson and Jamin conclude thus:—"In all phenomena produced by a radiation of definite and distinctive refrangibility at once Thermal and Luminous—the relations of the quantities of Heat and of Light, before and after the action, are identical. All vibratory modifications established in regard to Light, hold with the same intensity, and the same numerical value in the case of Heat. And this invariable correspondency of Effects, compels us to admit an identity of Causes."

2.—The inquiry next demanding attention relates to the dispersion or longitudinal distribution of this heating influence through the spectral space. In *Poggendorf's Annalen*, part ci., the student will find a clear *resumé* of all the investigations undertaken in relation to this interesting subject. The most recent and the most complete are by J. Müller, and to an account of these we shall here confine ourselves. Müller first proceeded by aid of absorption; but he subsequently adopted the much more accurate method of estimating the heat in successive strips or portions of the visible spectrum, and in the space beyond the red, by means of a delicate and powerful lineal thermo-multiplier. He also took the essential precaution to compare, previous to making his experiments, the various deflexions of the needle, with the force of the currents causing these deflections. He began by using for dispersion a fluid glass prism, and in the spectrum so produced he detected the following thermal effects:—

Boundary of indigo and violet,	2
Middle of the blue,	4
Middle of the yellow,	7
Middle of the red,	10
1‴ beyond body of red,	12
2‴ do. do.	11
4‴ do. do.	7
6‴ do. do.	2

But inasmuch as glass absorbs portions of the dark heating rays, or is not perfectly diathermanous, Müller next resorted to a prism of rock-salt, which does not impede the passage of any of these rays. The following are the mean of his results:—

In the blue,			3·7
In the yellow,			7·9
In the red,			10·0
1‴ in the invisible,			13·2
3‴	do.	do.	15·9
4‴	do.	do.	13·2
6‴	do.	do.	1·7

The nature of these results will be best seen by the aid of a graphic representation. In the line R S the bracket is supposed to include the entire visible spectrum, the space to the right of B being the dark heating space. For the sake of

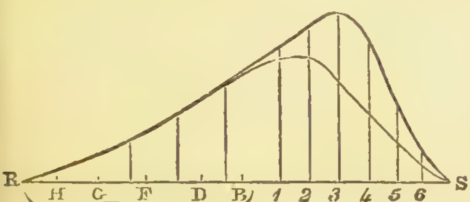


Fig. 4.

distinctness, the positions of the chief of Fraunhofer's lines, H, G, F, &c., are laid down. If the ordinates or perpendiculars represent the measured quantities of heat at each of the ponds from which they spring, the curves will be the curves of intensity. The lower curve is that given by the flint-glass prism; the upper one—of course the line curve—represents the results obtained by a prism of rock-salt. It will be noticed that Franenhofer's line B, lies about the middle of the line R S. Now, the refractive ndex for the line H is about 1·546, and for B 1·526. Taking this proportion as our guide, the refractive index for the rays at the extremity of the dark thermal spectrum would be 1·506. But from these separate indices the sensible length of the undulation of the ether at S may be computed. Müller, in the end, employs a very simple process. Slightly deviating from his earliest method, he adopts the formula of Cauchy,

$$\frac{1}{n^2} = a + \frac{c}{\lambda^2},$$

which expresses the relative position between n, the index of separation, and λ, the length of the wave; or the more accurate equation,

$$\frac{1}{n^2} = a + b\,\lambda^2 + \frac{c}{\lambda^2},$$

and determines the constants a, b, and c, by equations of condition: this result is—adopting Esselbach's conclusion—that we can distinctly recognize within the extreme limits of the fluorescent and thermal spectra, waves of the following lengths, estimated in millimeters:—

0·0003; 0·0006; 0·0012; 0·0024; 0·0048.

Consequently the solar spectrum, as at present analyzed, includes Four Octaves; and, of these Four Octaves, the visible part extends over one only.—It must not be rashly concluded, however, that there are no vibrations outside these two limits. They are merely the limits of the vibrations as yet recognized by us.

We owe to the kindness of Professor Stokes a plate and description of a series of lines he has found in the invisible part of the spectrum, made visible by the use of dilute tincture of turmeric. Part of the ordinary spectrum is included in the plate, to exhibit the relative scale of the two portions.

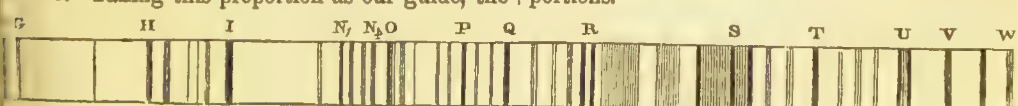


Fig. 5.

There follows a group of six nearly equidistant ines or bands, of which the first is rather hazy, and tuated at a distance from the second, somewhat ess than the mean interval of the lines of the roup; the third, marked P, is more conspicuous ian its neighbours; the last, marked Q, is rearkably black, and well defined. After a faint oup follows the group R, which, according to rcumstances, is seen as a dusky band, or is solved into four lines (the third of which is arked R), of which the two more refrangible e darker and sharper than the other two, and e also somewhat closer together. The interval tween the groups R and S is faintly shaded th inconspicuous lines. About half-way been these groups is a space brighter than usual, consequence of its freedom from fixed lines, ich accordingly, in negative photographs, nes out as a striking dark band. S is a very iking group, consisting of three dark bands, separated by rather shady intervals. The second of these bands, marked S, divides the interval between the first and third, in the proportion of almost three to two. After two lines not remarkable, comes the line T, which is a remarkably black and well-defined band, of sensible breadth. T is separated by three faint lines from U, which is a remarkably conspicuous double band, of some breadth. V is also conspicuous. When the sun is high, as during the middle hours of the day, throughout the summer months, and is shining clearly, the lines are seen without any difficulty, as far as U, or even V; but the light then becomes so very faint, that the last two lines in the group are barely visible. The rapidity with which the illumination decreases at the end of the spectrum is remarkable.

III.

III. Let us now turn to an analysis of the purely *Luminous Spectrum.*

SPE

I. The Spectrum of Dispersion, as manifested by the Prism.—The analysis of the Luminous Spectrum, through agency of Refraction, we owe to Sir Isaac Newton. The phenomena of the process are described as follows by Sir John Herschel, in the *Treatise on Light*, originally contributed to the *Encyclopædia Metropolitana*,—a treatise requiring only a degree of revisal by the same master hand, to be not only the most satisfactory dissertation existing in our language on that important department of Physics, but also perhaps the finest production of his classic pen.—" When a ray of light falls obliquely on the surface of a refracting medium, it is not refracted entirely in one direction, but undergoes a separation into several rays, and is *dispersed* over an angle more or less considerable, according to the nature of the medium and the obliquity of incidence. Thus, if a sunbeam S C be incident on the refracting surface A B, and be afterwards received on a screen R V, it will, instead of a single point on the screen as R, illuminate a space R V *of a greater extent* the *greater* is the angle of incidence. The ray S C, then, which, before refraction was single, is separated into an infinite number of rays C R, C O, C Y, &c., each of which is refracted differently from all the rest.—The several rays of which the dispersed beam consists, are found to differ essentially from each other, and from the incident beam, in a most important physical character. They are of different colours. The light of the sun is white. If a sunbeam be received directly on a piece of paper, it makes on it a white spot; but if a piece of white paper (that is, such as by ordinary daylight appears white) be held in a dispersed beam, as R V, the illuminated portion will be seen to be differently coloured in different parts, according to a regular succession of tints which is always the same, whatever be the refracting medium employed.—To make the experiment in the most striking and satisfactory manner, procure a triangular prism of good flint glass, and having darkened a room, admit a sunbeam through a small round hole, O P, in the window shutter. If this be received on a white screen D at a distance, there will be formed a round white spot, or image of the sun, which will be larger as the paper is farther removed. Now, in the beam before the screen, place the prism A B C, having one of its angles C downwards and parallel to the horizon, and at right angles to the direction of the sunbeam; and let the beam fall on one of its sides, B C, obliquely. It will be refracted and turned out of its course, and thrown upwards, pursuing the course F G R, and may be received on a screen E properly placed, But on this screen there will no longer be seen a white round spot, but a long streak, or, as it is called in optics, a *spectrum* R V of most vivid colours, (provided the admitted sunbeam be not too large, and the distance of the screen from the prism considerable). The tint of the lower or *least refracted* extremity R is a brilliant red, more full and vivid than can be produced by any other means, or than the colour of any natural substance. This dies away first into an orange, and then passes by imperceptible gradations into a fine pale straw-yellow, which is quickly succeeded by a pure and very intense green, which again passes into a blue, soon deepening to the purest indigo. Meanwhile, the intensity of the illumination is diminishing, and in the upper portion of the indigo tint is very feeble, but it is continued still beyond, and the blue acquires a pallid cast of purplish-red, or livid hue, more easily seen than described, and which, though not to be exactly matched by any natural colour, approaches most nearly to that of a fading violet: '*tinctus viola pallor.*'—If the screen on which the spectrum be received have a small hole in it, too small to allow the whole of the spectrum to pass, but only a very narrow portion of it, as X, the portion of the beam which goes to form that particular spot X may be received on another screen at any distance behind it, and will there form a spot D of the very same colour as the part X of the spectrum. Thus, if X be placed in the red part of the spectrum, the spot D will be red; if in the green, green; and in the blue, blue. If the eye be placed at D, it will see through the hole an image of the sun, of dazzling brightness; not, as usually, white, but of the colour which goes to form the spot X of the spectrum. Thus we see, that the joint action of all the rays is not essential to the production of the coloured appearance of the spectrum, but that one colour may be insulated from the rest, and examined separately. If, instead of receiving the ray X *d*, transmitted through the hole X, on a screen immediately behind it, it be intercepted by another prism *a c b*, it will be refracted, and bent from its course, as in X *f g x*; and after this second refraction may be received on a screen *e*. But it is now observed to be no longer separated into a coloured spectrum like the original one R V, of which it formed a part. A single spot X only is seen on the screen, the colour of which is uniform, and precisely that which the

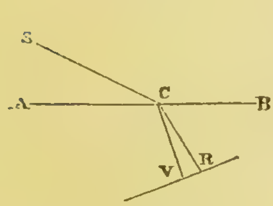


Fig. 1.

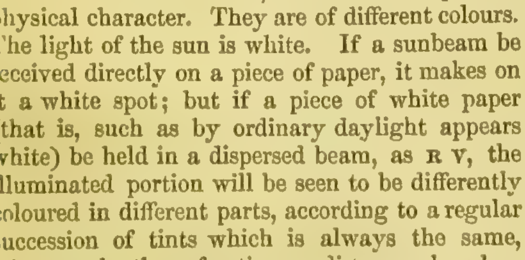


Fig. 2.

part x of the spectrum would have had were it intercepted on the first screen. It appears, then, hat the ray which goes to form any single point of the spectrum, is not only independent of all he rest, but having been once insulated from hem, is no longer capable of further separation nto different colours by a second refraction.—This simple, but instructive experiment, then, nakes us acquainted with the following propers of light:—1. A beam of light consists of a reat and almost infinite variety of rays differing om each other in colour and refrangibility.—For e ray S F from any one point of the sun's disc, hich, if received immediately on the screen, ould have occupied only a single point on it, (supposing the hole in the screen to have a nsible diameter) only a space equal to its area, dilated into a line V R of considerable length, ery point of which (speaking loosely) is illuminated. Now the rays which go to V must cessarily have been more refracted than those hich go to R, which can only have been in tue of a peculiar quality in the rays themves, since the refracting medium is the same all.—2. White light may be *decomposed*, *anaed*, or separated into its elementary coloured s by refraction. The act of such refraction is led the *dispersion* of the coloured rays.—3. ch elementary ray once separated and insued from the rest, is incapable of further deposition or analysis by the same means. True, may place a third, and a fourth, prism in the y of the twice refracted ray of *x*, and refract n any way, or in any plane; it remains unersed, and preserves its colour quite unaltered. . The dispersion of the coloured rays takes e in the plane of the refraction; for it is found the spectrum V R is always elongated in this e. Its breadth is found, on the other hand, measurement, to be precisely the same as that he white image D, of the sun, received on a en at a distance O D from the hole, (fig. 2) l to O F + F G + G R, the whole course of refracted light, which shows that the beam undergone no contraction or dilatation by the t of refraction in a plane perpendicular to plane of refraction." In the portion of same article immediately succeeding, Sir Herschel offers instructions concerning the ipulation requisite for the evolution of a spectrum. As no instruction can be of ter consequence to the Inquirer into the titution of the spectrum, we quote his words y at length.—"Refraction by a prism ls us the means of separating a ray of light into the rays of different refranty of which it consists, or of analyzing it. to make the analysis complete, and to insua ray of any particular refrangibility in a of perfect purity, several precautions are red, the chief of which are as follows:—1st. beam of light to be analyzed must be very , as nearly as possible approaching to a mathematical ray; for if A B, *a b* (fig. 3) be a beam of parallel rays of any sensible breadth incident on the prism P, the extreme rays, A B, *a b*, will each be separated by a refraction into spectra G B H and *g b h*: B G, *b g*, being the violet, and B H, *b h*, the red rays of each respectively; and since A B, *a b*, are parallel, therefore C G and *c g* will be so, and also D H and *d h*. Hence the red ray D H from B will intersect the violet *c g* from *b*, in some point F behind the prism; and a screen E F if placed at F will have the point F illuminated by a red ray from B, and a violet one from *b*; and therefore (as is easily seen) by all the rays intermediate between the red and violet, from points between B and *b*. F therefore will

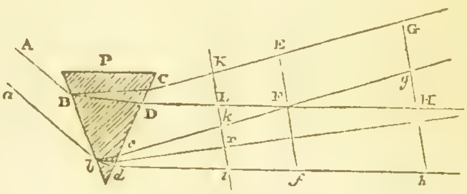


Fig. 3.

be white. If the screen be placed nearer the prism than F as at K L *k l*, it is clear that from any point between L and K lines drawn parallel to K *l*, D L, to any intermediate direction, will fall between C and *c*, D and *d*, &c., respectively; and therefore that every point between L and *k* will receive from some point or other of the surface C *d* of the prism a ray of each colour, and will therefore be white. Again, any point as *x* between *k* and *l* can receive no violet ray, nor any ray of the spectrum whose angle of deviation is greater than 180°—*a b x*; for such ray to reach *x* must come from a part of the prism below *b*, which is contrary to the supposition of a limited beam A B, *a b*; but all rays whose angle of deviation is less than 180°—*a b x* will reach *x* from some part or other of the surface B D. Hence the colour of the portion *k l* of the image on the screen will be white at *k*, pure red at *l*, and intermediate between white and red, or a mixture of the *least* refrangible rays of the spectrum at any intermediate point; and, in the same manner, the portion K L will be white at L, violet at K, and at any intermediate point will have a colour formed by a mixture of a greater or less portion of the more refrangible end of the spectrum. If the screen be removed beyond F as into the situation G *g* H *h*, the white portion will disappear, no point between *g* and H being capable of receiving any ray whose angle of deviation is between 180°—*a b g* and 180°—*a b* H. We may regard the whole image G *h* as consisting of an infinite number of spectra formed by every elementary ray of which the beam A B *a b* is composed, overlapping each other, so that the end of each in succession projects beyond that of the foregoing. The fewer, therefore, there are of these overlapping spectra, or the smaller the

breadth of the incident beam, the less will be the mixture of rays so arising, and the purer the colours. Removal of the screen to a greater distance from the prism, evidently produces the same effect as diminution of the size of the beam; for while each colour occupies constantly the same space on the screen (for G g = K k), the whole spectrum is diffused over a larger space as the screen is removed, by the divergence of its component rays of different colours, and therefore the individual colours must of necessity be continually more and more separated from each other.—2dly. Another source of confusion and want of perfect homogeneity in the colours of the spectrum is the angular diameter of the sun or other luminary, even when the aperture through which the beam is admitted is ever so much diminished. For let S T be the sun, whose rays are admitted to the prism A B C through a very small hole O in a screen placed close to it. The beam will be dilated by refraction into the spectrum v r. Now,

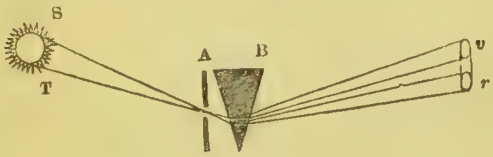


Fig. 4.

if we consider only the rays of one particular kind, as the red, and regard all the rest as suppressed, it is clear that a red image r of the sun will be formed by them alone on the screen; the rays from every point of the sun's disc crossing at O, and pursuing (after refraction) different courses. If the prism be placed in its position of minimum deviation, which at present we will suppose, this image will be a circle, and it and the sun will subtend equal angles at O. In like manner, the violet rays (considered apart from the red) will form a circular violet image of the sun, at v, by reason of their greater refrangibility; and every species of ray, of intermediate refrangibility, will form, in like manner, a circular image between r and v. The constitution of the spectrum so arising will therefore be as in fig. 4, a being an assemblage of images of every possible refrangibility superposed on and overlapping each other. Now, if we diminish the angular diameter of the sun or luminary, each of these images will be proportionally diminished in size; but their number, and the whole extent over which they are spread, will remain the same. They will therefore overlap less and less; and if the luminary be conceived reduced to a mere point, (as a star,) the spectrum will consist of a line d composed of an infinite number of mathematical points, each of a perfectly pure homogeneous light. There are several ways by which the angular diameter, or the degree of divergence of the incident beam may be diminished. Thus, first, we may admit a sunbeam through a small hole, in a screen, and receive the divergent cone of rays behind it on another screen, at a considerable distance, having another small hole to let pass, not the whole, but only a small portion of the sun's image. The beam so transmitted, will manifestly have a degree of divergence less than that of the beam immediately transmitted from the first screen, in the proportion of the diameter of the second aperture, to the diameter of the sun's image on the first screen.—Another and much more commodious method is to substitute for the sun its image formed in the focus of a convex lens of short focus. This image is of very small dimensions, its diameter being equal to focal length of the lens × sine of sun's angular diameter, (or sine of 30′, which is about one 114th part of radius,) so that a lens of an inch focus concentrates all the rays which fall on it within a circle of about the 114th of an inch in diameter, which, for this purpose, may be regarded as a physical point. It should, however, be remarked, that the intensity of the purified ray, and the quantity of homogeneous light so obtained, are diminished in the same ratio as the purity of the ray is increased.—A third method of obtaining a homogeneous beam is to repeat the process of analysis on a ray as nearly pure as can be conveniently obtained by refraction through a single prism. Thus the spectrum formed by a first refraction is received on a screen which intercepts the whole of it, except that particular colour we wish to insulate and purify, which is allowed to pass through an aperture; behind this is placed another prism, so as to refract this beam a second time. If, then, the portion transmitted were already perfectly pure, it would pass the second prism without undergoing any further separation; but if there be (as there always will) other rays mixed with it, these will be dilated by the subsequent refraction into a second spectrum of faint light, with a much brighter portion in the midst, and if the rest of the ray be interrupted, and this portion only allowed to pass through an aperture, the emergent beam will be much more homogeneous than before its incidence on the second prism, and in proportion as the distance between the second prism and the screen is increased, the purity of the ray obtained will be greater.—Another source of impurity in the prismatic rays is the imperfection of the materials of our ordinary prisms, which are full of striæ and veins, that disperse the light irregularly, and thus confound together, in the spectrum, rays which properly belong to different parts of it. Those who are not fortunate enough to possess glass prisms free from this defect, (which are very rare, and, indeed, hardly to be procured for any price,) may obviate the inconvenience by employing hollow prisms full of water, or, rather, any of the more dispersive oils. A great part of the inconvenience arising from a bad prism may, however, be avoided by transmitting the rays *as near the edge of it as*

possible, so as to diminish the quantity of material they have to pass through, and therefore their chance of encountering veins and striæ in their passage."—Besides these general rules, as laid down by Sir John Herschel, there are other precautions quite necessary, so that a pure spectrum be obtained; or if not absolutely pure, one sufficiently pure to entitle it to rank as evidence in very delicate inquiries. The imperfection of the glass of the prism, as noticed above, cannot probably be wholly got rid of. Let a prism be perfect as it may, there are scratches on its polished surface, and minute stray points in its interior; and these so disperse the incident light, that no absolutely pure spectrum can be expected. If this cause of error cannot be wholly removed or counterbalanced, there is another of similar effect which may easily be destroyed. This cause of uncertainty—first indicated by M. Helmholtz—is the *repeated reflexion of the light within the prism.* In the majority of prisms used for experiments on dispersion, the two refracting surfaces alone are polished, the other three being ground dull. If such a prism be placed upon a dark ground, so that the dark surface shall be illuminated, then within the prism a series of reflected rays of this surface is observed. The two polished sides act as an angular medium which exhibits a series of circular images of any object placed between its reflectors. In the case under consideration, the third surface occupies this position, and we look through one of the reflectors into the interior. The reflected images of the third surface appear in exactly *the same direction* as the spectra which are observed on looking through the prism; and as a portion of the incident light usually falls upon the third surface, illuminating it and its images, a *weak white luminosity* is thus created, which spreads itself over the spectrum. The quantity of the reflected light is certainly very small, and in general will not be at all observed beside the regularly refracted light. To cut it off, it is necessary to *blacken all the surfaces well, except the two refracting ones.*—The spectrum thus developed has been the subject of much scrutiny. The most important discovery regarding it—at least until quite recently—being that of the dark lines. See FRAUENHOFER'S LINES. These singular lines are invaluable in Optics, furnishing fixed positions within the separate colours, by which may be determined, with every accuracy, the refrangibility answering to their several positions. Having already given these determinations under DISPERSION, we shall not repeat them here. But it may be advantageous that we enumerate the fundamental propositions to which—as contemplated by the Undulatory Theory of Light—the phenomena of Dispersion give rise. They are mainly FOUR.—*First:* The doctrine of the Propagation of Light by Undulations, has been shown by M. Cauchy, to comprehend the fact of *Dispersion.* The various waves of which the solar beam is made up are not propagated onwards with exactly the same velocities; and the phenomena of *Dispersion* arise out of these varying velocities. The fact of *Refraction* assumes a proportional change in the velocity of the impinging wave, on its entering a new medium; and as such changes must be different with regard to the different components of the wave, we have necessarily, the separation of these components or their *dispersion.*—*Secondly: Colour* within the spectrum, and a special degree of *Refrangibility*, are *inseparable.* To the *violet* colour, and to the *red* colour, *specific refrangibilities* belong, as certainly and essentially as their *specific tints. Colour*, it must be recollected, is by itself purely *subjective:* it is a conception originating in an action on the retina, interpreted by the perceptive faculty. The *physical action* is indicated by the refrangibility of the ray; which again depends on the rapidity of the vibrations of the molecules of the ether that conveys the wave. *Colour*, in this respect, is analogous to *Sound.* And as the elementary colours indicate specific velocities of vibration, it is clear that no elementary colour can be decomposed by subsequent refraction.—*Thirdly:* Just as the notes of music, although in themselves distinct, merge insensibly into each other, so do the separate colours. Hence the impossibility of assigning absolute limits to any coloured space in the spectrum. Strictly speaking, the solar ray is composed not of seven colours only, but of an infinite variety:—to every separate shade or tint however, belongs its special refrangibility.—So fundamental is this fact, that if a change of colour should appear under any circumstances, the most natural inference would be, that it was caused or accompanied by a change of refrangibility—a subject referred to below.—*Fourthly:* Enough has perhaps been said already in reference to the discovery by Mr. Stokes. It may be well, however, to record here the specific propositions with which he closes his memoir:—"(1.) In the phenomenon of true internal dispersion the refrangibility of Light is changed; incident light of definite refrangibility giving rise to dispersed light of various refrangibilities.—(2.) The refrangibility of the incident light is a superior limit to the refrangibility of the component parts of the dispersed light.—(3.) The colour of light is in general changed by internal dispersion, the new colour always corresponds to the new refrangibility. It is a matter of perfect indifference whether the incident rays belong to the visible or invisible parts of the spectrum.—(4.) The nature and intensity of the light dispersed by a solution appear to be strictly independent of the state of polarization of the incident rays. Moreover, whether the incident rays be polarized or unpolarized, the dispersed light offers no traces of polarization. It seems to emanate equally in all directions as if the fluid were self-luminous.—(5.) The phenomenon of a

change of refrangibility proves to be extremely common, especially in the case of organic substances, such as those ordinarily met with, in which it is almost always manifested to a greater or less degree.—(6.) It affords peculiar facilities for the study of the invisible rays of the spectrum, more refrangible than the violet, and of the absorbing action of media with respect to them." It is not more than a bare acknowledgment of the truth, to say that these propositions—whether regarded in themselves or their undoubted effects—are the most important contributions made in recent times to Physical Optics.—Doubtless the student will remark that the phenomena suggest no hesitation as to the connection between specific colour and specific refrangibility; rightly interpreted, they do the very reverse.

II. THE ANALYSIS OF THE SPECTRUM BY DIFFRACTION.—The Phenomena and Laws of *Diffraction* have been fully explained under the appropriate article. See further, DIFFRACTION in Appendix.—If an ordinary ray of Light be subjected, by any of the methods described, to the operation of Diffraction, we have a succession or series of spectra. These spectra, examined by a telescope, exhibit all the usual colours and intermediate tints, and also the dark lines of Wollaston and Frauenhofer. But they differ from the ordinary spectra in one important particular. The form of the latter (the relative spaces, *i. e.*, occupied by the various colours)—changes with the matter of the prism and the position of the screen: the character of spectra of Diffraction, on the contrary, is determined by one element alone, *viz.*, the lengths of the undulations corresponding to each colour. On the ground of their developments, Frauenhofer calculated the lengths of the waves corresponding to the principal fixed lines in the spectrum; and the values he obtained must be accounted the most exact values yet extant, of these optical constants.—It scarcely requires to be remarked that the entire theory of Diffraction, confirmed in every instance by experiment, depends, (as well as the theory of Dispersion,) on the identity of the objective cause of colour, with the refrangibility of the various portions of the spectrum.—The phenomena of POLARIZATION have not in the main much direct relation to Dispersion; nevertheless they also throw a certain light on this subject. The different colours have slightly different polarizing angles (see HAIDINGER'S FRINGES); hence a slight dispersion. The analysis of the spectrum, in so far as the production of colour is at all involved in the phenomena of Polarization, yields the results established by the two former methods. Beyond these there appears no mode of detecting the elements of a ray of light.

III. THE SPECTRUM AS ANALYZED BY SIR DAVID BREWSTER.—A view of the constitution of the Solar Spectrum has been placed before the world by the very eminent physicist just named, which, if well founded, not only upsets the analysis of Sir Isaac Newton, but renders nugatory all supposed advances in the Theory of Light since the times of Huyghens. Sir David considers that the experiments he has detailed utterly dissolve connection between the *Refrangibility* of a ray and its *Colour*;—a blow as fatal to the doctrine of the Propagation of Light by Undulations, as it would be to the grand law of our Celestial Mechanism, if that bond could be severed which connects the *Attractive Influence* of a Planet, with its *Mass.*— In Sir David Brewster's opinion, the spectrum consists of three simple colours overlapping, or rather superimposed. Each colour extends across the entire length of the spectrum, and therefore includes within itself rays of all degrees of refrangibility; but the colours are so distributed in regard to intensity, that the *red* light contains a preponderance of rays of least refrangibility, the *yellow* more rays of mean refrangibility, and the *blue* of greatest refrangibility. In other words, let the curve whose apex is R indicate by its ordinates the intensities of the *red* at each part of it, the curve Y the intensities of the *yellow*, and the curve B the intensities of the *blue*, then the seven colours of the spectrum are formed by the overlapping and proportional admixture of all these. It is clear enough that while each line or portion of the spectrum would continue as before to preserve its specific refrangibility, the supposed connection between simple colour and specific refrangibility must be held a delusion, if this formidable analysis prove true.—The speculation as to the existence of three primitive tints, or rather three tints by whose intermixture all

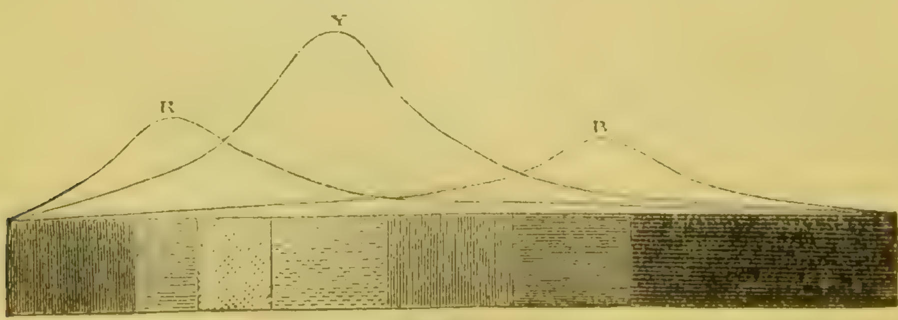


Fig. 5.

other tints might be produced, is by no means new; nor, taken in a certain acceptation, would that be very startling. Meyer, Young, and others have put forth notions of this kind;—most of which have been recently very acutely criticised by M. Helmholtz:—the assertion peculiar to Sir David Brewster, is that *the spectrum, as dispersed by the prism, consists of the overlapping of three dispersed simple rays, each having within itself all degrees of refrangibility.* The process employed to establish this conclusion, belongs to a portion of Physical Optics, to the phenomena of which, Sir David has made many very remarkable contributions,—the subject, *viz.*, of *Absorption.* If he would obtain a right comprehension of the experimental part of the inquiry, it is quite indispensable that the student resort to the original memoirs. In these, however, it is virtually asserted as follows:—*First;* By transmitting the ordinary spectrum of Dispersion, through coloured or absorbing media, one or two of the simple colours may be destroyed, and the residuum exposed to examination.—*Secondly;* Experiments conducted on this principle, leave no doubt as to the existence of a red, a yellow, and a blue colour, through the entire length of the spectrum, varying only in intensity, according to the *place* of the spectrum under examination.—*Thirdly;* Each of the so-called simple colours of the Newtonian spectrum can be changed into other colours *of the same refrangibility*, simply by removing—through agency of an absorbing medium—one or two of its component tints.—And *Lastly;* As white light consists of a mixture of red, yellow, and blue in certain proportions, a ray or band of white light, *indecomposable by dispersion*, may be extracted from any part of the spectrum by the simple absorption of the overplus of one or two of these its components. The view now given has been attacked by many able men and good experimenters. The Abbé Moigno objects theoretically that we do not at present know the nature of the agency named *Absorption;* and that we are not entitled to assert, that it consists in merely stopping one portion of an intrant ray and permitting the residuum to pass unaffected. He adds that although the doctrines of Erman and Von Wrede *promise*, they as yet only promise, to break in on the mystery still enshrouding the subject of the natural colours of bodies. But criticisms much more formidable, are those of an experimental kind, coming from practised hands. In a very remarkable memoir, Helmholtz has pointed out new precautions as *essentialia* to the production of a pure spectrum; and he asserts that the very puzzling appearances seen by Sir David Brewster, are not, under such circumstances, reproduceable. The student is referred to the memoirs by Sir David Brewster himself in the *Edinburgh Philosophical Transactions* and the *Philosophical Magazine;* to those by Airy, Draper, Melloni, and Helmholtz in the *Philosophical Magazine;* and to the memoir by Bernard in the *Annales de Physique et de Chimie.*—It is demanded by justice to state that, while Sir David Brewster firmly adheres to his first views, and has reproduced them in his recent interesting *Life of Sir Issac Newton,* Helmholtz thus concludes his elaborate essay:—"I have now mentioned all the facts adduced by Brewster. Although I have been unable to repeat all his experiments, I believe the discussion of those which I have succeeded in repeating, abundantly proves that in his method many hitherto unobserved influences come into play which render a sure judgment of the colours impossible and deprive his arguments of all force. If the assumed connection of refrangibility or length of wave, with colour, is to be proved erroneous, it must be done by some more certain method of observation, —a principal condition of which is, that the colour investigated be separated from the other colours, and rendered free from every trace of irregularly dispersed light."

Speculum. In optics, a metallic mirror, specially one of those which are used in the construction of reflecting telescopes. Referring to other articles for the detailed theory of these instruments, it suffices here to state that they consist essentially of a large concave speculum which forms in its focus the image of a luminous object; and of some optic apparatus for magnifying that image. As, however, the image and object are on the same side of the speculum, some contrivance is also required to prevent the observer from *standing in his own light.* In the first, and in many respects the best form, that contrived and executed by the illustrious Newton, the cone of rays, before meeting in the focus, is reflected at right angles by a plain speculum, little larger than its section there, to the side of the tube, where it enters an ordinary eye-piece. The observer, therefore, is at the top of the tube, and, it may be at a considerable height from the ground; or the great speculum is perforated at its vertex, and the ray-cone is received on a speculum parallel to the other, so that it is reflected back along its path through the opening, and forms a second image behind the great speculum, where the eye-piece is placed. No light is lost by the aperture, as it is always screened by the small speculum. If *this* be concave, the telescope is the one proposed by Gregory, before Newton's, but not executed for nearly half a century after; if convex, it is Cassegrain's. Both have the inconvenience of magnifying too much, in consequence of the action of the second speculum. The fourth kind of telescope has its great speculum inclined, so that its focus is at the very edge of the tube's mouth, and the observer looks through the eye-piece right at the mirror. It is called the Herschelian, from the great astronomer, who, with instruments of this construction, explored so deeply the abysses of the starry universe. In all the four, it is obvious that their power depends on the quantity of light which they

can concentrate into the eye, that is on the surface exposed by the great speculum; and also on the precision with which that light is condensed into a single point, qualities depending on the magnitude, the curvature, and polish of the surface. If every part have the same focus, the surface is parabolic; if the edge be of shorter focus than the centre, it is an ellipsoid of some kind, if longer, an hyperboloid. Any attempt to produce this precise figure, by any process of copying, is utterly hopeless; for even at the edge of a six feet speculum, a deviation from the parabola, sufficient to make it useless, could scarcely be detected by the most refined means of measurement which man has invented. Yet the difficulty has been completely vanquished, though only in our own time; and, independent of the inestimable results which have been given to mankind by the labours of the two Herschels, Lassell, and Rosse, it will not be without interest to consider a problem of mechanics of so high an order.—It involves the methods of casting, figuring, and supporting specula, and we purpose to give a brief exposition of each of these, referring those desirous of fuller information to the list given at the end of this article.

(1.) Casting.—The material generally employed (and whose use goes back to the remotest depths of Egyptian and Etruscan antiquity) is an alloy of copper and tin in the proportion of 32 to 15 nearly. This "speculum metal" is very brilliant, but is perhaps the most intractable among metallic bodies, so brittle that it breaks even in large masses with a slight blow or sudden change of temperature, and so hard and friable that it cannot be wrought by tools of steel. Some persons add a little arsenic, as recommended by Newton, to make it still more reflective, others zinc: these volatile metals seem objectionable. Silver makes it too soft, and nickel, which whitens so much the yellow alloy of copper and zinc, makes this yellowish. Lord Rosse, after many experiments, prefers the simple speculum metal, but attaches much importance to having it a definite atomic compound, consisting of four equivalents of copper to one of tin, or 32 to 14·911. In these proportions it is much less liable to be tarnished than when either component is in excess; and we have seen a two feet speculum of it, which retains much of its original brightness, though it has been left uncovered in an open workshop for more than sixteen years.* In forming this alloy, grain tin should always be used; for large specula the purest commercial copper must be selected, but for smaller (4 or 5 inches diameter) it is certainly advantageous to use that obtained by the electrotype process. The copper is to be fused by itself; the tin, previously melted in a separate crucible, is to be poured into it, rapidly stirred, and as soon as possible the mixture is cast into ingots. These precautions are taken to prevent as much as possible the oxidation of the tin at the high temperature of melting copper. The alloy, when once formed, melts at a much lower heat, and should always be re-melted for casting. It is a curious fact that it is scarcely possible to obtain a specimen of it without a number of minute pores, which are, in favourable circumstances, only to be seen with the microscope, but if cast at too high a heat, especially if they be pieces of the original ingots, these are conspicuous to the naked eye. This seems analogous to the extrication of oxygen from copper and silver when they become solid after fusion, and might perhaps be obviated, as in the first of these metals, by "poling," that is, stirring the fluid metal with dry poles of wood. As the copper may not be quite pure, or some tin may be lost by oxidation, samples should be examined, and a little tin added to the mass if they fall short of the standard brilliancy.—The casting a large and perfect disk of this most refractory substance, is one of the highest triumphs of the founder's art. If rapidly cooled, or cast in a close mould, it will *certainly* fly in pieces, and if cast in sand as an open casting, it will most probably be spongy or have a crystalline structure that will be visible when it is polished. These defects are prevented by casting it on a "chill," a surface of iron moderately warmed, which (as in the case of cast iron) gives a fine grain and increased compactness to a small distance from the surface which has been in contact with it. Mr. Lassell's mode of doing this, as improved by Mr. Nasmyth, is as follows. A cast iron mould, a little deeper than the speculum, with its bottom convex of the same radius, is attached to the end of a strong lever, which is loaded, so that when the mould is empty, its bottom makes a considerable angle with the horizon, but becomes horizontal when charged with the proper quantity of speculum metal. The fused metal is poured into a lateral cell communicating with the mould at its lowest point. This neat arrangement allows the metal to rise smoothly and evenly along the bottom, and will free the cast from all entangled air or scoria, but is not so likely to clear it from the gases evolved in its solidification as that which Lord Rosse had contrived after a long series of experiments. It may also be questioned whether it would be of easy management with specula of the very largest sizes.*—Lord Rosse makes the bottom of his mould of pieces of hoop iron, wedged tight on edge in an iron frame and turned to the proper curvature: it holds the fused metal, but lets gaseous matter pass freely through its interstices. On this a wooden pattern is laid about twice as

* A speculum should always be kept covered when not in use; and if very large, its box should communicate with a vessel containing quicklime, to dry the air in contact with it. Smaller ones are so easily repolished that for them it is scarcely required.

* The two feet speculum weighs 3¼ cwt., the three feet 13 cwt., and the six feet 80 cwt.

eep as the intended speculum, and $\frac{1}{65}$ larger o allow for the shrinkage): sand is rammed ound this. When the metal is poured into the vity so formed, the lower surface is chilled, e sides in contact with the sand become hard ext, but the central parts remain longer fluid, e top or back of the speculum congealing last, hich allows all the contraction and irregularity texture to occur there where they are not in- rious. He finds it essential to insure the pro- r quality of metal that the fusions should be rformed in covered crucibles; these are of cast on, and must be cast with their mouths up, or ey will be too porous to retain it; the best fuel peat or charcoal. In this way all his three d six feet specula have been cast, without (we lieve) a single case of failure, and we have ason to think that even larger diameters could managed.—However successfully the casting ay have been accomplished, the annealing must still more carefully attended to. While yet d hot (in which state it is not brittle), it must removed to a furnace, which for some days evious has been kept heated, so that all its in- ior brickwork is at a full red heat. If cast in iron mould, it may be transferred in that, but not, the floor of the furnace should be curved match the speculum. All the space round should be packed with ignited fuel; and every erture of the furnace being carefully luted, it left to cool. For a six feet this requires a onth or six weeks; and it has been found at the walls and vault of the furnace should at least two feet thick. Small specula may be ced in a Dutch oven filled with burning peat, d surrounded with a fire of the same, which is t to burn out.

(2.) Figure.—The giving to the disc of metal, is obtained, a brilliant polish, combined with a e parabolic figure, is one of the most wonder- achievements of art; and it must be kept in nd, notwithstanding all the improvements of process which have recently been made, that s one of the greatest delicacy, that it requires most scrupulous attention *in every part*, t a very slight omission will make it fail, d that even the most experienced operator is *secure* from disappointment.—This process twofold, grinding and polishing. The first these is performed when the speculum is igh from the annealing furnace, by a tool which small pieces of gritstone are cemented, ssed to the convexity of the surface by the ige. Two gauges should be provided, a con- c and a concave; they are pieces of sheet n on which circular arcs are struck, of a radius ual to twice the focal length, and afterwards free by filing and grinding. When the sur- e is made even by this tool, it is changed for other of cast iron, truly turned to the convex ige, and cut up into small squares by two sets grooves about $\frac{1}{4}$ inch wide, and the same depth, by the planing machine, or otherwise. This is charged first with quartrose sand, then with emery and water, and made to traverse the face of the speculum, by one of the machines here- after described, as in the process of polishing, but with shorter strokes. From time to time the mud must be washed away, and emery of increasing fineness applied, till all the scratches of the earlier part of the process are removed, and the tool is in perfect and uniform contact with the speculum. If the surface be broken up, the emery is not fine enough; but, if in the process of levigation it has been suspended in water for four minutes, it will give a face sufficiently smooth to show a star well enough for determin- ing its focal length, and even the quality of its figure. If the focal length prove too short, the central parts of the grinder must be cautiously reduced by filing or scraping, and the process repeated with the fine emery; and *vice versa*. Even in this stage the figure is under control, and it is best that it should be kept elliptic. If a duplicate speculum be wanted, it is well, as suggested by Mr. Lassell, to cover the face of the grinder with sheet lead, for the coarser part of the work; the emery beds itself in this cut- ting very keenly, and the iron surface is only used at the last, thus being not liable to much alteration. After the emery, tools of blue hone or slate have been used to remove scratches; but if the emery be of the qualities just mentioned, and the metal good, they are quite unnecessary. During the first part of this process, the edge of the speculum should also be ground true with sand, and a divided hoop of iron, which can be tightened as it wears by a screw; it is also desirable, though not absolutely necessary, that the back should be brought to a uniform surface.

(3.) Polishing.—In this, the grinder, or a similar tool (sometimes made of lighter materi- als), is coated with pitch to a thickness varying from the $\frac{1}{20}$ to the $\frac{1}{8}$ of an inch, keeping open the grooves which divide it into squares. The specu- lum, secured level on the polishing machine, and most carefully cleared from emery or dust, is smeared with a mixture of water and that form of peroxide of iron known in commerce as jeweller's rouge, and the polisher, warmed to 80°, is laid on it for a minute or two. If, on raising it, it appears that all the squares of the pitch have not come into full contact with the speculum, the process must be repeated till this is effected. Pitch was originally proposed for this purpose by Newton, and nothing has been found which answers so well. It must have a definite hard- ness, which can be proved by Mr. Lassell's test. A sovereign, when standing on it vertically for one minute, should leave on it the impression of four nicks of its milling. If softer than this, it should be kept some time boiling; if harder, softened by a little oil of turpentine. The pol- isher is now ready for work. In the old manual process, the polisher is fixed on a firm block, and the operator, holding the speculum by a

handle cemented to its back, works it backwards and forwards across the other, by straight strokes passing the edge a little, and applying no pressure beyond that caused by its own weight. After a few of these "cross strokes," he shifts his position to give them a new direction, and, at the same time, turns the speculum a little, to prevent, as far as possible, any inequality of the abrading action. Occasionally he varies them by a few circular ones, carrying the centre of the speculum round that of the polisher in a small spiral. As the moisture evaporates, a few drops of water must be supplied at the edge (or, in the modern process, through holes in the polisher, which is always the uppermost), just sufficient to prevent it from getting fast. The adhesion and friction rapidly increase, and the red of the peroxide changes to bronze colour by the abrasion of the metal. At last all traces of the emery disappear, a fine polish covers the surface to the edge, and then a few of the circular strokes should make it perfectly or nearly parabolic. If this figure be passed, it is scarcely possible to recover it, except by regrinding and repeating the whole process.—The most unequivocal way of examining the figure, is to have a series of diaphragms and disks which can be placed in the mouth of the tube so as to divide the surface of the speculum into successive rings; and, adjusting each of these to distinct vision of a close double star, to measure its focal length by means of a vernier attached to the focal adjustment. It is thus found that when all the rings agree, the image possesses a character by which this quality can be recognized at once. Throwing it a little out of focus, it swells into a ring with a dark centre (the shadow of the small speculum), and this is similar at equal distances on either side of the focus. If, on the other hand, the ring, with dark centre, is seen *outside* the focus, but *inside* of it the centre shows a plane with a bright central point, the speculum will prove elliptic; and if the reverse be the case, it is hyperbolic; and a practised eye finds this criterion quite sufficient. —It is evident that all this manipulation is tentative and uncertain; and, equally so, that all of it can be performed with precision by machinery. The supposed advantage of the hand, that of *feeling* if anything goes wrong, can be dispensed with by paying such attention to the conditions of the operation *that nothing shall go wrong.* It may be added, that the hand fails entirely for specula above nine inches, the only sort, which, in the present state of practical optics, are of any great value. But it required a combination of no ordinary qualities to realize these anticipations. The first who appear to have succeeded in mechanical polishing were Sir W. Herschel and his not less illustrious son; but their process has never been described; and it would be a valuable boon if the latter would give some record of the experience which was crowned by such results. Lord Rosse must therefore be considered the person to whom is due the credit of having given the impulse to this art, not only by directing attention to it, but by himself bringing it to a high state of perfection, in a series of researches combining first-rate practical skill with extensive theoretic knowledge. That others, following in this track, have been as successful, is the best proof as well as the best reward of what he has accomplished.—He and they, however, have taken different parts of the process just described, as the bases of their systems. Lord Rosse works by a double system of cross strokes; while, at the same time, the speculum and polisher revolve slowly, but with unequal velocities. This machine is shown in fig. 1. The speculum, H I (under which are seen the lever supports, that shall be explained after-

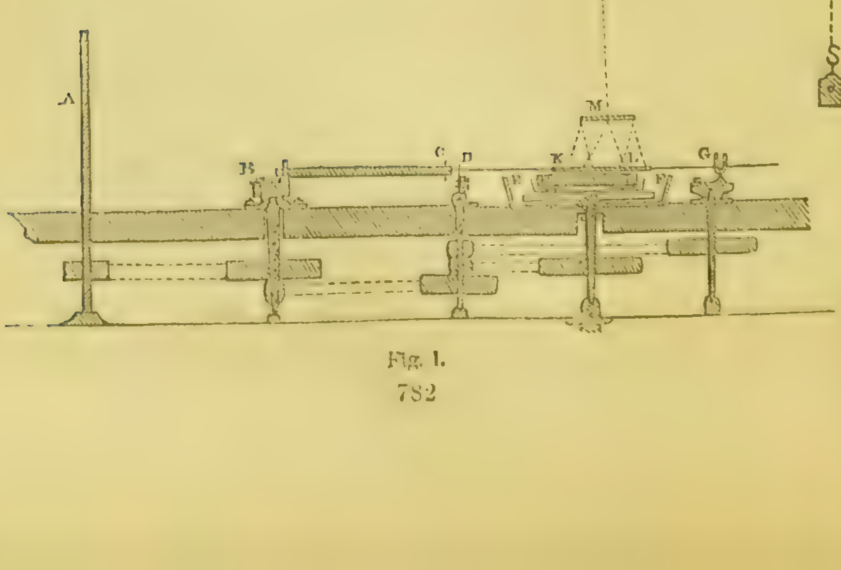


Fig. 1.

wards), is carried by a firm disc or chuck which turns with its strong vertical shaft in the cistern E F. This is filled nearly to the face of the speculum with water kept at 55°, to insure the proper consistence of the pitch, as much heat is evolved by the friction of a large polisher. K L is the polisher, which lies on the speculum, and is held in an annular expansion of the polishing bar D G, in which it fits loosely. The polisher, of the same diameter as the speculum, is made of cast iron, with six ribs at the back for lightness;* but as it is still far too heavy, it is suspended (by six points, to lessen the chance of flexure), from one end of the lever N, the other being loaded so as to reduce the pressure on the speculum to a pound for every circular foot of surface; this lever is free to traverse with the polisher. The polishing bar receives a rectilinear motion from the variable crank B, combined with the joint C, and the guide D; it also receives a transverse one from the crank G, (called the eccentric). These cranks and the speculum are driven by bands and drums from the shaft A, in a way which is obvious on inspection. He considers a one horse power sufficient to polish and grind a three feet speculum. When this mechanism is acting, the effect of the primary crank B, corresponds to the cross stroke of the manual process; giving the same movement, and, of course, the same polishing action to each point of the polisher. This would produce a form nearly spherical, but the rotation of the speculum adds to this a further action, increasing from its centre to its edge, and lengthening the focus of the exterior zones of its surface. This is the effect which is desired, but these two agencies are not sufficient to produce enough of it; and he increases it by the eccentric, which augments in any required degree the circumferential action, besides giving a truer surface. The figure then depends on four things, the radius of the primary, that of the eccentric, and the angular velocities of it and the speculum. Lord Rosse has found it sufficient to make the second the variable one, giving the others values established by a very wide course of experiment. He makes the primary stroke one-third of the speculum's diameter, the eccentric revolves in fifteen primary strokes, and the speculum in twenty-six. The control of the process is made by the throw of the eccentric, which must be regulated by circumstances, especially by the proportion of aperture to focal length. In his large specula, where this ratio is 1 to 9, it will in general be sufficient that it shall make the centre of the polisher move one-fifth of the diameter. It should also be noted that, in polishing his six feet speculum, the number of strokes per minute is eight, and for smaller ones, inversely as their diameter. The polisher should also revolve to keep its figure uniform; but this requires no special mechanism, because at each change of stroke it is released from lateral friction and is left free to be carried round by its adhesion to the speculum: it thus makes one turn for eleven or twelve of the other. The hygrometric state of the air must be attended to, as when it is too dry the rouge is not uniformly distributed over the speculum; and in that case it should be damped by permitting a jet of steam to escape into the laboratory. The polisher, like the grinder, is cut up into squares by transverse grooves; but Lord Rosse found its action improved by still further reducing the surface by a set of circular grooves. In the six feet there are thirty-one of these, and the squares are of 2½ inches on the side. We have found the same advantage by making the rectangular grooves twice as wide at their extremities as at their centre. With this machine a coating of pitch alone is not sufficient to produce the union of a high polish and perfect figure. It gives the first, and to a certain extent the other: the general form is correct, but there are irregularities which spoil the definition. The pitch must, therefore, be covered with a layer of harder composition (rosin fused with an eighth of dry flour to toughen it, and oil of turpentine added till at 55° it barely yields to the pressure of the nail). It seems as if the reversing of the motion at every stroke enables the abrading material to bite too sharply on any parts of inferior hardness, when bedded in the soft pitch, while the harder coat will not accommodate itself to such irregularities. Without the softer stratum below, however, it will not give a true figure. Lord Rosse rolls the pitch to a sheet of the proper thickness, by a roller with guides at its ends, and under tepid water. The hard composition reduced to powder is sifted over this, and fused by a hot iron held over it. The sheet is cut, under water as before, into pieces of the requisite size, and these are separately attached to the projecting parts of the polisher slightly heated. In fine, to insure the highest polish with this hard surface, it is found useful to moisten the speculum towards the end of the process, not with pure water, but with a solution of soap in strong water of ammonia. The polishing a six feet requires five hours. These details may seem too minute, but on such depends the difference between success and failure. If attended to with care, they will be found to give very satisfactory results. The process has been proved through a very wide range of size, from specula of six feet to those of two inches; and with a very great ratio of aperture to focus, for example, that of his equatorial, eighteen inches aperture and ten feet focus, and one of ten inches and five feet, both very good. The machine is also simpler and less expensive than those which we proceed to describe.—A few years later, Mr. Lassell, also an amateur optician, who had constructed for himself one of the finest nine inch

* He has since made the polisher much lighter by ranging a number of deep ribs like the walls of a honeycomb, and with greater increased stiffness.

Newtonians ever made, was induced by Lord Rosse's success to undertake one of two feet. The path which he followed is in most respects his own; he takes the second part of the old process, and relies entirely on circular strokes. His machine will be easily understood by a reference

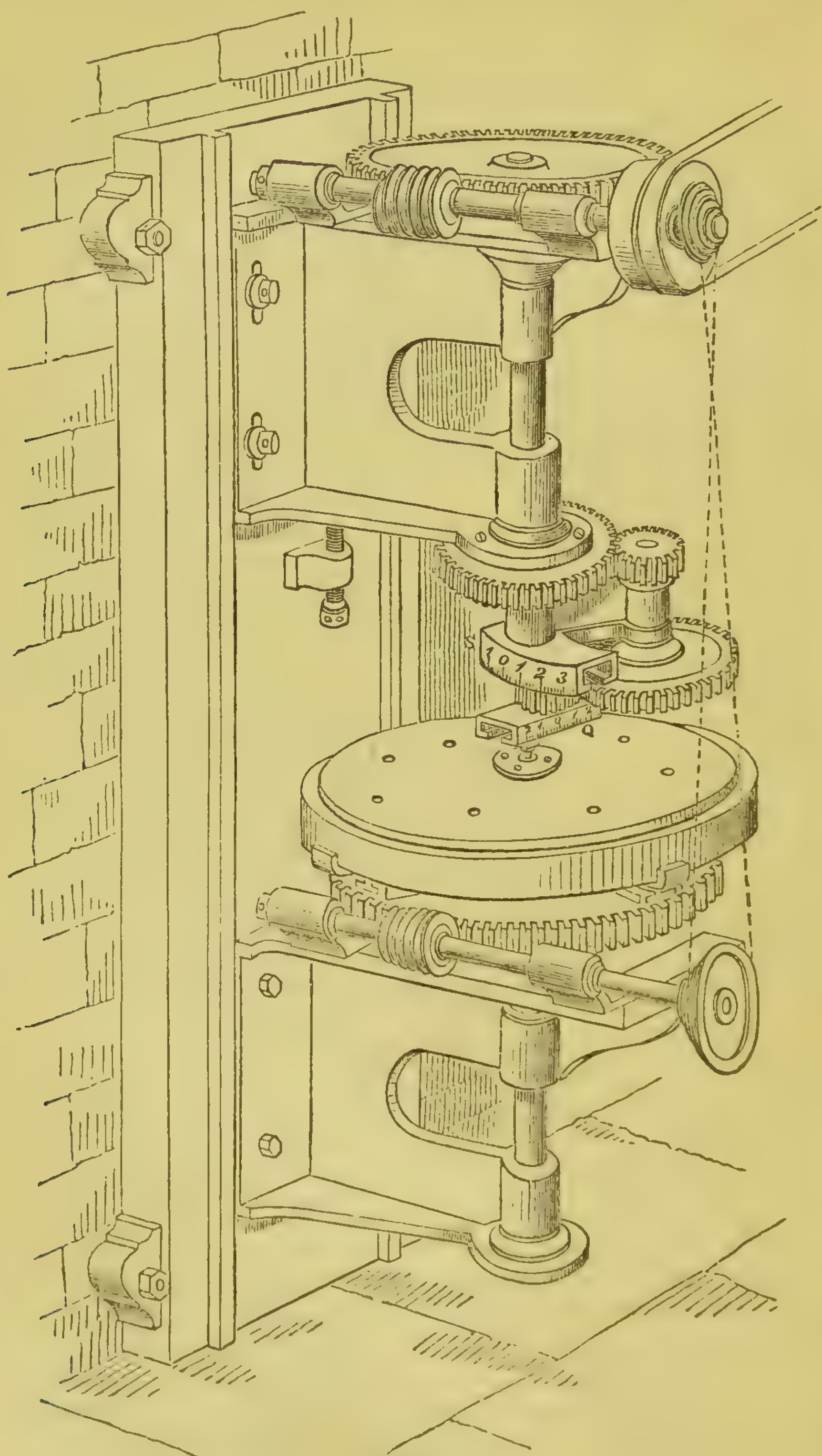


Fig. 2.

to fig. 2. It has two vertical shafts in the same line, each bearing a worm-wheel of seventy-seven driven by its screw. The worm-shafts are connected by a band, and the driving power is ap-

plied to the upper one. The lower vertical shaft carries, as in Lord Rosse's, a cistern (not shown in this figure), in which the speculum rests on its lever supports and revolves slowly. The upper shaft, passing *through a fixed wheel* of seventy-two, carries at its lower extremity the crank S, and in doing so, gives rotation to the wheel which is seen below it, also of seventy-two, by means of the pinion of sixteen, which gears in the fixed wheel. There is in S a circular dove-tail, concentric with the arbor of the pinion and wheel, in which there can be fixed, at any moderate distance from the axis of the shaft, a sliding piece, on which turns the second crank Q, by means of a second pinion, also of sixteen, seen between it and S. In this, there is also another dove-tail, but a straight one, in which slides another piece carrying the pin which drives the polisher. This elegant mechanism, analogous to the geometrical pen of Suardi, makes the pin, and of course the polisher, describe hypocycloidal curves, the radius of whose base is that of the crank S, and of their describing circle that of Q: the precise form of them depending on the proportions of these radii and the relation of their angular velocities. The actual lengths are much less than in Lord Rosse's machine; S being for a two feet speculum $1^{i.}7$ and Q $1^{i.}4$, these two quantities being the variables, and Q making about thirty-four revolutions in a minute. In this arrangement it is difficult to counterpoise the polisher, it is therefore made of deal for lightness; and to prevent it from warping, it is in two pieces, glued and screwed with their grains at right angles. It is about two inches thick, and only 0·85 of the speculum's diameter; not itself grooved, but when covered with pitch, *this* is divided into squares of an inch by a suitable tool. It appears that ordinary pitch alone is used, which is a great advantage; on the other hand the *wooden* polisher seems objectionable, both from the difficulty of making it exactly fit the speculum in the first instance, and the likelihood of its shape changing after a time, both which make doubtful that uniformity of thickness of the pitch, which is believed to be very important.* This, however, cannot weigh against the fact that the process has produced admirable two feet specula; as is shown by Mr. Lassell's discovery of an eighth satellite of Saturn, one of Neptune, and many other splendid proofs of their sharp definition. When applied to larger specula, it may possibly require some modification, at least we believe that Lord Rosse has not found it to give as good results as his own process. One reason for this seems probable; the application of the moving force (which on such a scale is very considerable) at the centre of the polisher, where it is much more likely to distort the figure of this tool, than when applied round its circumference by the ring of the polishing bar.—Mr. Lassell subsequently suspected a tendency to polish in rings; to remove which he introduced a small rectilinear motion; which is done still more effectually by Mr. Warren Delarue, who has made very fine specula of thirteen inches. His machine is of the same character as Mr. Lassell's, with a provision for insuring a given rotation to the polisher. This appears necessary to uniform action, but seems not to be as certainly obtained as in Lord Rosse's.—It will be observed that the motion of the crank S is concentric with that of the speculum, and, therefore, useless except as a means of driving Q. If the centre of the crank Q were fixed at a distance from that of the speculum = radius of S, and the angular velocity of the speculum made = the relative velocity of it and S in Mr. Lassell's, and in the opposite direction, it is manifest that precisely the same curve must be described by the polisher. This was at once perceived and acted on by Mr. Grubb of Dublin, a distinguished mechanician and optician; and his machine appears to deserve the preference, not merely on this account, but for the wider range of its powers,* which comprise those of the other two. Fig. 3 (taken from a photograph) will give a sufficient explanation of it. The bed-plate or base A supports

1. The worm-shaft *f*, which drives

2. The worm-wheel *b*: this is bolted to the spur-wheel *c*, both of ninety. They turn on the stud *g*, and are formed with a circular flange near their periphery, the lower surface of which bears on a trued part of A, and the upper gives a firm support to the speculum and its cistern (which are not shown in the figure). The wheel *c* drives another of fifty-two, not seen, but attached to the lower surface of

3. The cam *d*; in the groove of this is engaged a pin, fixed in the lower surface of

4. The cam-lever *e*, which is thus made to vibrate as the cam revolves; the lever has a series of holes, to any of which can be attached one end of a link whose other end is connected with

5. The upper or sliding-plate B, which thus by the rotation of the cam is made to vibrate to and fro, according to any required law: the actual law is one of uniform motion.—The plate B carries a strong spindle *h*, on which vibrates the arm *i*. This arm can be, at pleasure, either held in position by the connecting link *k*, or made to vibrate by the revolution of the variable crank *l*,

* The greater inertia of the metal polisher must make its action differ from that of the wooden one, even though it be counterpoised to exert only an equal pressure.

* Mr. Grubb has since contrived another machine of remarkable simplicity, which seems to possess the powers of all those described in the text, with some peculiar to it. It is not described here, because it has not yet been tried on specula. Its chief performance as yet has been the lenses of an object-glass of twelve inches aperture, which it has wrought with singular facility, varying the curves at pleasure, by changing the adjustments, so as to make slight change of the corrections.

which is driven by a shaft at the back of the slide-plate B. The arm carries a variable crank m, which can be driven either quickly, by the shaft and pulley n, or slowly by the worm-wheel o. The pin of m, as in Mr. Lassell's, drives the polisher.—If it be required to give Lord Rosse's action; the crank m being fixed and its pin set central, the crank l is set in motion: this gives the primary stroke, and the reciprocation of the slide-plate B gives that of the eccentric. If the worm-wheel o be made to act at the same time, a light lever attached to m will

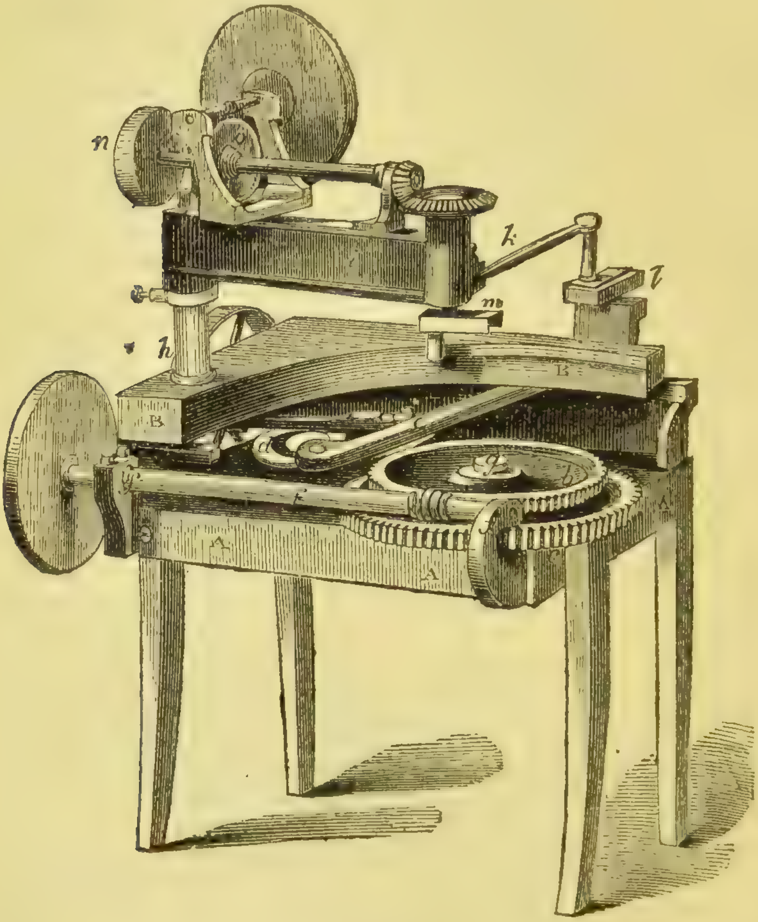


Fig. 3.

carry round, with a slow motion, the polisher by a stud projecting at its circumference.—If it be wished to have Mr. Lassell's motion, the crank l is stopped and it is fixed in such a position that the pin of m shall be at the required distance (= radius of S) from the centre of the speculum, the arm i being held firm by the link; the pin of m is next set to the distance which = radius of Q, and it is driven by n. In this case, however, there is no provision to secure the rotation of the polisher.* The eccentricity can also be given by means of B; and as it can be made to vibrate slowly on each side of this to any required extent, the effect of Mr. Lassell's last improvement

* It is worthy of notice that Lord Rosse's machine becomes capable of the same action (or very nearly so) by a very trifling alteration. Were the primary crank at G fig. 1, and the eccentric at B (in which case the link B C and guide D would be transferred to the right of G, the former being below the bar). Then removing the guide and link and engaging the crank-pin in a hole in the polishing bars; the centre of the polisher will describe an approximate ellipse, whose major axis = the throw of the crank, and its minor = that $\times \frac{a-b}{a}$, a being the distance between the centres of the two cranks, and b that from the centre of G to that of the speculum. In the figure this ratio is five to seven, and

is given in perfection.—Mr. Grubb decidedly prefers the Rossean action for grinding, but thinks the other less likely to fail in the polishing with unpractised hands, and uses it himself. He begins with a large eccentricity and gradually diminishes it, which can be done without stopping the machine. His polisher is also of wood, but made with peculiar care. It is formed by six layers of mahogany, each $\frac{5}{16}$ of an inch thick, and not continuous, but built up of pieces three inches square. These are only glued where they cross, being, at least in the interior, not in close contact at their edges, and the direction of their grain varied as much as possible. The disc when turned true, is plugged at the edges, varnished, and coated with tin foil, at the edge and back. It is the same diameter as the speculum. He uses pitch alone; rolls it like Lord Rosse, cuts it into squares of $\frac{3}{4}$ inch, and attaches them to the surface, warming it by a spirit lamp.—This machine measures about three feet every way, and can work a two feet speculum; but the largest on which it has been tried is the Newtonian of the Glasgow Observatory, twenty inches aperture and fifteen feet focus.—Its arrangements seem particularly adapted to the figuring a speculum for the Herschelian telescope, respecting which a few remarks must be premised. In the other three forms of the reflecting telescope, the light suffers two reflections instead of one; and therefore the image with a given great speculum will be brighter than theirs inversely as the per centage of reflection. This per centage was found to be 0·647, by careful experiments with a plane speculum of Lord Rosse's metal, which has been $2\frac{1}{2}$ years in use, and therefore may fairly be assumed to represent the ordinary working condition of a telescope. Hence, the Herschelian will be as effective as the others in point of illumination, when its aperture is to theirs as four to five: and a still greater advantage than mere economy and convenience follows from this reduction of bulk in the diminished effect of atmospheric disturbance. Besides the influence of this on all telescopes, reflectors are specially affected by the current caused in their tubes, by the difference between the temperatures of the air and the speculum which almost always exist. This disturbance will be as the *mass of air* traversed by the light and as the *cube* of the diameter.*

It is therefore of the utmost importance to keep this diameter as low as possible. Lord Rosse has, by a very ingenious process, made the plane speculum of pure silver, whose reflective power is no less than 0·93; but the metal tarnishes so rapidly, that such specula can only be used on special occasions, for they can scarcely be repolished. Mr. Lassell has substituted for this speculum a glass prism of forty-five degrees, as originally proposed by Newton; but the gain is not considerable: the reflection is indeed total, but much light is lost by the first surface and by absorption in its substance. One of very clear glass made for this purpose was found to give 0·746, the length of its sides is 1·125, it is of English plate and certainly much clearer than Munich flint: the absorption will of course increase with the size of the prism. It is therefore an important matter to perfect the Herschelian. But here as the reflection is oblique, there is no definite focal *point*, even with a speculum whose direct action is perfect; there are only two ill-conditioned focal lines at right angles and not in the same plane. To act properly it should be a circular segment cut out of a paraboloid with the vertex at its edge; and a rude mode of conceiving and perhaps executing it is this: let three specula be arranged on the machine in contact round its centre, and let them be polished at once by a polisher of double their diameter, as if they were in one large one, then supposing *that* perfect, each of them would have its focus in the general axis. Now, if the properties of such a disc be investigated, it will be found that the change of curvature across any diameter is as the cosine of the angle which it makes with what may be called the speculum's "line of dip." It therefore is probable that as the curvature is changed by varying the eccentricities; if *this* were altered according to a law whose period is that of the rotation of the speculum, at least an approximation to the true figure would be obtained. Mr. Grubb proposes to do this very simply, by making the cam-wheel of the same number as the wheel *c*, and if the present cam were found not to be of the proper form, by substituting one of a shape which would be indicated by the result. A few trials would show what that should be. This seems quite feasible: and it is to be wished that some person would work the problem out, for assuredly it is by such means that the remotest limits of telescopic vision will be reached. Lord Rosse appears to have thought on the subject and tried some experiments, but he has probably been diverted from it by the wondrous results of his great telescope. It should, however, be remembered that unless considerable defining power can be obtained along with a focal ratio not much larger than that of his specula, this construction loses much

it might come nearer to equality by making the cistern smaller, and G as close to it as possible. The action of the eccentric is not interfered with, the counterpoising remains unchanged, and the moving force acts as before, at the circumference instead of the centre. The eccentricity must be given either by shortening the shank of the bar, or in some forms of this machine more conveniently by shifting the eccentric laterally.

* A large reflector will sometimes define marvellously for a few minutes after the dew (which is apt to cover the speculum when opened too early in the evening) has dried off: but as the night grows colder, while the large mass of metal retains its heat for a long time, it loses all defining power, though it will still perform well with a reduced aperture, and this *without any change of figure*. This evil would be greatly lessened, if not removed, by Sir J. Herschel's device (*Cape Observations*), of an open frame-work of iron instead of a tube.

of its value.—The small specula are best figured in the same way as the great, even the plane of the Newtonian, except that its polisher must be larger than the metal: it must be examined during the process, viewing some distant object by reflection in it, and then direct, with a good achromatic. Thus any curvature can be detected, which must be corrected by properly altering the adjustments of the machine. With respect to those of the Gregorian or Cassegrain, it is still more important that they should be polished by the machine, as their figure must be carefully adjusted to that of the great one, which they may even compensate if it be slightly defective.

(4.) SUPPORTS.—Whatever perfection the large speculum may possess, is all in vain unless it be properly supported; and more difficulty is felt in doing this rightly than perhaps any other part of its history. To many it will seem incredible that a disc of this hard and rigid material, four or six inches thick, can bend except under some great force purposely applied; while the fact is that it requires the most refined mechanical contrivance to prevent injurious flexure from its own weight. Even a nine-inch one, if resting on a ring at its circumference, on three screws, or according to the old plan, pressed by springs against three stops bearing on its edges, loses its defining power. The system of support must be twofold; for the back and for the edge.—Where it can be applied, the simplest and best is Sir John Herschel's: he lays the speculum on a bed composed of several folds of woollen cloth,* and prevents it from lateral motion by packings of the same material placed between its edge and the side of its box. Of course the play which its elasticity permits will alter the adjustment of the telescope at different altitudes; but he corrects these by means of a small collimator fixed in the inside of the telescope's tube, with its optic axes parallel to the line of adjustment. If its cross of wires is seen to depart from that of the telescope, they can be made to coincide in a few seconds by the adjusting screws which act on the speculum-box. The objection to it is, that it occasionally must require re-arrangement; and that it would not bear to be immersed in water during the polishing: it is therefore unsuited to very large specula, which, when once figured, should *never be raised from their supports.* Perhaps a three feet might not suffer from this, but a six feet most assuredly would be spoiled if lifted, however carefully. For such, therefore, the system of equilibrated bearings is used, which was invented many years ago by Mr. Grubb, and more fully developed by Lord Rosse. As applied to the three feet it may be thus described: suppose the speculum divided into eighteen equal portions by concentric circles and radii, whose centres of gravity rest on circular discs covered with felt, each pair of which are at the ends of a lever: each of these levers turns at its centre on a strong pin, at the angle of a triangle, whose centre of gravity bears by a hemispherical cup on the rounded end of a screw tapped in the bottom of the strong box which holds the speculum, and serving to adjust it. Thus each of the eighteen points of bearing is equally pressed, and if the speculum have a proper thickness, the weight of the portion between any two is not sufficient to bend it. It is however obvious that the number of points should increase with the diameter. Mr. Grubb found that six were sufficient for the nine inch; twelve were used in a fifteen inch Cassegrain, which he constructed for the Armagh Observatory in 1835, but it showed traces of flexure which disappeared with eighteen. Lord Rosse, therefore, carried the principle still further for the six feet; the three primary triangles carry, by spherical bearings at their angles, nine secondary, which similarly carry twenty-seven pieces of cast iron, shaped like the outlines of the imaginary segments of the speculum, and made extremely stiff by cross bracing. In every case this apparatus should be massive to avoid tremors.—Nothing can be better than these arrangements as long as the speculum is nearly horizontal; but new forces come into play when the telescope is lowered. (1.) The speculum tends to slide down the inclined plane on which it rests: this develops friction, probably about the sixth of its weight, which tends to distort it: this tendency must be prevented by some lateral support. (2.) Its pressure on this tends to change its curvature unequally in its different diameters. (3.) The pressure on the back supports is lessened; they, therefore, spring up and press the speculum forward in the direction of its axis. It however is held back by the friction of its edge against the lateral supports below; this partly produces a severe transverse strain there, partly gives a fulcrum round which the speculum is tilted. In consequence of this the upper back supports are released more than the lower, and therefore press less.—Mr. Grubb supported his specula (almost all which were for equatorials) in a metal hoop sufficiently flexible to distribute the pressure round the lower semi-circle, and supported by three or four equidistant screws, passing through holes in the speculum-box which have play enough for the springing of the back supports. The elasticity of the hoop is sufficient to leave the lower screws slack while the upper bear the load, whatever be the position of the telescope. If this be equatorially mounted (as should be the case wherever practicable), it seems possible to make this arrangement perfect: the speculum must be counterpoised by weights at the upper end of the tube. Now, if these weights were applied at the ends of levers attached with spherical bearings to the cradle which bears the tube, so that they would always act vertically (as was done by Fraunhofer in the Dorpat achromatic, *Trans.*

* Vulcanized caoutchouc would be preferable, but that the sulphur in it would tarnish the speculum.

R. Astr. Soc., vol. i.), and the other ends were connected with the hoop, all lateral pressure could be entirely removed. Even then, however, there might be some of (1.) remaining caused by the weight of the back levers; in Lord Rosse's six feet, they, with the strong frame which bears them, weigh at least four tons, and such a drag behind the speculum must do mischief. He has however latterly obviated the entire of (1) by interposing three small balls of bronze between one of his six feet specula and each of the bearing pieces of the levers, so that the speculum rests now on eighty-one points of bearing. This part therefore is completely effective, and the use of the balls permitted him to give the lateral support by a suspendsd ring. Originally it was not so satisfactory, being two curved pieces, each bearing at two points and supported at their centres by strong uprights. Probably balls or rollers applied here also would remedy the most injurious part of (3). Mr. Lassell's lateral arrangement appears excellent. For back supports he uses the system already described, with eighteen bearings; but, besides these, he has another set of levers, whose fulcra are in the back-plate of the speculum box, and their short arms act in holes made in pieces of metal cemented on the back of the speculum; to their longer arms are fixed weights, such that the pressure of the speculum on its hoop is almost (not entirely, that it may be steady) taken off. Thus all edge-strain is removed; but his arrangement of the levers requires that the same diameter of the speculum shall always be vertical. There seems no difficulty in removing this restriction, and at the same time simplifying the arrangement. Let the fulcrum of each of these levers be a sphere turned on it, and working in a spherical cup hollowed in one of the bearing discs of the back levers; let its point be also spherical, and received in a spherical cup cemented to the speculum, the distance between the centres of these, that is the short arm of the lever, being as small as possible, and a system of complete equilibrium would be attained. It must, however, not be forgotten, that thus a stress (equal nearly to the whole weight of the speculum when the telescope is low) is applied in the plane of its back, which tends to produce the strain (1). The lateral levers should therefore in strictness be applied round the circumference, and in a plane passing through the centre of gravity. This might, perhaps, be done by means of segments of Lord Rosse's brass (possessing the expansion of speculum metal) closely fitted and cemented all round it by marine glue in solution, or some other strong cement. This part alone seems yet imperfect; but, as has been indicated, the difficulties which remain present nothing insuperable. It is to be hoped, that others besides those who have laboured on the subject with such splendid results, will turn their attention to it; for there is a boundless harvest of discoveries to be yet reaped, even in this hemisphere of the sky. It can only be thoroughly explored by large reflectors, for though the achromatic possesses great advantages in some respects, yet it falls short in others. In illuminating power especially, we have no sufficient data to compare them exactly; but it has been ascertained by Amici, that for *small* apertures, the achromatic and Newtonian show objects equally bright when these apertures are as 3 and 4. For larger, the co-efficient of the achromatic must decrease on account of the increased thickness of the glass, and the consequent greater absorption. From the measure already given for the reflecting prism, we may infer, that if it were *one inch and an eighth mean thickness*, it would only transmit three-fourths of the incident light. It is therefore nearly certain, that there is not in the world an achronametic which, *if placed beside* Mr. Lassell's telescope, on a good night, would equal it on faint objects. As to matching a six feet, especially if made Herschelian, that is out of the question. Besides, could discs of glass be procured of sufficient size? They would be liable to bend and have double refraction from pressure, as they could only be supported at the circumference; and the cost of such object-glasses would be enormous. Nor could they be executed with any chance of success by amateurs. It is therefore to be hoped that the Reflecting Telescope, British by invention, by all its subsequent improvements, and by its most glorious applications, will continue to attract the attention of our countrymen, so that they may keep their present lead both in making and using it.—A list is subjoined of the most important memoirs in which the subject of this article is treated.

1. Newton: *Phil. Trans.*, 1672. The description of his telescope: it is wonderful how thoroughly he seems to have mastered the details of the whole process.—2. Molyneux: *Smith's Optics*, vol. ii., p. 301. Minute details of Hadley's process, as improved by him and the celebrated Bradley. In the same volume, p. 298, is described Huyghen's machine for polishing lenses.—3. Mudge: *Phil. Trans.*, 1777, p. 296. A valuable paper, points out the necessity of remelting the metal.—4. Edwards' *Nautical Almanac*, 1787. This is very scarce, but its substance is given in Rees' *Cyclopædia*, article Speculum. He polishes by cross strokes, but uses an elliptic polisher. Also of great value.—5. Sir W. Herschel: *Phil. Trans.*, 1795. Description of his forty feet telescope. Something more of his modes of casting specula is given in Rees' *Cyclopædia*, article Telescope, in a letter from Smeaton: he occasionally mentions "machine-polished specula"—6. Little: *Trans. R. Irish Academy*, vol. x., 1805. Worth reading. He cut a series of circular grooves in the pitch of his polisher.—7. Cecil: *Trans. Cambridge Phil. Soc.*, 1822. A machine for polishing spherical specula. He proposes to make them parabolic, by cutting out four curved sectors from the polisher's pitch.—8. Lord Rosse: *Edinburgh*

Journal, vol. ix., 1828. Account of a speculum of three spherical zones, whose centres are separated, so that their foci coincide. In the same volume he describes his present machine, but is still uncertain whether it may give the parabolic figure. In vol. ii. of the new series of the same Journal, 1829, he has satisfied himself of this, and describes his speculum of plates, of the metal soldered on a back of a peculiar brass.—9. Potter: vol. iv. of the same *Journal*, 1831. Casts small specula on a cold surface of polished steel, prepares rouge by precipitating sulphate of iron with ammonia and cautious ignition.—10. Sir John Herschel: *Introduction to Cape Observations.* Some incidental details of great value.—11. Lord Rosse: *Phil. Trans.*, 1840. Details of the present process which he uses.—12. Robinson: *Proceedings of R. Irish Academy*, 1840. Account of Lord Rosse's three feet telescope. The same, for 1842 and 1845. A full account of the casting and polishing the six feet.—13. Greene: *Proceedings of British Association in* 1843. Notice of his polishing machine. There is an earlier paper by him, 1834, to which we cannot refer; but an extract of it is given in Holtzapfel's *Mechanical Manipulation*, p. 1292.—14. Lassell: *Trans. R. As. Soc.*, 1849. Full details of his process This, and still more, Lord Rosse's memoir, are the standard authorities.—*Proceedings of British Association*, 1850. Description of the lateral support by levers.—15. Lord Rosse: *Phil. Trans.*, 1850. Notice of the six feet.—16. Lassell: *Proceedings of R. As. Soc.*, Cambridge. For giving rectilinear motion to speculum.—17. Delarue in the same volume; another mode of effecting the same end, and also for giving definite rotation to the polisher.

Speculum, the Silver. It has recently been proposed to substitute silver for the ordinary speculum metal. It is far more brilliant, reflecting 0·91 of the incident light, while the other only gives 0·67; but besides its high price, there are almost insuperable difficulties in figuring and polishing it by the usual methods. Its liability to tarnish is also a weighty objection. In 1853, Lord Rosse met these in the case of the plane mirror of his great Newtonians, by depositing silver from a solution of ammoniated nitrate on a polished glass, detaching it, and employing the specular surface which had been in contact with the glass. He found the gain of light very great, the speculum (if protected when not in use) does not tarnish very rapidly, and the cost and trouble of renewing it are trifling. This would be impracticable for the large speculum; but in 1857, M. Leon Foucault* published a process which has given very satisfactory results, and seems applicable to any dimensions. His speculum is made of glass (which for this purpose need not be optically perfect): it is figured and polished to a true parabola, and the silver is deposited in a thin but uniform film on its surface.† Its exterior surface is of course parabolic, but has no lustre: it however receives this in a high degree, and without any sensible change of figure, by light friction with soft wash leather and a little peroxide of iron. In case of tarnish the same process renews the polish, and it may be often repeated (how often has not yet been ascertained) before the silvering requires renewal.—The advantage of such specula appears to be considerable. A Newtonian furnished with them would, with 53 inches aperture, equal in light Lord Rosse's six feet, and one of 64·5 the same, were it Herschelianized. The great speculum will also be much lighter, and less liable to bend.—M. Foucault has already constructed a telescope of 13 inches aperture, and 8¾ feet focus, which has clearly separated into two the blue companion of γ Andromedæ, a test which is quite decisive as to the accuracy of the surfaces. He uses Drayton's process, in which the reducing material is oil of cassia: Lord Rosse employs saccharic acid, and Liebig sugar of milk, which has the advantage of not requiring the application of heat. Liebig's process, as given by Delarue, is as follows:—10 grammes of fused nitrate of silver are dissolved in 200 cubic centimètres of water, and just enough liquid ammonia is added to this solution to redissolve the brown precipitate first formed. This solution is mixed with 450 cubic centimètres of a solution of caustic soda, specific gravity 1·035. A purplish-black precipitate forms on the admixture of these solutions, which is to be dissolved again by the careful addition of ammonia. After which 800 cubic centimètres of water are added and so much of a dilute solution of nitrate of silver as to occasion a permanent slight precipitate of oxide of silver; 50 cubic centimètres of water finally are added, and the solution filtered. This is then the normal alkaline solution of silver which is retained for use when required. Just previous to the silvering operation eight parts of this alkaline silver solution are mixed with one part of a solution of sugar of milk, containing one part of sugar of milk in ten parts by weight of water. In order to prepare the surface to be silvered so as to insure the adhesion of the silver film, it is rubbed over with a solution of cyanide of potassium by means of cotton wool; after the surface is thoroughly cleansed in this matter, it is washed with distilled water, and, just previous to its immersion in the silvering liquid, it is wetted with strong spirit of wine. All things being ready, the silvering solution is poured into a vessel sufficiently deep to contain the speculum

* Dr. Steinheil, of Munich, has claimed priority in this discovery. We have not the means of verifying his references; but it is certainly to M. Foucault that astronomers are indebted for the practical result.

† This film is thin enough to transmit blue light, while it totally reflects the rays of solar heat. M. Foucault has availed himself of this to construct a most effective shade for observations of the sun.

in a vertical direction; but instead of placing the mirror exactly vertical, it is allowed to lean slightly over to one side, the surface to be silvered being placed downwards. In a few seconds the solution darkens, becomes brown, and deposits the silver on the glass in the first instance as a black or purple transparent mirror; this gradually brightens and becomes more and more metallic, and finally, in about three-quarters of an hour, a film is obtained at ordinary temperatures which is extremely thin and regular in thickness; this mirror has a slight bronzy hue by reflected light, and if of the proper thickness, transmits a deep blue light when the sun is viewed through it. It readily, after drying, receives a polish when rubbed with a piece of chamois leather and dry rouge (peroxide of iron): it is much harder than ordinary silver, and bears the same relation to it as electro-precipitate, copper, iron, &c., bear to those metals prepared by the ordinary metallurgic methods. In another memoir on the configuration of optical surfaces, Foucault concludes as follows:—"The mechanical processes by which the working of glass surfaces is effected seem to lose of their efficiency when the pieces are of great dimensions; the results they then furnish are but an approximation, which is certainly far from being satisfactory. But where mechanism becomes powerless, the hand of man can do something more: assisted by the resources which optics put at his disposal, and guided by a system of observations, the power of which increases with that of the instrument about to be constructed, the human hand is then enabled to proceed with the work, and to carry it out to the greatest degree of precision."

Spheroid. A species of ellipsoid (*q. v.*) It is formed by the revolution of an ellipse round either of its diameters. If about its longer diameter it is called a prolate spheroid. If about its shorter it is called an oblate spheroid. Its surface is given by this equation

$$\frac{x^2}{a^2}+\frac{y^2}{a^2}+\frac{z^2}{c^2}=1$$

the same as that of the ellipsoid.

Spherical Trigonometry. See TRIGONOMETRY.

Spring. See BALANCE and ELASTICITY.

Squares the Least, Method of. On this very remarkable and important method of combining *Equations of Condition* in the best possible manner, we shall be able to offer only a few practical hints and illustrations.—In the first place, however, let the nature of the problem to be solved be accurately understood. In ERROR and CORRECTION we have shown the chief precautions necessary, so that the observer who requires to measure any quantity—let that quantity be an angle or a linear magnitude—may come as near to the truth as possible. But no precautions can enable him to reach the absolute truth, on which account it is the practice to take a *number of measures*, and to trust that somehow an estimate more accurate than any single measure may be deduced from the *mean* of the whole. And the question is—*how shall this mean be obtained?* By the usual mode of adding all observations together and dividing the sum by the number of these observations? Or is there any other preferable rule? It may be concluded from our few remarks under ERROR, that the usual *arithmetical mean* is by no means necessarily the best or truest mean; and the inquiry thus opened has given rise to those elaborate series of investigations which have issued in the *Law* or *Theorem* of the MINIMUM SQUARES. The fullest and most satisfactory discussions of the subject are unquestionably those by Gauss and Laplace, with the latter of whom we must join Poisson. It is scarcely necessary to mention the very inconclusive efforts of Mr. Ivory in *Tilloch's Magazine*. The student is especially referred to a valuable review of the whole subject by Mr. Ellis, in *Cambridge Transactions*, vol. viii.: we may be permitted just to add that the inquiry has yet scarcely escaped that taint of metaphysics which still affects, more or less, many points connected with the doctrine of PROBABILITIES. The general theorem arrived at and sanctioned by all physicists is this:—*If the mean of a number of distinct observations be calculated, so that the squares of the errors shall be a minimum, the values obtained for the quantities, will, under the given circumstances, be the nearest or best obtainable values.* Suppose, as is usually the case in Astronomy, the number of unknown quantities, x, y, z, v, &c., to be less than the actual number of equations of condition from which we desire to deduce the values of these quantities. In other words, suppose that we have

$$\begin{aligned}
\mathrm{X} &= \alpha - b\,x - c\,y - d\,z - e\,v - \text{\&c.}\\
\mathrm{X}_1 &= \alpha_1 - b_1\,x - c_1\,y - d_1\,z - e_1\,v - \text{\&c.}\\
\mathrm{X}_2 &= \alpha_2 - b_2\,x - c_2\,y - d_2\,z - e_2\,v - \text{\&c.}\\
\mathrm{X}_3 &= \alpha_3 - b_3\,x - c_3\,y - d_3\,z - e_3\,v - \text{\&c.}\\
\mathrm{X}_4 &= \alpha_4 - b_4\,x - c_4\,y - d_4\,z - e_4\,v - \text{\&c.}\\
&\text{\&c.} \qquad \text{\&c.} \qquad \text{\&c.}
\end{aligned}$$

Let us put for the possible errors the symbols Δ, Δ_1, Δ_2, Δ_3, Δ_4, &c., and for brevity's sake as below,—

$$\begin{aligned}
\mathrm{A} - \alpha &= a\\
\mathrm{A}_1 - \alpha_1 &= a_1\\
\mathrm{A}_2 - \alpha_2 &= a_2\\
\text{\&c.} \quad &\text{\&c.}
\end{aligned}$$

where A, A_1, A_2, &c., represent the values obtained from observation of the functions X, X_1, X_2, &c.; then

$$\begin{aligned}
\Delta &= a + b\,x + c\,y + d\,z + \text{\&c.}\\
\Delta_1 &= a_1 + b_1\,x + c_1\,y + d_1\,z + \text{\&c.}\\
\Delta_2 &= a_2 + b_2\,x + c_2\,y + d_2\,z + \text{\&c.}\\
&\text{\&c.} \qquad \text{\&c.} \qquad \text{\&c.}
\end{aligned}$$

Next let us calculate

$$
\begin{aligned}
(ab) &= ab + a_1 b_1 + a_2 b_2 + a_3 b_3 + a_4 b_4 + \\
(ac) &= ac + a_1 c_1 + a_2 c_2 + a_3 c_3 + a_4 c_4 + \\
(ad) &= ad + a_1 d_1 + a_2 d_2 + a_3 d_3 + a_4 d_4 + \\
(ae) &= ae + a_1 e_1 + a_2 e_2 + a_3 e_3 + a_4 e_4 + \\
&\&c. \qquad \&c. \qquad \&c.
\end{aligned}
$$

$$
\begin{aligned}
(bb) &= bb + b_1 b_1 + b_2 b_2 + b_3 b_3 + b_4 b_4 + \\
(bc) &= bc + b_1 c_1 + b_2 c_2 + b_3 c_3 + b_4 c_4 + \\
(bd) &= bd + b_1 d_1 + b_2 d_2 + b_3 d_3 + b_4 d_4 + \\
(be) &= be + b_1 e_1 + b_2 e_2 + b_3 e_3 + b_4 e_4 + \\
&\&c. \qquad \&c. \qquad \&c.
\end{aligned}
$$

$$
\begin{aligned}
(cc) &= cc + c_1 c_1 + c_2 c_2 + c_3 c_3 + c_4 c_4 + \\
(cd) &= cd + c_1 d_1 + c_2 d_2 + c_3 d_3 + c_4 d_4 + \\
(ce) &= ce + c_1 e_1 + c_2 e_2 + c_3 e_3 + c_4 e_4 + \\
&\&c. \qquad \&c. \qquad \&c.
\end{aligned}
$$

$$
\begin{aligned}
(dd) &= dd + d_1 d_1 + d_2 d_2 + d_3 d_3 + d_4 d_4 + \\
(de) &= de + d_1 e_1 + d_2 e_2 + d_3 e_3 + d_4 e_4 + \\
&\&c. \qquad \&c. \qquad \&c.
\end{aligned}
$$

$$
\begin{aligned}
(ee) &= ee + e_1 e_1 + e_2 e_2 + e_3 e_3 + e_4 e_4 + \\
&\&c. \qquad \&c. \qquad \&c.
\end{aligned}
$$

Then

$$
\left.
\begin{aligned}
(ab) + (bb)\mathrm{X} + (bc)\mathrm{Y} + (bd)\mathrm{Z} + (be)\mathrm{V} + \ldots &= 0 \\
(ac) + (bc)\mathrm{X} + (cc)\mathrm{Y} + (cd)\mathrm{Z} + (ce)\mathrm{V} + \ldots &= 0 \\
(ad) + (bd)\mathrm{X} + (cd)\mathrm{Y} + (dd)\mathrm{Z} + (de)\mathrm{V} + \ldots &= 0 \\
(ae) + (bc)\mathrm{X} + (ce)\mathrm{Y} + (de)\mathrm{Z} + (ce)\mathrm{V} + \ldots &= 0
\end{aligned}
\right\} 5
$$

are the equations which serve for ascertaining x, y, z, v.

Example. Suppose we have the equations

$$
\begin{aligned}
\mathrm{X} &= x + y + 2z \\
\mathrm{X}_1 &= 3x + 2y + 5z \\
\mathrm{X}_2 &= 4x + y + 4z \\
\mathrm{X}_3 &= -x + 3y + 3z
\end{aligned}
$$

and found by observations

$$\mathrm{A} = 3,\ \mathrm{A}_1 = 5,\ \mathrm{A}_2 = 21,\ \mathrm{A}_3 = 14;$$

then

$$
\begin{aligned}
\Delta &= 3 - x + y - 2z \\
\Delta_1 &= 5 - 3x - 2y + 5z \\
\Delta_2 &= 21 - 4x - y - 4z \\
\Delta_3 &= 14 + x - 3y - 3z
\end{aligned}
$$

$$
\begin{aligned}
a &= 3, & b &= -1, & c &= 1, & d &= -2 \\
a_1 &= 5, & b_1 &= -3, & c_1 &= 2, & d_1 &= 5 \\
a_2 &= 21, & b_2 &= -4, & c_2 &= 1, & d_2 &= -4 \\
a_3 &= 14, & b_3 &= -1, & c_3 &= 3, & d_3 &= -3,
\end{aligned}
$$

consequently as above.

$$
\begin{aligned}
(ab) &= -88, & (bb) &= 27, & (cc) &= 15, & (dd) &= 54, \\
(ac) &= -70, & (bc) &= 6, & (cd) &= 1, \\
(ad) &= -107, & (bd) &= 0,
\end{aligned}
$$

whence by equation 5

$$
\begin{aligned}
-88 + 27x + 6\mathrm{Y} + \quad &= 0 \\
-70 + 6x + 15\mathrm{Y} + \mathrm{Z} &= 0 \\
-107 + \mathrm{Y} + 54\mathrm{Z} &= 0
\end{aligned}
$$

From which three equations the most probable values follow,

$$
\begin{aligned}
\mathrm{X} &= 2\,.\,470 \\
\mathrm{Y} &= 3\,.\,551 \\
\mathrm{Z} &= 1\,.\,916
\end{aligned}
$$

A very elegant process by Gauss, to determine the most probable values of x, y, z, v, and their *weights* $\mathrm{W}, \mathrm{W}_1, \mathrm{W}_2, \mathrm{W}_3$—(the weight of all the observations being put $= 1$)—consists in the following. Let us put

$$
\begin{aligned}
(ab) + (bb)\mathrm{X} + (bc)\mathrm{Y} + (bd)\mathrm{Z} + (be)\mathrm{V} + \ldots &= \mathrm{P} \\
(ac) + (bc)\mathrm{X} + (cc)\mathrm{Y} + (cd)\mathrm{Z} + (ce)\mathrm{V} + \ldots &= \mathrm{Q} \\
(ad) + (bd)\mathrm{X} + (cd)\mathrm{Y} + (dd)\mathrm{Z} + (de)\mathrm{V} + \ldots &= \mathrm{R} \\
(ae) + (be)\mathrm{X} + (ce)\mathrm{Y} + (de)\mathrm{Z} + (ee)\mathrm{V} + \ldots &= \mathrm{S} \\
\&c. \qquad \&c. \qquad \&c.
\end{aligned}
$$

and then eliminate from these equations, x, y, z, v by known algebraic methods, we shall always be able to exhibit the values of these unknown quantities by expressions of the following form;—

$$
\left.
\begin{aligned}
\mathrm{X} &= \mathrm{L} + \mathrm{A}_0^0 \mathrm{P} + \mathrm{B}_0^0 \mathrm{Q} + \mathrm{C}_0^0 \mathrm{R} + \mathrm{D}_0^0 \mathrm{S} + \ldots \\
\mathrm{Y} &= \mathrm{L}_1 + \mathrm{A}_1^1 \mathrm{P} + \mathrm{B}_1^1 \mathrm{Q} + \mathrm{C}_1^1 \mathrm{R} + \mathrm{D}_1^1 \mathrm{S} + \ldots \\
\mathrm{Z} &= \mathrm{L}_2 + \mathrm{A}_2^2 \mathrm{P} + \mathrm{B}_2^2 \mathrm{Q} + \mathrm{C}_2^2 \mathrm{R} + \mathrm{D}_2^2 \mathrm{S} + \ldots \\
\mathrm{V} &= \mathrm{L}_3 + \mathrm{A}_3^3 \mathrm{P} + \mathrm{B}_3^3 \mathrm{Q} + \mathrm{C}_3^3 \mathrm{R} + \mathrm{D}_3^3 \mathrm{S} + \ldots
\end{aligned}
\right\} 7
$$

&c. &c. &c.

and if we have actually eliminated these the most probable values of x, y, z, v, are

$$
\left.
\begin{aligned}
\mathrm{X} &= \mathrm{L} \\
\mathrm{Y} &= \mathrm{L}_1 \\
\mathrm{Z} &= \mathrm{L}_2 \\
\mathrm{V} &= \mathrm{L}_3
\end{aligned}
\right\} 8
$$

&c.

and the weights of these results,

$$
\mathrm{W} = \sqrt{\frac{1}{\mathrm{A}_0^0}}, \quad
\mathrm{W}_1 = \sqrt{\frac{1}{\mathrm{B}_1^1}}, \quad
\mathrm{W}_2 = \sqrt{\frac{1}{\mathrm{C}_2^2}}, \quad
\mathrm{W}_3 = \sqrt{\frac{1}{\mathrm{D}_3^3}}
$$

&c.

If we call ϕ, ϕ_1, ϕ_2, the mean errors which may have been committed in determining the probable values of x, y, z, v, (according to 8)

$$\phi = \frac{0{\cdot}282095}{\sqrt{\mathrm{W}}} \quad (\log 0{\cdot}282095 = 9{\cdot}4503954)$$

$$\phi_1 = \frac{0{\cdot}282095}{\sqrt{\mathrm{W}_1}}$$

$$\phi_2 = \frac{0{\cdot}282095}{\sqrt{\mathrm{W}_2}} \quad (10)$$

$$\phi = \frac{0{\cdot}282095}{\sqrt{\mathrm{W}_3}}$$

Lastly, the probable errors $\mathrm{F}, \mathrm{F}_1, \mathrm{F}_2$, which may have been committed in finding the most probable values of x, y, z, v; or those errors, of which it is equally probable that they have been com-

mitted, or that they have not been committed, are the following,—

$$F = \frac{0.476936}{\sqrt{w}} \quad (\log 0.476936 = 9.6784601)$$

$$F_1 = \frac{0.476936}{\sqrt{w_1}}$$

$$F_2 = \frac{0.476936}{\sqrt{w_2}} \quad (11)$$

$$F_3 = \frac{0.476936}{\sqrt{w_3}}$$

The limits ΔF, ΔF_1, ΔF_2, between which the true values of F, F_1, F_2 will occur, are

$$\Delta F = F\left(1 \pm \frac{0.476936}{\sqrt{n}}\right)$$

$$\Delta F_1 = F_1\left(1 \pm \frac{0.476936}{\sqrt{n}}\right)$$

$$\Delta F_2 = F_2\left(1 \pm \frac{0.476936}{\sqrt{n}}\right) \quad (12)$$

$$\Delta F_3 = F_3\left(1 \pm \frac{0.476936}{\sqrt{n}}\right)$$

&c. &c.

in which expressions n is the number of the observations or the number of the given equations in (2).

To show an application we shall employ again the example formerly chosen. We have according to (6)

$$\begin{aligned} -88 + 27x + 6y \quad &= P \\ -70 + 6x + 15y + z &= Q \\ -107 \quad + y + 54z &= R \end{aligned}$$

Whence, according to (7),

$$x = \frac{49154}{19899} + \frac{809}{19899}P - \frac{324}{19899}Q + \frac{6}{19899}R$$

$$y = \frac{2617}{737} - \frac{12}{737}P + \frac{54}{737}Q - \frac{1}{737}R$$

$$z = \frac{12707}{6633} + \frac{2}{6633}P - \frac{9}{6633}Q + \frac{123}{6633}R$$

And, according to (8)

$$x = \frac{49154}{19899} = 2.4702$$

$$y = \frac{2617}{737} = 3.5509$$

$$z = \frac{12707}{6633} = 1.9157$$

Likewise, according to (9)

$$w = \sqrt{\frac{19899}{809}} = 4.9595$$

$$w_1 = \sqrt{\frac{734}{46}} = 3.6913$$

$$w_2 = \sqrt{\frac{6633}{123}} = 7.3435$$

Whence it follows that z is determined most precisely of all, and y least precisely, so that y is only half so certain as z. Further, we obtain from (10)

$$\phi = 0.1267,\ \phi_1 = 0.1468,\ \phi$$

as the means and according to (11)

$$F = 0.2142,\ F_1 = 0.2481,\ F_2 = 0.1760$$

as the probable errors.

Lastly, in section 7 the number of the given equations was four, consequently $n = 4$ and $\sqrt{n} = 2$; therefore, according to 12,

$$\Delta F = \begin{pmatrix}1.238468\\0.761532\end{pmatrix}F = 0.2653 \text{ and } 0.1630$$

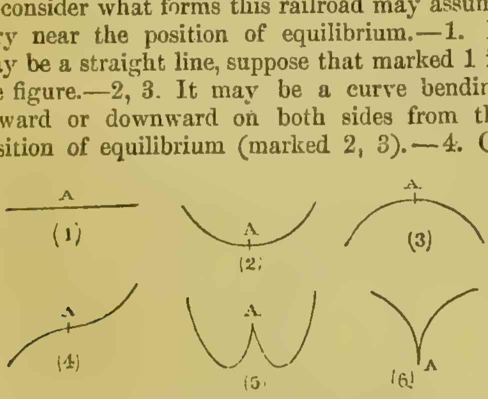
$$\Delta F_1 = \begin{pmatrix}1.238468\\0.761532\end{pmatrix}F_1 = 0.3071 \text{ and } 0.1890$$

$$\Delta F_2 = \begin{pmatrix}1.238468\\0.761532\end{pmatrix}F_2 = 0.1180 \text{ and } 0.1341$$

Gauss had previously communicated, besides this method, a method of solving equations of the first degree by the method of least squares, if their number exceeded the number of unknown quantities involved in them. He transformed these equations into so many others, of which each succeeding one contains one unknown quantity fewer than the foregoing one. This process indeed is applicable to every number of unknown quantities, but the operation always becomes more difficult, so that, to avoid error, we must constantly have before us the notation of the formulæ, which, in every particular case, are to be transformed into numbers.

Stability. When a body is in equilibrium, and a small force, in any direction, is added to those already acting upon it, motion in that direction is immediately produced. But after this force shall have spent its effort, there will be an endeavour to return to rest. If the new position of rest, be the same as that in which the body was when the force was applied, the body is said to have been at first in *stable equilibrium*—if not, in *unstable* equilibrium.—One does not see at first how stable equilibrium can be possible. If a force act, it must produce some result. It cannot spend itself in vain, or neutralize its own efforts utterly. But, in stable equilibrium there is a return to the exact position occupied before motion, and therefore it would seem that there may be an utter loss of force and work in the universe. The mistake arises from this, that account is not taken of the heat which is generated. The work done is, in fact, converted into heat. In the case of a pendulum, for example, there is a perfect return to the original position, but heat of friction is generated somewhere. This heat is the result of the forces which have operated, and

will be their equivalent. An equivalent for all living force must be found, and always is found in the economy of Nature. Regarding *unstable* equilibrium, take the following illustration: If we balance a rod carefully on the top of the finger, the tendency of any side motion to make the rod fall is sufficiently manifest. In fact, there are only two positions of perfect equilibrium in such a case—the first, when the rod hangs freely in the vertical line downwards; the second, when it stands upright. In the former case, a pull or push to the side would just cause oscillation, with a final return to the original position—in the latter, a push or a pull would make the body swing wholly away and settle in the vertical line downward.—To investigate fully the conditions of stability is impossible in this place, but we shall consider a few cases, pointing out how they are to be treated. We shall confine our attention to the case of *points*—taking for granted (see CENTROBARIC) that where a body moves, we may substitute for it a point of exactly the same weight. If such a body be disturbed and its centre of gravity moved, it must do so in a certain line, straight or curved. Suppose then that we slip a line of rigid matter, of the form of this line along which the body must move in the circumstances of disturbance, we can now abstract all consideration of the forces themselves. The forces drive the body along the line from a position of equilibrium (the point of commencement of motion) in it, and we may suppose the motion along it, to be produced by a different cause, the presence of a rigid railroad, instead of the real one. Let us consider what forms this railroad may assume very near the position of equilibrium.—1. It may be a straight line, suppose that marked 1 in the figure.—2, 3. It may be a curve bending upward or downward on both sides from the position of equilibrium (marked 2, 3).—4. Or



one bending upwards on the one side, and downwards on the other (marked 4).—5, 6. Or the position of equilibrium may be the point where two curves meet, or rather two branches of one curve, of the bending of both of which branches, this point cannot be considered the centre (marked 5, 6). Suppose motion from A along the straight line,—there is here no accumulating force of gravity to call the body back to its old position, and displacement would only be removal to another place of rest. The body starting from A and moving along the line would just be in the same circumstances as if it started from any other point along the line. It can rest indifferently under the action of gravity at any point in the line, and its equilibrium is called *indifferent*. In motion from A (in 2,) there would be an effort exerted by gravity to retard and destroy this motion, and as it constantly acts, it must ultimately succeed in so doing—continuing to act after destroying it, it will bring down the body to the old position, but there it will have given it such a velocity that it will go up as high on the other side. Were motion in a perfect vacuum possible, this oscillation would go on for ever; but, as it is not, the body gives part of its force away *in heat* to surrounding bodies; the oscillations become less and less, and finally the body settles again at A. The equilibrium here is *stable* equilibrium. In motion from A in 3, again, there is the same action of gravity, but it goes to increase and not diminish the motion; and, therefore, A being once left, there is no tendency to return. This equilibrium is called *unstable*. In motion from A, in 4, there are two possibilities to be considered—that the body move to the right or the left. In moving to the left we have exactly the same results as in 3—gravity increasing the disturbance, and no tendency to restoration after disturbance. In movement to the right, gravity diminishes the disturbance, as in 2, and brings back the body to A, but still, as in 2, with such a velocity as to take the body beyond A. Motion in this direction once obtained, gravity increases it, and there is no return to A. Here also, therefore, the equilibrium is *unstable;* and in all cases of motion from A, the body will go down the slope to the left. In motion from A, in 5, a very peculiar result is obtained. It is evidently of no consequence here in which direction the motion is. In every case the body moving from A will go downwards to the bottom of one of the curves—be carried by its velocity up from that to a height rather higher than A (supposing it sent from A with considerable velocity), will go round, perform the same thing on the other side, and so on. Theoretically there would be an incessant motion round A, as theoretically there is in 2, and so in this view we might call the equilibrium stable. In almost no practical case, however, would the body come back and rest just on A. With a perfect equality of circumstances (*e. g.*, friction) on each side, it would do so; but in practice that could never be. In some one of its swingings upwards from the bottom of one of the branches to A, it would come short of A; its force on the other side in the previous rise having been just sufficient to carry it over, and some of that having been lost—say by friction—in the interval. We thus return to the case of 2, and have the bottom of that branch of the curve in which A was, as the ultimate position of stable equilibrium. Hence, therefore, the result that, with perfect equality of action on each side, a body at A would be in stable equilibrium,

and without that, as in almost every practical case, in unstable equilibrium. The equilibrium in case 6 is evidently stable.—We may thus establish the general law that a system acted upon by gravitating forces is in equilibrium only when the tangent to the path, which alone it can describe, is *horizontal*. This will be found to hold for all the six cases. Withdrawing 5 from consideration, we have this result—that stable equilibrium is only possible when the centre of gravity has reached a position lower than all those immediately around it, in its possible path. Case 5 modifies the *theoretical* statement of this law, for there the body is *not* in a position lower than all around it, but yet in stable equilibrium. But the statement, nevertheless, remains practically true.—Such is the technical meaning of the word *stability*.

Stars, Fixed. The name given to those myriads of shining points—with the exception of our few planets—which glorify the midnight skies. What these Orbs are, it is impossible positively to demonstrate: but as our sun would shrink into the dimensions of a fixed star, and put on all its other visible characteristics, were it removed to a corresponding distance, we are sustained by every consideration of probability, in assuming that the luminaries, scattered so profusely around us, are also suns. If a thousand balls were placed within an urn, *one* only being black, it is little likely that, on one drawing, this single black ball would appear: and it were an equal strain of logic to conclude, that the only fixed star with whose nature we can be conversant—(because of its neighbourhood with the earth)—is the sole organized globe amidst the throng constituting our galaxy. But we have no room for speculation, and must leave untouched discussions as to the *Plurality of Worlds*. There are enough of positive inquiries regarding the Fixed Stars,—one class referring to the visible and real distribution of these orbs,—the other to the physical causes and consequences of their *grouping*. The latter subject forms the matter of next article;—the former is sufficiently extensive to justify our dividing it into sections.

I. The Relative Positions of the Fixed Stars.—This important inquiry has two elements:—it is concerned, *first*, with the accurate determination of a star's true position on that apparently concave surface environing the earth:—*secondly*, with the determination of its absolute or relative depth in space.

(1.) *Position of the Stars on the Surface of the Celestial Concave.*—This position is said to be ascertained when the star is referred to its distances from two fixed great circles of the sphere. Observation alone cannot determine these distances. To do that, calculations of a laborious nature must be applied to every observation;—calculations which are technically named, *Corrections*, and *Reductions*. They are as follows.—1. The apparent places determined by the *Transit* and *Circular Instruments* (*q. v.*) must be corrected for the errors of these instruments, including among such errors, the personal equation of the observer.—2. The instrumental determination thus accomplished, corrections must be applied to free that determination from influences depending on the position of the observer. These influences are three; *first*, atmospheric *Refraction* (*q. v.*); *secondly*, *Aberration* (*q. v.*); and, *thirdly*, *Parallax*. This latter correction is required for a very few stars only, and, with existing instrumental means, it is almost evanescent. Its nature is fully explained below, and its amount also indicated.—3. The two previous corrections, would suffice, were the great circles to which the place of the star is referred, absolutely fixed. But they are not so. *First*, we have those motions of the pole of the Earth and the pole of the Ecliptic, whose character has been explained under Precession and Nutation;—motions, that change, according to regular laws, the position of the great circles of Reference: and, *secondly*, the position of the axis of the earth's rotation appears itself to undergo a change, although that is nearly evanescent, and at present by no means ascertained or valued.—An observation of a star then, is, itself, but the simplest part of the determination of its place. That observation must first be freed from instrumental errors, and next from effects of Refraction by a set of suitable tables. The other tangible causes of displacement, are Precession, Nutation, and Aberration; and in practice they are now easily estimated and corrected for, by tables whose convenient form we owe to Bessel. He has reduced the whole of these corrections, "*en bloc*," under a form which, as given by Mr. Baily in his Introduction to the *Catalogue of the British Association*, is—

$$\text{A}\,a + \text{B}\,b + \text{C}\,c + \text{D}\,d = \text{cor. for R. A}:$$
$$\text{A}\,a' + \text{B}\,b' + \text{C}\,c' + \text{D}\,d' = \text{cor. for Dec.}:$$

in which the quantities A, B, C, D, depend solely on the season of the year, and are therefore common to all the stars, and the quantities a, a'; b, b'; c, c'; D, D′ depend on the star's apparent place. It is easy to tabulate the former quantities for all the stars, and the latter for every star approximately known; nor do the formulæ, enabling us to ascertain the values of a, a'; b, b', &c., although somewhat tedious, present real difficulty in their application to any newly observed point in the heavens.—As presented in the foregoing few words, the process of Correction and Reduction must appear simple: but to perfect it, all the ever growing resources of practical and theoretical Astronomy have been put in requisition. It is only after this process has been completed, that we can venture to speak of the Proper Motions of the Fixed Stars.

(2.) *Position of the Fixed Stars, as to Absolute and Relative Depth in Space, or Distance from our System.*—This most important subject is divisible into the two parts indicated by its title.

a. The Parallax of the Fixed Stars.—The dis-

covery of *parallax*, or the determination of the *absolute* distances of many of these remote orbs, is probably the greatest triumph of modern practical astronomy. The parallax of a fixed star signifies the difference of the directions along which the telescope must be pointed, to descry that star, when we are at opposite extremities of the earth's orbit; in other words, it is the angle subtended by the breadth of that orbit, as seen from the star in question. On the first promulgation of the Copernican system, this eminently practical objection was raised by Tycho,—if the earth rolls round the sun, why do we discern the fixed stars, at opposite periods of the year, in exactly the same apparent places? Little did Tycho dream that the day would come, when tenths of seconds of space would be measurable, and when his difficulty would, by such almost incredible attainments, be removed! With his gigantic quadrant before him—measuring with pain a space of ten minutes—he could not be expected to anticipate the proximity of an epoch, in which a circle of three feet in diameter would be virtually divided into *thirteen millions of equal parts!* We cannot offer even a *résumé* of these long and ultimately successful efforts to detect the parallax of the fixed stars. Two methods have been resorted to, with equally satisfactory results. The parallax of a star may be ascertained by its differences of right ascension and declination, when its position is determined from opposite parts of the earth's orbit; but as determination of minute *spaces* has, until very recently, been much more accurate than our determination of minute periods of *time*, the transit instrument has not been applied to this most delicate inquiry. As, however, the determination of the absolute position of a star within these narrow limits, involves all the uncertainties belonging to refraction, the effect of parallax has been sought in annual changes of the relative places of two stars, so near each other in the celestial concave that their refraction might be assumed to be identical. In this latter way, the acute and most accurate Bessel, first detected the parallax of sixty-one *Cygni.* But we claim for a lamented countryman—the late Professor Henderson of Edinburgh—the distinguished honour of establishing this evanescent element, for the southern star α *Centauri*, by use of the mural circle. Henderson's observations were anterior to Bessel's, although it cannot fairly be said that his suspicions did not require to be confirmed by his excellent successor, Mr. Maclear. These researches have been successfully followed up by many inquirers. The student who would know the full history of the subject, as well as the critical nature of the instrumental research and subsequent discussion of results, must refer to that classical memoir by M. Peters, —"*Recherches sur la Parallax des étoiles fixes;*" a memoir in which every source of possible fallacy is recognized, and its value estimated, with an acuteness belonging especially to M. Peters, and that scrupulousness of conscience, which it were desirable, should belong to every inquirer in any walk. M. Peters proposes, as fixed, the following parallaxes:—

61. Cygni,	0″·349	....prob. error	0″·080
α Lyræ,	0 ·103	—	0 ·053
Polaris,	0 ·067	—	0 ·0 2
1830. Groomb.,	0 ·226	—	0 ·141
Capella,	0 ·046	—	0 ·200
Arcturus,	0 ·127	—	0 ·073

The amount of probable error in several cases, compared with the determination, shows, of course, the amount of uncertainty attending these determinations; that with regard to Capella, for instance, is worth nothing. Since the date of M. Peters' memoir, many other researches have enriched this most difficult department of astronomy,—none more curious or interesting than those in reference to that remarkable star, 1830, Groombridge. This star has the largest apparent proper motion yet known,—no less than 7″ a year; and the general expectation was, that it would yield a very large parallax, or be found to be very near our solar system. Faye in Paris, Struve and Otto Struve in Russia, and finally, Wichmann of Königsberg (the latter piously fulfilling the wishes of the illustrious Bessel), have all wrought at the problem. But there is no discussion of it equal to Wichmann's—occupying four entire numbers of the *Astronomische Nachrichten*,—an essay much more valuable in its criticism and explanation of method, than even as to its positive conclusions. Wichmann establishes the parallax of the star in question to be 0″·71—considerably less than that of α *Centauri.* The memoir of M. Peters does not conclude, however, with these separate or individual determinations, important though they are. He deduces generally that the parallax of stars of the second magnitude, is 0″·116, with a probable error of only 0″·014. And on the ground of this and other researches, M. Struve has proposed the following table of distances:—

Apparent Magnitude.	Parallax.	Years occupied by light in traversing these remote regions.
1·	0″·209	15·5
1·5	0 ·166	19·6
2·	0 ·116	28 0
2·5	0 ·098	33·3
3·	0 ·076	43·0
3·5	0 ·065	49 7
4·	0 ·054	60 7
4·5	0 ·047	69·0
5·	0 ·037	84·8
5·5	0 ·034	96·6
6·	0 ·027	120·1
6·5	0 ·024	137·9

The numbers 1, 2, 3, &c., in the foregoing table indicate the average places of stars of the *first*, *second*, *third* magnitudes; the numbers 1·5,

2·5, 3·5, &c., the boundary lines between spheres occupied by the different sizes of stars. The hypothetical portion of the subject will be discussed in next section; in the meantime, what reader can contemplate unmoved, depths of space —accessible to observation—across which light cannot travel in less than *one hundred and forty years!*

b. But it has been conceived that the *relative* depths of masses of stars may be estimated beyond the limits within which our *absolute* determination must ever be confined:—the table just given, indeed, rests largely upon this mode of estimation. The principle of the method does not at first sight appear open to formidable objection. It is this—*the various magnitudes of the stars are in the main owing to their different distances.* This proposition cannot be applied to individual orbs; neither, perhaps, to separate and limited groups; but that it is to a considerable extent applicable to the masses of the stellar orbs, seems to follow from the fact, that these increase in number as they diminish in magnitude, at a rate, in some correspondence with the enlarging capacities of the spheres of space they occupy. There are two grave uncertainties, however, attaching to the employment of such modes of estimation.—*First*, if we would infer distance from magnitude, it is necessary that we have a correct estimation of magnitude, or a correct photometry of the stars. Unfortunately, this important department of practical astronomy remains in a condition very far from satisfactory. At present there is no fixed or universal scale; and the same observer frequently varies in interpreting his own scale. No man has laboured more to impress the necessity of an uniformity than Sir John Herschel. The scale adopted by this astronomer is explained as follows by himself:—"The principle on which I have endeavoured to proceed in estimating the relative magnitudes of stars below the sixth, is that of continually halving the light of each magnitude to give that of the next inferior denomination; so that, in fact, *two* stars of the 9th magnitude, so close together as not to be distinguished from *one*, shall affect the eye as a single star of the 8th."—Struve's scale is different. According to him a star having *half* the light of a given magnitude, is counted of a magnitude only a half lower: what Sir John Herschel calls the 8th magnitude, in the foregoing passage, would with Struve be 8½; and so on. The following list shows the names given to equivalent *lights*, by the two astronomers, in the case of the smaller stars:—

Struve.	Herschel	Struve.	Herschel.
8½	11	12	18
9	12	12½	19
9½	13	13	20
10	14	13½	21
10½	15	14	22
11	16	14½	23
11½	17	15	24

For full discussion on the subject, the student is referred to the Introduction to Struve's *Mensuræ Micrometricæ.* Very valuable contributions, as to the mode of determining the relative illuminations, have recently been made by Mr. Dawes, and Professor Manuel Johnson of Oxford.—*Secondly;* an objection very much more grave, however, is also alleged to apply. Are we authorized to assert that the whole light emitted by a star reaches the earth? In other words, is not part of that light extinguished or absorbed as it traverses the interstellar spaces? If absorption of this kind takes place, our inference as to distance, on the ground of mere apparent magnitude, would evidently greatly err by excess; for the stars must be comparatively near us. The fact of such absorption was first asserted by Olbers, on grounds which we cannot at present dismiss as insufficient. If there were no absorption, these bodies should all shine with the same *intrinsic brightness*, whatever their distance:—*i. e.*, although distance would diminish the *quantity* of light they transmit, it would not affect its *quality.* But the smaller stars are also *duller:* their light fades through effect of an increasing deadness. How is this to be accounted for? Not by absorption in our atmosphere merely: hence, said Olbers, the likelihood of an interplanetary ether, and of *extinction* in some proportion to the distance of the star. The opinion has recently been revived by M. Struve, who has supported it by other considerations, which, however, we cannot deem cogent. And on the ground of these considerations he has sought a formula expressive of the brightness of a star (ξ) in terms of its distance x. If there were no absorption, that brightness would of course be proportional to the inverse square of the distance; or

$$\xi = \frac{1}{x^2}$$

Struve puts forward as the equation

$$\xi = \frac{1}{x^2} \cdot 0{\cdot}990651^{x-1}$$

Assume the truth of this equation, and we are forced to modify vastly our ideas of the profundities of these orbs. For instance, the space-penetrating powers of telescopes are reduced, as shown by the subjoined numbers.

	Herschel's Telescopes.	Range without Extinction.	Range with Extinction.
	7 feet	219·8	123·2
	10 —	311·1	152·2
First	20 —	423 2	183 2
Second	20 — side view	663·8	227·8
	20 — front view	813 9	250·7
	25 —	1010·2	279·6
	40 —	2089·3	368·5

The difference is startling indeed: and although Struve's conclusions have by no means been accepted absolutely, it is clear that while such uncertainties remain, we are not entitled to speak with any confidence of the relative numerical distances of the stars, on the ground of the method above described. Our only absolute and reliable

knowledge, therefore, comes through the determination of parallax.

II. General Distribution of the Fixed Stars.—Under Nebula, some general facts are stated regarding the irregular distribution of stars in our galaxy, and the conclusions early drawn by Sir William Herschel, as to its form. Those conclusions did not pretend to be other than of a very general nature, and as such they first shadowed forth a mighty truth. But since the views in question were propounded, the structure of our galaxy has been the subject of much elaborate investigation. In attempting to unfold the existing condition of the inquiry, we shall use the language of Professor Nichol, in the last edition of his *Architecture of the Heavens*, descriptive of those labours of M. Struve, in which, with signal ingenuity and success, he has summed up the researches of Piazzi, Bessel, and Argelarder. We are unfortunately unable to spare room for the accompanying illustrations.—"The aim of the Poulkova astronomer was the following:—Suppose a thin slice or disc, having the great circle of the equator in its centre, to be cut out of our stellar sphere, he desired to lay down from observation, the number of the stars of the various magnitudes which are found in it, and to express also the mode of their distribution. That his results might not be affected by uncertainties arising from our comparatively imperfect acquaintance with remote regions, Struve confined himself within the limit of stars of the Ninth Magnitude; so that the chart he gives is merely the representation of the contents of a thin circular disc, whose radius, probably, reaches to the end of existing perfectly *accurate* inquiry. But notwithstanding the limited nature of its pretensions, it is replete with interest, and profoundly suggestive.—In constructing his chart, M. Struve proceeded in the simplest way. By aid of the best catalogues, he first collected and arranged, according to their magnitudes and *direction*, all the stars whose existence and position have been recorded, up to the boundary assigned by himself; and by some curious and ingenious means, he felt enabled to conclude how many in each case had probably escaped observation. The *numbers* of the various orders thus completed, yielded an approximation to their respective *distances;* and then the astronomer proceeded to shade the various regions according to their density or *richness*, in stars. The latter element was obtained as follows:—Taking the entire number of stars belonging to any magnitude, he supposed them equally distributed over the space belonging to that order, which gave him its *mean* or *average* density, and a comparison of the actual number of the same magnitude in any *hour* of right ascension, with this average, established the comparative density of such stars in that *hour* or direction. For instance, there are 37,739 stars of the ninth magnitude, which, divided by twenty-four, gives 1,572 as the average or mean density of this order; the density that would characterize *each hour*, if the stars had been uniformly distributed. But in the first hour, there are only 1,084 of such stars; while in the sixth, we have 3,318. Compared with the *mean*, then the density of that first hour is represented by 0·689, or about $\frac{2}{3}$ the adopted average or unit; while in the sixth hour, it is 2·11. Shadings in their proper places on the chart, of brightness proportional to the two numbers $\frac{2}{3}$ and 2·11, will thus exhibit the construction of these two regions of the sphere; and a similar method enabled M. Struve to represent graphically the actual plan of the heavens, in all regions, and with reference to every order of stars. We shall now study this remarkable chart, in reference to the grander truths it shows forth.—I. It will not escape notice, that while characterized by a general uniformity, or rather by a strongly marked plan, certain minor irregularities, especially within the sphere of stars of the sixth magnitude, are likewise very distinctly manifested. These irregularities are not explained by any system hitherto apprehended. Spots of light in one place, comparatively faint districts elsewhere,—they indicate in the heavens a variety characteristic of all nature; and, as we shall discern afterwards, this too—even amid forms so august—may originate in such processes of change, as diversify and stamp with the cheerfulness of life every other explored portion of the universal order.—II. Passing by all partial irregularities, however, our attention becomes fixed on the main feature of Struve's chart —viz., that grand irregularity, expressive of an extraordinary condensation of stars, along a *belt*, or *stripe*, crossing the whole disc, between the *sixth* and *eighteenth* hours of right ascension. Not only is that belt the densest portion of the disc, but as we withdraw from it to either side, stars of all orders become dimmer; for instance, the line or diameter from hour I to hours XIII, or its neighbourhood, is, as a whole, the faintest in the sphere. This, then, in so far as its equatorial disc is concerned, is the leading district of our stellar system;—a stripe, or belt, towards which stars of all magnitudes have, by some potent influence, been concentrated. And, if M. Struve had portrayed in a similar manner all other discs above and below this equatorial one, as far as the poles of our system, his maps would have exhibited predominating belts running across every one of them, belts so corresponding to the first one, and to each other in position, that they could be laid *above each other*, and made to form, as a whole, one continuous solid slice, standing upon the equatorial disc, and—rising upwards somewhat obliquely—penetrating all the sphere. It is not without considerable difficulty that minds not habituated to representations of solid figure, can comprehend the exact significance of a description like this; on which account, and because of the paramount importance of the conception we would impress, we shall take assistance from

a homely illustration. Place before the eye an ordinary celestial or terrestrial globe; fancy that a wooden horizon of some thickness is made to pass through the globe, or to be cut out of it; and an idea will be had of the equatorial disc pictured by M. Struve. Again, let another section be imagined along the direction of the brazen meridian, so that a thick slice be cut from that portion of the globe, it will nearly correspond with the massive region to which I am now referring, that region of our stellar system, which seems to include the largest proportion of its luminaries of every order; and which, if we were to fancy it accompanied *laterally* by stars strewn comparatively *sparsely* through the neighbouring spaces, would give no very false idea of the chief feature of that grand sidereal sphere whose radius reaches to the depth occupied by orbs of the ninth magnitude.—III. On turning again to the skies, in search of traces there, of the dense mass whose existence—as a *rich vein* through our galaxy—careful observation has thus disclosed, we immediately light on the Milky Way. This zone crosses the equator precisely at these two points at which, in Struve's chart we detect the belt of greatest lustre; so that the former phenomenon is merely the prolongation and external picture of the other. Concerning the Milky Way then, farther, and, in so far, very distinct statements may be now hazarded. If from the sides of that superb girdle, planes be supposed stretched across the sky, they would enclose, as by gigantic walls, a space of shape like a common grindstone; and within this enclosure is the *dense* region of the starry heavens. On referring back to the chart of M. Struve, it is found that the sun is not quite in the centre of the space between these planes, but at a distance from it, ascertained to be nearly equal to that which separates stars of the second, from those of the third magnitude; so that we look at it, not from *within* it, but from a position *outside* of it. It will, of course, not be overlooked, that the questions remain, what is the nature and extent of those lateral spaces or those fainter districts of Struve's sphere: and, how deeply—if that depth can be measured—does the dense belt itself stretch before and after towards immensity? The first inquiry can fortunately be replied to with some confidence—M. Struve, as before, furnishing the means. It will appear that if zones, parallel to the belt of greatest density, were compared with each other, the more distant would be found always the fainter, or the less rich in stars. Nay, this diminution is so certain and determinate, that the Prussian astronomer has been able to ascertain its *law*; a law, whose form, indeed, seems complex—that form, we mean, in which alone we can at present express it—but whose existence shows how emphatic is the fact, that, as we pass laterally away from the central stripe or disc of our cluster, the distances between the stars are found augmenting, *just as with the particles of our atmosphere among its higher regions.* In cases like the present, it is never safe to pass, on the strength of mere analogy, beyond the sphere of positive observation; so that it were wrong to rely absolutely on indications given by any such law, as to the existence of a LIMIT—that, too, not far off—across which our system has no lateral extension; the dark ocean of space lying there fathomless, washing its shores: but we hesitate not to record our belief, that the mysterious boundary has been visited, and these starry fields passed through by telescopic energy. To clear all doubt away, to follow the irregular coasts of our cluster, in its shallowest regions; to survey its gulfs and headlands, and chart their forms,—these, with other grand achievements, await the application of the superb powers called recently into being by the genius of the noble observer at Parsonstown.—IV. The arduous question, however, yet remains—one whose solution will task the loftiest energies alike of mind and art. Through those untrodden paths of the *Milky Way*, can we pass with secure foot? Is the starry demesne endless there, or can the sounding lines of human thought and vision cope with its profundities? I believe that in his earlier papers—having as yet no mistrust in the majestic instrument he had created—our immortal countryman spoke, with an excess of confidence, in his *gauging* powers; but that he duly appreciated the extension of the wonderful zone which, first of all men, he had dared to propose analyzing, appears elsewhere quite as clearly as in his remarks on the intractable depths of the spot in Perseus. The present condition of the inquiry will, I trust, appear from the following considerations:—*First.* When an eye is directed towards a prolonged bed of stars, there is no reason to fancy that it has reached the termination of that stratum, so long as there appears behind the luminaries, which are individually seen, any milky or nebulous light; such light most probably always arising from the blended rays of remoter masses. But if, after struggling long with a nebulous ground, we obtain a telescope that gives us additional stars with *a perfectly black sky*; we then have every reason the circumstances can furnish, on behalf of the supposition, that at length we have pierced through the *stratum*; a probability, indeed, which can be converted into certainty only in one way—viz., when no increase of orbs follows the application of a still larger instrument. This latter test is *absolute*: it intimates a decisive exhaustion of the bed of stars; though it can be employed but rarely, in cases, demanding even for their imperfect investigation, the largest accessible telescopes. When, however, an observer of sound and reliable judgment—one duly exercised in these most delicate inquiries—vouches for the entire dissipation of nebulous back ground, I consider his assertion virtually conclusive of the point; for though we may not even then venture to conclude on the absolute and immediate *termination* of the stellar cluster

at a certain *special* depth, we are assuredly authorized to infer a diminution of its *density* so signal and rapid, that the proximity of a limit may safely be assumed. The phenomenon now alluded to is far from strange; it cannot be unfamiliar to any one who, with the aid of extensive powers, has looked thoughtfully into the Milky Way; and Sir John Herschel, in his recent very interesting analysis of that zone, has adduced the occurrence of such perfectly *black grounds*, as bearing irresistibly on the point at issue—viz., our power to exhaust in certain places the riches of our stratum.—*Secondly.* At times, when this black ground has been reached, and also in other cases before it has been reached, another very remarkable phenomenon appears.—In a field of view lit up with the splendours of the Milky Way, the eye sometimes discerns across these lights—deep, apparently, in onward space—a cluster of stars, of trifling superficial extent, but intense richness; and as there appears no trace of *continuity* between the magnitudes of the orbs composing it and those in the stratum through which we are looking, we are constrained to infer that this cluster of stars is really *isolated;* that between it and the Milky Way there is a vast interval of untenanted space; and, therefore, that in this locality we have pierced beyond the termination of our galaxy. The principle now employed is a very fertile one. Its authority is evidently co-extensive with our fundamental proposition—*that the distances of the stars are roughly indicated by their magnitudes;* and a slight reflection will show that it must reveal to careful inquiry not merely the termination of a prolonged stratum, beyond which an isolated cluster is found to lie; but also all irregularities within that stratum itself—a partial irregularity, a break or vacuity. Dr. Robinson mentions two occasions on which clusters thus singularly associated appeared to him, while using the great reflector at Parsonstown; and he speaks of them with espccial interest, because of the light they cast on the structure of the Milky Way. If, however, that vast instrument, with all the immensity of its range, can produce evidence, nothing more than *probable*, that it has sounded the galaxy; through depths how inconceivable must our bed of stars stretch out, in those its more brilliant regions! And how overwhelming the thought, which first grew up in the mind of the veteran Herschel, that, after all, this is no more than a speck, an islet on the breast of the great sea!—It cannot be doubted, we think, that the truths now unfolded, are in entire consonance with the general views of the illustrious founder of sidereal astronomy, as to the structure of our galaxy. That the galaxy is a limited cluster, comparatively shallow laterally, while, in the direction of the Milky Way, it stretches indefinitely onwards, are propositions seemingly as nearly demonstrated as it were reasonable to expect in inquiries of this nature; but, further than the broad statement, that we are in the midst of a stratum, which, viewed from above, might have an aspect corresponding in so far with many external nebulæ—we presume Herschel himself, would not at present have been inclined to adventure. It is possible, however, to obtain a more accurate conception of the *internal* structure of this extraordinary stratum; with which aim, we shall request attention to certain additional remarks.—1. The illustration which presents the Milky Way within the region of stars of the ninth order, as a solid massive disc, rising obliquely from the equatorial plane, requires considerable modification. We find, from our best catalogues, that the density of the stars does not augment with signal rapidity in the neighbourhood of the Milky Way, until we have gone onwards to the sphere of the *smaller magnitudes.* Nearer us than the sphere of orbs of the *sixth* order, for instance, we detect no *very remarkable* accumulation in the line of the predominating belt; there is enough, perhaps, to reveal the existence of an aggregating influence, within the plane of that belt; but the central part of the disc is far from being a *regular continuation inwards*, of the masses in the Milky Way. It is after we pass the eleventh or twelfth order of *distances* only, that any extraordinary increase of relative density marks those regions; intimating, apparently, that *then* we are touching the interior surface of something like a *ring* or *annulus*, encircling the spaces containing our sun and the luminaries near him: nor, in so far as this is concerned, would we be greatly in error in suspecting that the stellar scheme to which we belong, may be allied to the singular annular forms portrayed in Plate IV., only that the central regions of many of those figures are, in the main, less rich than ours in stars.—2. But we are not permitted to believe that the conception of it as a gigantic and most gorgeous ring, would solve all the peculiarities of our great galaxy; for a narrower scrutiny of its structure banishes from it all appearance of regularity. It is, indeed, only to the most careless glance, or when viewed through an atmosphere of imperfect transparency, that the Milky Way seems a continuous zone. Let the naked eye rest thoughtfully on any part of it, and if circumstances be favourable, it will stand out rather as an accumulation of patches and streams of light of every conceivable variety of form and brightness; now side by side; now heaped on each other; again spanning across dark spaces, intertwining and forming a most curious and complex network; and at other times darting off into the neighbouring skies in branches of capricious length and shape, which gradually thin away and disappear. The appearance of this wonderful stream is so complicated that the inquiry as to its significance may seem next to hopeless. There is one very prevalent and not unnatural conception of which the student must dispossess his mind at the outset, as a first essen-

tial to any comprehension of these marvels. The masses he sees in the Milky Way are not, as *on the face of a picture*, necessarily either at the same distance from him, or connected with each other. He is looking, on the contrary, deep into space; and these luminous forms are the contents of space presented according to the laws of the only possible perspective. A dim streak, for instance, is probably not dim in itself, or less gorgeous than the brighter one it seems to cross; but only a branch of our most complex system, at some inconceivable remoteness, lying athwart the field of view—a portion of one of its far-off convolutions. That this is an accurate interpretation, is amply confirmed by any telescope capable of dissolving these various masses into discrete stars; for the magnitudes of the bodies composing them are neither corresponding nor continuous; they belong to orders indicating very different profundities in space. The view we enjoy, in fact, is, in its chief characteristics, not unlike what would appear were a spectator near the centre of a superb spiral nebula, looking towards its circumference: there, as to us, branching and rounded light clouds, of all degrees of remoteness and comparative brightness, would be presented in one mass before the eye; reason alone could disentangle the complex appearances, and reveal their relations as to distance. Thus regarded, our surrounding zone is revealed in its true magnificence; interminable in its splendour, and baffling all analysis. In that peculiar state of our atmosphere when the cumulus cloud predominates, ranges and banks are usually seen, rising above and behind each other in gorgeous perspective, building up a noble and endless landscape of such hues and forms as occupy one's dreams: such, even *thus*, would appear our Milky Way, if at a breath its surface should part, and through the opening, we saw its ascending cumuli of star clouds, stretching away in unrivalled glory, higher and higher up to where man's eye shall never reach them, or his most vaulting imaginations break in on their repose.—*Lastly*, One other fact connected with our system, remains to be noticed—one especially dwelt on by Sir John Herschel—in that work on the Southern Skies, in which he has worthily closed the greatest, most complete, and most classical series of inquiries yet possessed by sidereal astronomy. On the background of the sky, in regions not *apparently* connected either with the Milky Way, or any foreign cluster, this acute observer has been frequently called on to remark, instead of that perfect darkness usually characterizing recesses which no star illumines, " an exceedingly delicate and uniform dotting, or *stippling* of the field in view, by points of light too small to admit of any one being steadily or fixedly examined, and too numerous for counting, were it possible, so to view them." He has specified no fewer than thirty-seven places distinguished by this strange and evanescent presence, the shadow as of some far away reality, or light blushing through darkness. The phenomenon, indeed, is so faint, that he says, " The idea of illusion has continually arisen subsequently;" but as to its reality, it is enough to read from Sir John's note book, " I feel satisfied the stippling is no illusion, as its dark mottling moves with the stars as I move the tube to and fro;" and more that is similar. What are these fresh intimations from beyond abysses so awful? On examining, by aid of a stellar chart, whether the patches of light could be grouped in any consistent or intelligible manner, the same astronomer found that, with the exception of *three* that appear outlying and disconnected, they form several distinct but continuous streams; and it seems, therefore, that as they must be held to be starry regions of great extent and excessive remoteness, we are constrained to consider them branches or arms of the system of our Milky Way, amid depths to which no adventurous conception ever penetrated before. It is far from the least singular of these recent revelations of Lord Rosse, that, attached even to the simplest and most regular shapes of the external clusters, are stray filaments, dim and sparse, groping outwards, as it were, from the mass of the system, into surrounding vacancy; are *those* such arms attached laterally to the principal regions of our galaxy, or are they portions of its general structure, piercing into vacuities yet more wild and perilous, and carrying its relations onwards towards the sphere of other systems? Whatever their character or function, all hope of giving form or definite outline to the Milky Way, now necessarily disappears; we are dealing with magnitudes so vast, so transcending comparison with any palpable unit, that they merge into what is formless.

III. Proper Motions of the Stars.—Under Sun, the circumstances have been alluded to that led to the definite discovery of the grand motion of translation of our luminary. Not only is it most improbable, that, alone of all the stars, our sun moves in some gigantic orbit; but the discovery of the latter fact, involved the discovery of proper motions in the stars also. The reality and direction of the Sun's path, is established by the circumstance, that the stars *en masse* seem drifting in a certain direction,—that is to say, the *sum of their apparent motions* is of this nature; and that sum is so clear, that no doubt remains as to its reality. But *each single star* does not drift in this way. On the contrary, taking them individually, they seem to move very irregularly; and the irregularity establishes and expresses the existence of *proper motions* for each star. To detect the amount of their proper motions, the effect of the Sun's motion must be eliminated from the apparent changes of place discerned in each star; the residue is the quantity we desire. Thanks to the astonishing perfection of modern astronomical instruments, these

all but evanescent motions are so well established in the case of very many stars, that an ingenious Inquirer, to whom observation owes much, has deemed it not premature to offer a remarkable speculation regarding the nature of the grand galactic system, of which such proper motions are the indications. According to Maedler of Dorpat, the star *Alcyone*—the principal orb in the Pleiades—is at, or very near, the centre of gravity of all the stellar orbs — the point around which the Sun and his innumerable companions are performing their revolutions. He tells us, that, as a consequence of his theory, the Sun's distance from the centre of his orbit, is thirty-four millions of times the radius of the orbit of the Earth, and that the duration of his course is about 19,256,000 years! Astonishing conclusions! But the speculation of M. Maedler in nowise overtakes the complicacy or reaches the elevation of the actual case. The curious concurrence of motions on which he founds it, may have some general and even important cause; but the affections of all the orbs he has examined, though these were augmented an hundredfold, can in nowise be assumed as a key to the mysteries of our extraordinary galaxy. Situated exclusively within a sparse district enclosed by the ring of the Milky Way, these stars are but a few even of the orbs which are scattered there; and towards the massive annulus itself, far less amid its wonderful, its bewildering prolongations, observation has not stretched at all, or taken account of the proper motion of its luminaries. It is as if at a star near the centre of the great spiral of Plate IV., an Inquirer had descried some orderly system comprehending the orbs which are there disposed with comparative regularity, and forgot thereupon the intractable and wild complexity of the object, of which the central mass, however great in itself, is but an insignificant portion. Immersed in the interior of the Nebula, the skies over his head, sparkling with the stars he knows best, that astronomer might for a moment encourage such delusion; remove him, however, to the height from which we inspect his galaxy, show him its fantastic arms thronging with star-clouds, each one of which might be mistaken for a universe; cover with your finger the little circle beyond which he knows the proper motion of no star, and assuredly the ambition will endure with him no longer! Maedler has not succeeded, nor are we sure that the success he sought, will ever be attained. Achievements like these belong to that lofty class, concerning whose realization it were as "unwise to be sanguine" as it would be "unphilosophical to despair."

Stars Multiple. These stars are apparently groups of individual stars exceedingly close to each other—so close, in many cases, that the separation between them cannot be discerned without the aid of the telescope. They are divided into classes according to their complexity; hence we have Double Stars, Triple Stars, &c., &c. On discovering that multitudes of apparent double combinations exist, far beyond what could be expected from mere chance, or optical conjunction,—that is, from the occurrence of one star nearly in the same visual line with another,—Sir William Herschel had recourse to the reasoning employed long before by Michel, and ventured the bold but most fortunate prediction, that these objects would be found to be in physical union, forming revolving systems. The same consideration has been absolutely relied on and extensively employed by M. Struve; nor was its validity called in question, until—comparatively recently—strong objections were started by Professor J. D. Forbes. The scientific difficulty, in so far as this special application of the doctrine of probabilities is concerned, seems to have been settled by Professor Boole of Cork, one of our best mathematicians and most judicious thinkers. He lays down this proposition as applicable to the case,—"If the probability of an indicative law of distribution, and the consequent existence of a double star, is greater than the probability in favour of a random distribution, and a consequent absence of double stars, then the probability in favour of an indicative law of distribution, granting the existence of a double star, is stronger than the probability against a double star, granting the hypothesis of a distribution at random." Constrained to agree with Professor Forbes, that the *numerical* conclusions reached on this subject are not defensible, we consider it clear that a probability, such as that indicated by Michel and Herschel, unquestionably exists; at all events, the belief in it led to discoveries than which none in modern times are more brilliant and interesting. The student who would follow the course of research concerning the double, triple, &c., stars, should study the memoirs and writings of Herschel, South, Struve, Dawes, Villarceau, &c., &c., unfortunately much scattered through the Transactions of our scientific societies. The great practical work is undoubtedly Struve's *Mensuræ Micrometricæ*. Our limited space confines us in this place to a few brief notices of the leading results of such inquiries.

1. The existence, or rather the prevalence of orbital motions among these remarkable combinations is established beyond all doubt. Nay, the periods of revolution are fixed in many cases, and approximated to in others. The periods of the following binary combinations, are, as they are stated, correct, within small limits of error:—

ζ Herculis,	30 years.
ξ Ursæ Majoris,	61 —
p Ophiuchi,	74 —
α Centauri,	77 —
γ Virginis,	169 —
Castor	153 —
σ Coronæ,	608 —

If we could afford time to pass into the fields of

highly probable conjecture, we should come into presence of far vaster periods. The great year of Alcor, Mizar, and the small star near them, cannot be less than 180,000 of our years, while that of the quadruple ε Lyræ, must exceed 500,000! How inconceivable the periods then, that shall represent the completion of one cycle of such a group as the Pleiades: we utterly shrink from raising our thoughts to one of the multitudes of groups in the Milky Way!

2. The energy obeyed by these gigantic motions, is undoubtedly that very force of gravity which controls all movements within the solar system. These orbs move in Ellipses, and Kepler's Law of the Areas is also recognizable as the principle of their velocities. So surely is this ascertained, that the government of gravitation is assumed, in every effort to establish the special orbits of the various groups. One circumstance, however, distinguishes these motions from all that we have detected as characteristic of the orbits of our planets. The orbits of the planets and their satellites are almost circular, or ellipses of very small eccentricity; while the curves of the double stars have every degree of elongation —challenging the freedom of the comets. The orbits in the case of α Centauri and γ Virginis, are especially eccentric, so that at opposite periods of their cycles, the two connected suns must be very close to each other, and very far apart. If each of these suns is attended by planets, how extraordinary the physical condition of these planets, and how inextricable their mechanical relations! Besides passing through the varying climates of a year, depending on its revolution around its own luminary, every planet of either system must undergo the changes of another cycle, whose course is the great period of the binary system, and which, at one of its terms, must subject it to the influences of two suns almost in contact. Under this view, raise our thoughts somewhat higher—to such a triple system as ζ *Cancri*. Restricting attention to two of the stars, whose period is fifty-eight years, we find, as we have just said, two sets of seasons inseparably intermingling. The course of the shorter year of a planet rolling around one of these suns, may be reckoned an *incident* only, a recurring variety within that larger year, which doubtless comprehends many of its returns. As that grand summer and winter succeed, there must come and pass away numbers of minor periods of comparative life and luxuriance, diversifying that longer course; but only when the planet's summer coincides with the summer of its sun, will the glory of its seasons attain its culmination. Call in the third element. Passing slowly along a career far more majestic, another orb is advancing with a cycle of seasons grander still. That orb brings its *third* summer to superadd to the foregoing complexity; one which, in the case we have spoken of, arrives but once in six hundred years; and who shall picture the effects on all life, on all action, on every internal arrangement of these orbs and their dependents, when, in virtue of the mechanism they constitute, the three suns attain their greatest proximity, and shower on each other their most abundant influences! Often one uses the word *bouleversement* or *catastrophe*; and truly when one thinks of the immense width of fluctuation inevitable on provisions like these, or on the opposite condition of every member of such a system in different epochs of its existence, —still further, if, as alone we know the remote past or even the larger relations of the present, fragments, rapid glimpses of moments far apart, or of detached portions of its structure, were all that rested under the eye, how could the word *catastrophe* be avoided, or the idea of something diverse from peaceful and solemn law, which, by overthrowing order, had instituted disturbance and change? Yet in the deep quiet of the night, look at a triple star, and, with your reason, follow the motions of its orbs! So would confusion vanish and perplexity be felt no more, if, from a height superior to that which is his summit now, man could behold unwinding the full destinies of the world.

3. Many further and minuter inquiries have been suggested by the phenomena of the double stars. Three only can be adverted to. *First*, The two stars are frequently of complementary colours. We know little or nothing in the way of explanation. An ingenious theory has been started by M. Doppler, that the great velocities of the two bodies in opposite ways, may, by their shortening or lengthening the undulations of the emitted luminous rays, be an efficient cause. See Abbé Moigno's *Répertoire*, or Professor Nichol's *Architecture of the Heavens*. *Secondly*, Apparent irregularities occasionally appear in the motions of these conjoined bodies, that demand some special explanation. Captain Jacob has recently drawn especial attention to this, in case of the remarkable star in *Ophiuchus*. It seems not unlikely that an opaque orb of great magnitude is revolving around one of these associated suns. *Thirdly*, The idea of the possible existence of massive dark orbs was first started by Bessel. The stars Sirius and Procyon have very irregular proper motions—as if, while they move in a vast orbit, they are also moving around some massive but dark star. If such dark orbs exist, man shall never learn the complete system of things; his highest *formulæ* can only be tentative; and he need not strain after the unattainable.

Stars Variable. A very curious set of orbs which are subject to periodical changes of brightness. About a dozen are known to belong to this class—one of the most remarkable being Algol, which passes from a star of the 2·3 magnitude to one of the 4th and back again in two days, twenty hours, and forty-nine minutes. The longest period known is that of χ Cygni,

which occupies 406 days. It is needless to speculate as to the causes of these fluctuations of brightness. They have been referred to the rotation of these bodies on their axes—their different faces having a different intrinsic brightness. No theory, however, can as yet explain the phenomena.—See a very interesting memoir on this subject by Argelander.—To this class may also probably be referred those strange phenomena of the sudden appearance and disappearance of brilliant orbs. Tycho's star was one. It appeared suddenly in the midst of the constellation *Cassiopeia*, and rose in magnitude until it shone with a lustre beyond that of any fixed star. It surpassed Jupiter in brightness, and was seen even in the day time. After blazing thus memorably for a few months, it began to wane, passed through various hues of colour, and finally disappeared. Kepler saw another such star in the constellation *Serpentarius*. We have had something of the same phenomenon in recent times in the case of η Argus, as described by Sir John Herschel.—No theory can be possible until we shall learn something more concerning our own Sun, and the physical causes of his illumination.

Statics. The science of the equilibrium of forces. There are two modes, according to which, the general problem of Statics may be treated; *first*, by the effort to discover a principle of equilibrium sufficiently general to enable it to comprehend the conditions of the equilibrium of all possible forces; *secondly*, the problem may be considered as a special case of Dynamics; that case—viz., in which the resultants of all the forces applied to a point or a body, become *nil*. The latter method is not the logical one, for it reduces the simple subject of statics to dependence on dynamics, into which higher and more complex ideas are necessarily introduced; nevertheless, until quite recently, it has been the common way of treating this grand division of rational mechanics. Viewed under this latter light, the general expression for the action of forces upon a body, resolved according to rectangular axes, is expressible in six equations; each of which, when equilibrium takes place, must be equal to zero. Three of these equations have reference to the directions in which the forces are acting; and the remaining three—which include the *doctrine of moments*—to the distances of the points of application of the forces, from the three rectangular planes. The six fundamental equations of equilibrium are technically as follows:—

$$\Sigma \text{P} \cos \alpha = o$$
$$\Sigma \text{P} \cos \beta = o$$
$$\Sigma \text{P} \cos \gamma = o$$

$$\Sigma \text{P} (y \cos \alpha - x \cos \beta) = o$$
$$\Sigma \text{P} (z \cos \alpha - x \cos \gamma) = o$$
$$\Sigma \text{P} (y \cos \gamma - z \cos \beta) = o$$

Of these six equations, the last three sum up the doctrine of *moments*; but they have received their true and rational explanation from Poinsot, by aid of his theory of Couples. This theory has been frequently adverted to in our Cyclopædia: its ultimate significance is simply this,—there are only two kinds of motion predicable of a body,—a motion of *translation*, and a motion of *rotation*; these first three equations having reference to *direction*, establish the nullity of any motion of *translation*; the second three, having reference to the moment of a force, in reference to an axis or a plane, establish by their nullity, that there is no motion of *rotation*; so that if the *six* are satisfied, equilibrium must be complete.—The foregoing method of treating the subject of Statics, however, does not now comport with the condition of dynamical science. Dynamics, in its most general form, has been reduced, or thrown back on Statics, by aid of the grand principle of d'Alembert. It behoves, therefore, that Statics be itself previously constructed on the independent basis of some primitive and adequate law of *equilibrium*. Starting from the point from which Archimedes contemplated equilibrium, the illustrious Lagrange detected the required principle, and laid it down at the commencement of his *Mecanique Analytique*. The principle in question, is the famous *Principle of Virtual Velocities* (*q. v.*); and it is not too much to say, that it will henceforth serve as the basis of all great treatises, and all ulterior researches concerning the laws of equilibrium. Discovered first by Galileo, extended by John Bernouilli, and systematized by the industrious Varignon, this principle came into the hands of Lagrange already shaped; but it required an insight like his to discover its importance, and to see, that, in connection with the principle of d'Alembert, it might dominate all Rational Mechanics. It consists in this,—imagine a system of forces or points in equilibrium, momentarily disturbed; estimate the disturbances of all the forces or points, in any direction, provided that direction be the same for all; these disturbances are the *virtual velocities*, and each force multiplied by its virtual velocity, is its *virtual moment*. Now, in consequence of the existence of equilibrium, the sum of these virtual moments must be o. Terming P P′ P″, the several forces; and, according to the notation of Lagrange, $\delta\varrho$, $\delta\varrho'$, $\delta\varrho''$, the corresponding virtual velocities, this principle is expressed by the equation—

$$\text{P}\,\delta\varrho + \text{P}'\,\delta\varrho' + \text{P}''\,\delta\varrho'' + \&\text{c.} = o$$

or, succinctly,

$$\Sigma \text{P}\,\delta\varrho = o$$

To give full effect to a view so general, many modifications were needed in the Transcendental Calculus; but these were all supplied by Lagrange himself, through aid of his *Calculus of Variations*. It may safely be alleged, that while, by aid of the principle of d'Alembert, every

dynamical problem may be reduced to a statical one, so, by aid of the Principle of Virtual Velocities, every statical problem falls back on a problem of pure analysis. True methodology always requires—viz., that more complex inquiries shall, by the introduction of an expressive *formula*, be thrown back on the step preceding them.—The departments of statics are various. Many important classes of problems turn up; but the main ones are those that have reference to the means of resisting Terrestrial Gravity, and to the determination of its own action on separate bodies; the latter having concern with the Theory of Centres of Gravity; the former, with such constructive problems as affect the forms of *Arches*, *Vaults*, *Bridges*, *Domes*, &c., &c. At a more advanced portion of the subject, we meet such problems as these:—What is the form of equilibrium of a rotating sphere? What is its form according to any given law of increase of density from surface to centre? What, under given circumstances, is the most probable law of that increase?—problems affecting many inquiries in Celestial Mechanics.—We can yet scarcely venture to name the subject of the Equilibrium of Liquids or Gases, under the head of Pure Statics. Such laws as there exist are briefly treated in HYDROSTATICS and PNEUMATICS. But molecular forces are rapidly yielding their definite characters to research, and the time is not distant when analysis shall supply methods free from complicacy, and adequate to grasp such problems. We recommend the student to Poinsot, Earnshaw, Whewell, and finally, Lagrange.

Steam (in the scientific sense of the word), means *water in the vaporous or gaseous condition.* In the popular sense, "steam" also means water, in the state of cloud or mist at a high temperature, mingled with water in the vaporous condition. In the sequel, this word will be employed in the scientific sense alone. Steam is a chemical compound of oxygen and hydrogen, in the proportions of eight parts by weight of oxygen nearly to one of hydrogen. Its composition by volume is such, that the quantity of steam which, if it were a perfect gas,* would occupy one cubic foot at a given pressure and temperature, contains as much oxygen as would, if uncombined, occupy half a cubic foot, and as much hydrogen as would, if uncombined, occupy one cubic foot, at the same pressure and temperature; so that steam, if it were a perfect gas, would occupy two-thirds of the space which its constituents occupy when uncombined. Hence is deduced the following computation of the weight which one cubic foot of steam would have, at the temperature of 32° Fahrenheit, and pressure of one atmosphere (or 14·7 lbs. on the square inch), if steam were a perfect gas, and if it could exist at the pressure and temperature stated.

* For the definition of the term *perfect gas*, see the article HEAT, MECHANICAL ACTION OF, § 9.

(*Data from the experiments of Regnault.*)

	lb.
Half a cubic foot of oxygen, at the pressure of one atmosphere and temperature 32°,...	0·044628
One cubic foot of hydrogen,................	0·005592
One cubic foot of steam in the ideal state of perfect gas, at one atmosphere and 32°,...	0·050220

If steam were a perfect gas, the weight of a cubic foot could be calculated for any given pressure and temperature by the following formula:—Weight of a cubic foot = 0·05022 lb. × pressure in atmospheres,

$$\times \frac{493° \cdot 2}{\text{Temp.} + 461° \cdot 2}.$$

For example, at one atmosphere of pressure, and 212°, the weight of a cubic foot of steam would be

$$0·05022 \times \frac{493° \cdot 2}{673° \cdot 2} = 0·03679 \text{ lb.}$$

But steam is known not to be a perfect gas; and its actual density is greater than that which is given by the preceding formula, as has been shown by the experiments of Messrs. Fairbairn and Tate. The most probable method of indirectly determining the density of steam, is by computation from the latent heat of evaporation, according to principles already explained in §§ 19 and 20 of the article on the Mechanical Action of Heat; from which it appears, that at one atmosphere and 212°, the weight of a cubic foot of steam is probably 0·0379 lb. The greatest pressure under which steam can exist at a given temperature, which is also the least pressure under which liquid water can exist at the given temperature, is called the *pressure of saturation* for steam of the given temperature; the temperature is called the *boiling point* of water under the given pressure. The pressure of saturation is the only pressure at which steam and liquid water can exist together in the same vessel at a given temperature. Many experiments have been made to determine the relation between the boiling point and the pressure of saturation of steam; the latest and most accurate, which have superseded all others, are those of M. Regnault, published in the *Memoirs of the Academy of Sciences for* 1847. In 1849 it was shown (*Edin. Phil. Jour.*, July, 1849), that the results of those experiments are accurately represented by a formula, already quoted in § 19 of HEAT, MECHANICAL ACTION OF. In § 19, 20, and 21, of the same article, are given formulæ for the computation of the *latent heat of steam* (or the heat which disappears in evaporating the water), and of the *total heat of steam* (or the whole heat expended in raising a given quantity of water from a certain fixed temperature to a given temperature, and then evaporating it). Steam, which is not in contact with liquid water, may be raised to a temperature higher than the boiling point corresponding to its pressure; it is then said to be *superheated*, and is analogous in its condition to a permanent gas. Little is yet known of

the mechanical properties of superheated steam. Steam, which expands in performing work, cools so much, that a portion is liquefied. Steam, on the other hand, which expands in rushing through an orifice, has its temperature maintained by the friction of its particles above the boiling point due to the diminished pressure. Besides the proportions of machinery (specially treated of in the ensuing article, STEAM ENGINE,) steam is extensively used in the arts as a convenient vehicle for the transmission and diffusion of heat.—Further information on those properties which steam possesses in common with other vapours, will be given in VAPOUR.

Steam Boilers. For remarks on this subject see Appendix.

Steam Engine. A machine in which heat is made to perform work by means of the elasticity of steam.—§ 1. *Historical Sketch.*—The origin of the steam engine, in its rudest form, is lost in antiquity. The earliest written account of mechanism in which heat is made to perform work by means of steam, is contained in the *Pneumatics* of Hero of Alexandria, who flourished about 130 B.C. That author describes a rotatory engine, driven by the reaction of jets of steam issuing from orifices in revolving arms, and also an engine in which the pressure of steam is made to raise liquid by expelling it from a receiver. An apparatus similar to the last is described by Giovanni Battista della Porta, in his *Pneumatics*, published in 1601. A French engineer, Solomon de Caus, in a work entitled *Les Raisons des Forces Mouvantes*, published in 1615, described a machine for propelling a jet of water to a great height by the pressure of steam evaporated in the same vessel from which the water was ejected. In 1629, Branca described an engine, in which a wheel was driven round by the impulse of steam against vanes. The Marquis of Worcester, in his work called *A Century of the Names and Scantlings of Invention*, &c., published in 1663, described a machine for raising water by the pressure of steam. So far as the description is intelligible, it appears that this machine differed from that of de Caus, in having a separate boiler for the production of the steam which forced water out of other vessels; and it appears further, from the Diary of Cosmo, Grand Duke of Tuscany, that the machine of the Marquis of Worcester had been constructed, and was in operation at Vauxhall, in 1656. About 1697, Savery invented an engine in which water was not only (as in those of de Caus and Worcester), forced above the level of the engine by the pressure of the steam, but was also raised to the level of the engine, from a lower level, by the pressure of the atmosphere, after the condensation of the steam in the water-receiver, by means of cold water externally applied. In all the machines hitherto described, the steam either acted by its momentum alone, or by pressing directly on the surface of water. The first invention of the important idea of making steam afford the means of driving a *piston*, which should communicate motion to mechanism, appears to be due to Denis Papin, who, about the year 1690, constructed a working model, consisting of a vertical cylinder with a piston. In the lower part of the cylinder was placed a small quantity of water. On placing a fire under the cylinder, the water evaporated and lifted the piston; on removing the fire from the cylinder, or the cylinder from the fire, the steam was condensed, and the piston forced down by the pressure of the atmosphere. Papin proposed that engines on this principle should be made to work pumps, and also, by means of rack and pinion work, and ratchet wheels, to drive paddle wheels of vessels, and other revolving mechanism. Papin had, about ten years before, invented the safety valve for boilers. In 1705, Newcomen combined the cylinder and piston of Papin, with the separate boiler of Worcester and Savery, and the surface condensation of the latter, and produced the well known atmospheric engine for pumping mines. He afterwards rendered the condensation more rapid and complete by injecting a shower of cold water into the interior of the cylinder. Apparatus for enabling the engine to open and shut its own valves, was introduced by Humphry Potter, and improved by Beighton. The high pressure engine was invented in 1725 by Leupold. About 1770, the details of the atmospheric engine were much improved by Smeaton. Up to this period the progress of the steam engine had consisted in a series of ingenious contrivances and empirical improvements, unaided by theory; and, notwithstanding all that had been done, it was an imperfect and most wasteful machine, practically suited to the sole purpose of draining mines, at a great expense of fuel. But now had arrived the time when one man was to discover and to apply to practice all those principles upon which depend the economy and the utility of the saturated steam engine, leaving to his successors only to develop, extend, and perfect his inventions, and to improve matters of detail—until the saturated steam engine shall be superseded by some more economical and useful prime mover. In 1759, James Watt had his attention directed by Robison to the subject of the steam engine, and for a few years afterwards made various experiments on the properties of steam. In 1763 and 1764, Watt, while engaged in the repair of a small model of Newcomen's engine (belonging to the University of Glasgow, and since preserved by that University as the most precious of relics), perceived the various defects of that machine, and ascertained by experiment their causes. Early in 1765, he discovered those principles of the action of the steam engine, which are embodied in an invention described by himself in the following words, in the specification of his patent of 1768:—

"My method of lessening the consumption of steam, and consequently fuel, in fire engines, consists of the following principles:—

"*First*, That vessel in which the powers of steam are to be employed to work the engine, which is called the cylinder in common fire engines, and which I call the steam vessel, must, during the whole time the engine is at work, be kept as hot as the steam that enters it; first, by enclosing it in a case of wood, or any other materials that transmit heat slowly; secondly, by surrounding it with steam or other heated bodies; and thirdly, by suffering neither water nor any other substance colder than the steam, to enter or touch it during that time.

"*Secondly*, In engines that are to be worked wholly or partially by condensation of steam, the steam is to be condensed in vessels distinct from the steam vessels or cylinders, although occasionally communicating with them; these vessels I call condensers; and, whilst the engines are working, these condensers ought at least to be kept as cold as the air in the neighbourhood of the engines, by application of water, or other cold bodies.

"*Thirdly*, Whatever air or other elastic vapour is not condensed by the cold of the condenser, and may impede the working of the engine, is to be drawn out of the steam vessels or condensers by means of pumps, wrought by the engines themselves, or otherwise.

"*Fourthly*, I intend, in many cases, to employ the expansive force of steam to press on the pistons, or whatever may be used instead of them, in the same manner in which the pressure of the atmosphere is now employed in common fire engines. In cases where cold water cannot be had in plenty, the engines may be wrought by this force of steam only, by discharging the steam into the air after it has done its office.

"*Lastly*, Instead of using water to render the pistons and other parts of the engines air and steam tight, I employ oils, wax, resinous bodies, fat of animals, quicksilver, and other metals, in their fluid state."

The expense of carrying out of Watt's invention was at first defrayed by Dr. Roebuck. On his retirement from the enterprise, his place was taken by Matthew Boulton of Birmingham, whose liberality and energy furnished all that was necessary to render the genius of Watt practically available.—Few patents have had their validity more obstinately contested than that of Watt's great invention, and the successful result of the trials of which it was the subject, has greatly contributed to ascertain and fix the interpretation of the patent laws.—Papin's method of producing a rotatory from a reciprocating motion, by ratchet work, had proved abortive. Before 1778, Watt had invented the double acting steam engine, and the application of the crank to the steam engine; but the latter invention having been pirated and patented by another, Watt invented and patented other methods of producing rotatory from reciprocating motion, which were used until the patent for the crank expired; after which time the use of the crank became general. The adaptation of the steam engine to the production of rotatory motion was the crowning improvement, which led to its employment as the prime mover of every kind of mechanism.—The improvements on the steam engine since the time of Watt, have chiefly related either to the boiler and furnace for the details of mechanism, to the more full development of Watt's principle of using the expansive force of the steam to drive the piston, or to the means of applying the steam engine to the propulsion of carriages and ships. —The true mathematical theory of the relation between the evaporating power, the load, and the speed of the steam engine, was first expounded in 1835 by the Count de Pambour. The history of the true theory of the relation between the heat expended and the work performed in the steam engine, as well as in other engines in which heat is the source of power, has already been sketched in § 1 of HEAT, MECHANICAL ACTION OF.—The following four engines may be considered as belonging to classes distinct from Watt's engine. They have all been constructed and set to work, but none of them have yet been extensively used. 1. The reaction steam engine of Mr. Ruthven, on the principle of that of Hero. 2. The momentum or vortex steam engine of Mr. Gorman, in which steam is introduced by jets at the circumference of a circular case, drives a vane wheel or turbine revolving in the case, and escapes at a central orifice. 3. The steam and ether engine of M. du Trembley, mentioned in § 30 of HEAT, MECHANICAL ACTION OF. 4. The regenerative superheated steam engine of Mr. Siemens, mentioned in § 31 of the same article. AUTHORITIES on the steam engine:—Stuart's *Descriptive History of the Steam Engine* (the best authority on its early history); Tredgold *On the Steam Engine*, edited by Woolhouse; Robison, Watt, and Scott Russell, *On the Steam Engine*, *Encyclopædia Britannica;* Scott Russell *On Steam Navigation*, &c.; Murray's *Rudimentary Treatise on the Marine Engine;* Bourne *On the Steam Engine*; de Pambour *On the Locomotive Engine;* de Pambour *On the Theory of the Steam Engine;* Arago, *Sur les Machines à Vapeur* (Historical Notice, published in the *Annuaire du Bureau des Longitudes for* 1837); Muirhead's *Mechanical Inventions of James Watt;* Fairbairn's *Useful Information for Engineers;* Clark *On Railway Machinery;* Rankine *On the Steam Engine.*

§ 2. *General Description of the Parts of the Steam Engine.*—The following are the principal parts, which, with their appendages, constitute a steam engine. 1. The furnace and boiler; 2. The cylinder with its piston; 3. The condenser with its air pump (these are wanting in non-con-

densing engines); 4. The Mechanism.—1. The *furnace*, with its appendages, consists of grate, ashpit, flues, and chimney; the chimney sometimes contains a damper for regulating the draught of air. The furnace is sometimes contained within the boiler. The *boiler* is made of iron or copper, and often contains internal flues and tubes. Amongst its appendages are, the *feed pump*, and other apparatus for providing it with a regular supply of water; the *safety valve* for permitting the escape of steam before the pressure becomes dangerous; the *fusible plug* for melting and allowing the steam to escape when the temperature becomes dangerously high (a contrivance not to be relied on); the *vacuum valve*, opening inwards, to admit air and prevent the boiler from collapsing if the steam in it should be condensed; the *water-gauge cocks* and *water-gauge tube* for showing the level of the water, so that the engineman may ascertain whether it stands sufficiently high to cover all parts exposed to the fire; the *pressure-gauge*, for indicating the pressure of the steam; the *man hole*, closed by a steam tight cover, for getting access to the interior of the boiler; the *blow-off cock* for emptying the boiler of water when it is to be cleansed, and in marine engines for occasionally discharging brine, so as to prevent over-concentration of the sea water in the boiler, which would lead to the deposit of salt. Marine boilers are sometimes provided with *brine pumps*, to draw off a fixed quantity of brine from the bottom of the boiler at each stroke of the engines; the outer parts of the boiler are sometimes *clothed* with felt, wood, brickwork, or ashes, to prevent loss of heat. Large engines have often several furnaces and boilers. 2. The *boiler* and *cylinder* are connected by means of the *steam pipe*, in which is the *throttle valve* or *regulator*, for adjusting the opening for the admission of steam to the cylinder, and sometimes also the *cut-off valve* or *expansion valve*, for cutting off the admission of the steam to the cylinder at any required period of each stroke of the piston, leaving the remainder of the stroke to be performed by the expansion of the steam already admitted. The *cylinder* may be single or double acting. In a single acting engine (used generally for pumping only), the piston is forced in one direction by the pressure of the steam, and made to return in the opposite direction when the steam is discharged by the action of a weight or *counterpoise*. In a double acting engine (which is the class of engine used for locomotion, and for driving machinery, and sometimes for pumping also), the piston is forced in either direction by the pressure of the steam which is admitted and discharged at either end of the cylinder alternately. The admission and discharge of the steam take place through openings near the ends of the cylinder called ports, connected with passages called *nozzles*, which are opened and closed by *induction* and *eduction valves*. Sometimes the induction and eduction valves are combined in one valve, called a *slide valve*. In *non-condensing* engines (conventionally called *high pressure engines*), the waste steam discharged from the cylinder escapes into the atmosphere through the *blast pipe;* in locomotive engines, as well as some others, the blast pipe is placed in the centre of the chimney, so that the successive blasts of steam discharged from it augment the draught of air through the furnace, and cause the combustion of the fuel to be more or less rapid, according as the engine is performing more or less work. The *cylinder cover* has in it a *stuffing box* for the passage of the piston rod; in large engines there are sometimes more than one piston rod and stuffing box, and sometimes a tubular piston rod, called a *trunk*. The cylinder cover is also provided with a *grease cock*, to supply the piston with unguent. In many large engines, there is a spring safety valve, called an *escape valve*, at each end of the cylinder; the chief use of which is to discharge water, which may condense in the cylinder, or be carried over in the liquid state from the boiler, by what is called *priming*. To prevent loss of heat, the cylinder is sometimes enclosed in a casing, called a *jacket*, the intermediate space being filled with hot steam from the boiler, or hot air from a flue; outside this, there is a *clothing* of felt and wood. *Double cylinder* engines have two cylinders; the steam being admitted from the boiler into the first cylinder, and then filling the second by expansion from the first. 3. The ordinary *condenser* is a steam and air-tight vessel of any convenient shape, whose capacity is from $\frac{1}{4}$ to $\frac{1}{2}$ of that of the cylinder; good authorities consider that it ought to be at least $\frac{1}{2}$, and might with advantage be more. The steam discharged from the cylinder is liquefied in it by a constant shower of cold water from the rose-headed *injection valve*. In land engines the *injection water* comes from a tank called the *cold well*, surrounding the condenser, and supplied by the *cold water pump;* in marine engines, it comes directly from the sea. In the *surface condenser* (which has been tried with varying success), the steam is liquefied by being passed through tubes or other narrow passages surrounded by currents of cold water. The condenser is provided with *blow-through* valves, communicating with the cylinder, usually shut, but capable of being occasionally opened, and with a *snifting valve* opening outwards to the atmosphere; through these valves steam can be blown to expel air from the cylinder and condenser before the engine is set to work. The condenser has also a *vacuum gauge*, to show how much the pressure in it falls below that of the atmosphere. The water, the small portion of steam which remains uncondensed, and the air which may be mixed with it, are sucked from the condenser by the *air pump* (whose capacity is from $\frac{1}{8}$ to $\frac{1}{6}$ of that of the cylinder), and discharged into the *hot well*, a tank from

which the feed pump, formerly mentioned, draws the supply of water from the boiler. The surplus water of the hot well in land engines is discharged into a pond, there to cool and form a store of water for the cold well; in marine engines, it is ejected into the sea. 4. The principal parts of the *mechanism* may be generally described as follows:—In all except certain peculiar classes of engines, there is a *parallel motion* for guiding the head of the piston rod to move in a straight line, consisting either simply of straight cheeks or slides, or of a combination of levers and linkwork, invented by Watt, and more or less modified by others. The peculiar classes of engines above excepted, are—first, *trunk engines* (including Mr. Hunt's z crank engine), where the shuting box is the guide; secondly, *oscillatory engines*, in which the head of the piston rod is directly connected with the crank, and the cylinder oscillates on trunnions; thirdly, *disc engines*, in which the functions of a cylinder are performed by a vessel of the figure of a spherical zone, and those of a piston by a disc having a motion of nutation in that zone; and, fourthly, *rotatory engines*, in which the piston revolves round an axis. Trunk engines and oscillatory engines are of common occurrence in steam-ships. The z crank engine has not yet been tried on a large scale. Disc engines are said to answer well, but are of rare occurrence. Rotatory engines of various kinds have often been tried, but seldom with good results. In single acting engines for pumping water, the pump rods are worked either by direct connection with the piston rod, or through the intervention of a beam. In double acting engines, the power is communicated to a revolving *shaft*, driven by means of a *crank* and *connecting rod*, with or without the intervention of a beam. (In oscillating engines the *piston rod* and connecting rod are one). Most marine and locomotive engines, and many stationary engines, have, in order to equalize the action of the power, a *pair* of cranks at right angles to each other, driven by a *pair* of pistons in a *pair* of cylinders, with their appendages; and are, in fact, *pairs of engines*. In stationary engines the shaft carries a *fly-wheel*, to distribute and equalize irregularities in the action of the power by its inertia; this function is performed in marine engines by the inertia of the paddle-wheels or screw, and, in locomotive engines, by the inertia of the driving-wheels and of the engine itself. The feed pump, and other pumps which are appendages of the engine, are worked by the mechanism; so also are the induction and eduction valves, through what is called the *valve gearing* or *valve motion*,—a part of the machinery which is under the control of the engineman, and so contrived as to enable him to stop and reverse the motion of the engines at will, and whose forms are too various and intricate to be here described in detail. In engines for manufacturing purposes, the mechanism comprises a revolving pendulum, called a *governor*, which regulates either the admission of steam by the throttle valve, or the cut-off by the expansion valve, so as to make the engine move with a nearly uniform speed. The governor as applied to regulate the speed of water mills and wind mills is of old date; it was first applied to the steam engine by Watt, and has been of late very much improved by Mr. Siemens. In marine and locomotive engines, and winding engines for collieries, the admission and cut-off of steam are regulated at the discretion of the engineman.

§ 3. *Power of the Steam Engine.*—The *available power* of a steam engine is the *useful work* which it performs in some given time. The *indicated power* is the work performed by the steam upon the piston, is equal to the product of the mean available pressure of the steam, the area of the piston, and the distance traversed by the piston in a given time, and is greater than the effective power by the amount of energy expended in overcoming the resistance of the engine. The power of an engine, whether indicated or available, is expressed either in foot pounds (that is, pounds lifted one foot), in some fixed time (such as a minute or an hour), or in *horse power*, as defined by Watt; one horse power being 33,000 foot pounds per minute, or, what is the same thing, $33,000 \times 60 = 1,980,000$ foot pounds per hour. This is the meaning of the *actual* horse power of an engine. But there is, besides, employed in commerce, a measure called *nominal horse power*, which is simply an arbitrary and conventional method of expressing the *size* of the engine, without reference to its actual power. To compute the nominal horse power of an engine, the effective pressure of the steam on the piston is assumed to be, in some localities 7, in others $7\frac{1}{2}$ pounds per square inch. The velocity of the piston is assumed to be 120 feet per minute $\times \sqrt[3]{\text{length of stroke in feet}}$. Lastly, the foot pounds of work per minute computed on these assumptions, are divided by 33,000. This is a rule adopted in some localities and by some engineers; but the rules of other localities and other engineers are different; and, in fact, the system of describing engines by nominal horse power is uncertain, inconvenient, and absurd. The nominal horse power of different engines is from $\frac{2}{3}$ to $\frac{1}{5}$ of their actual horse power. The *duty* of an engine is the work performed by a given quantity of fuel, such as one pound. (The Cornish engineers state the work performed by a *Cornish bushel*, or 94 pounds of coal). The *efficiency* of an engine is the ratio of the work performed to the mechanical equivalent of the heat expended. See HEAT, MECHANICAL ACTION OF, §§ 22 to 31 inclusive. The *duty* of a pound of coal varies in different classes of engines from about 100,000 to 1,900,000 foot pounds. These are extreme results, as respects

wastefulness on one hand, and economy on the other. In good ordinary engines, the duty varies from 200,000 to 700,000. The *efficiency* of steam engines lies between the limits 0·02 and 0·2 in extreme cases, and 0·04 and 0·125 in ordinary cases.

Steel Yard. See BALANCE.

Stereographic. See PROJECTION.

Stereoscope. As is explained and illustrated in PARALLAX BINOCULAR, in viewing an object, each eye sees a different figure of it, some parts being hidden from the one eye which are visible to the other, and *vice versa*. It is by the union of these two somewhat dissimilar pictures on the two retinæ, that the ordinary mental impression of any object or series of objects is produced.—This *dissimilarity* of the pictures formed in each eye is of fundamental importance to the theory of the stereoscope, and may be further made evident by inspection of the diagram in fig. 1. Let

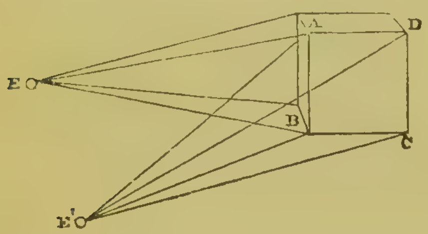


Fig. 1.

A B C D represent a cube, as seen by the two eyes placed at E and E′. The eye E will see only the side A B, no rays of light from the remaining sides A D, D C, B C being able to reach E, they will be invisible to it. The eye E′ being, however, differently placed, will receive rays from the sides A B and B C, which will be both visible to it, and thus the two pictures obviously differ. Not only is this the fact, but every point, even when seen by both eyes, appears to each in a different direction; for instance, the point B is seen by E along the line E B, and by E′ along the line E′ B. How then, it may be asked, can it be that the mind only perceives one image of the cube and that in one direction and occupying one single position? The answer to this question will be found discussed at some length in PARALLAX BINOCULAR, and it must here suffice to state that when the eyes are so turned to any point of an object that its image in each eye falls on the point of distinct vision, then the point is seen single with the two eyes, while all other points of the object appear more or less separated into two, yet, as the vision of these two images is nowhere perfectly distinct, the inconvenience is scarcely noticed. By rapid movement of the eyes each part in succession is brought on the point of distinct vision, and the mind satisfies itself that it is looking only on one single object. In viewing a *picture*, the same dissimilarity of view, as seen by each eye separately, does not occur, as the whole figure is on one plane, and one part of it cannot by its projection intercept the view of another. There is thus an important difference between the appearance presented by the real object and that given by even the best drawn representation of it. The one appears solid and real, the other seems flat and wants relief.—To give the picture this relief it would be requisite to put it into the same circumstances as the real object, viz., to enable it to present two different aspects, one to each eye; in fact to make two pictures and at the same time to cause them both to, *apparently*, emanate from the same position. The first, viz., to draw two separate and somewhat dissimilar pictures is done by the hand or by the camera obscura; and to cause them apparently to emanate from the same position or object, is accomplished by the instrument called the stereoscope. When this is done, the pictures present to the mind the aspect of a real and solid substance in full and round relief, hence the name *Stereoscope*, which signifies an instrument for solid sight.—It has been already mentioned that, on turning both eyes toward any point of an object, so as to see it distinctly, or in optical language, on turning the *optic axes* toward it, it is seen in two different directions, that is, it overlaps different parts of the background. Thus in fig. 2 the point O

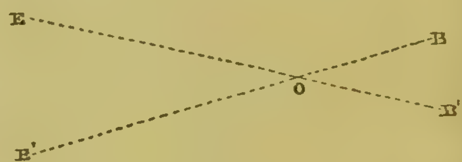


Fig. 2.

is seen by E projecting against the object B′, and by E′ against the object B. This, it will be observed, never can occur with any part of a flat picture, and is one of the chief means by which the mind distinguishes between a picture and a real object in relief. This being the case, it will be requisite that in the two pictures to be united by the stereoscope the objects should overlap different parts of those behind them, and in the same degree as they would do if observed in the real scene by each eye separately. This is the *dissimilarity* of the two stereoscopic pictures which is requisite and which has been referred to above. It is exactly that which is produced by taking the drawings from points slightly differing in position in a direction perpendicular to the line of sight. It is obvious on the slightest consideration that, in many cases of common vision by the two eyes, the picture of any line or surface on the one eye must be longer and larger than on the other. This will always be the case when the object is nearer one eye than the other. The same thing is often observed in the two stereoscopic pictures, and indeed it is easy to draw two lines, one of them longer than the other, and then to combine them by means of the stereoscope. A question has arisen in

uch case, how it is that a single picture is presented to the mind? Mr. Wheatstone, to whom he world is chiefly indebted for the invention of he stereoscope, supposes that the eye has the ower by some means to modify within certain imits the dimensions of the impressions made on t in such a manner as to cause the two impressions to appear of the same size and so to combine into one; and he surmises that the vision f pictures in relief, by means of the stereoscope, s caused by the combination of the two single iews, in exactly the same manner as takes lace with those presented by the two eyes in ommon vision. Sir D. Brewster, the highest ving optical authority, on the other hand, holds hat the eye has no power to modify the shape r dimensions of the images made on it, and that he apparent coalescence of images of different engths is a deception caused by the habit of egarding chiefly the points of distinct vision here the two lines are really brought into oincidence, and the indistinctness of the other ortions of the impressions, added to the circumstance that one or other of the separated portions f the images, by a peculiarity of vision, disppears and without minute scrutiny may be ltogether overlooked. He holds the vision of he two combined pictures, in solid relief by eans of the stereoscope, to be produced by the ame process of vision as that by which objects hemselves are seen in relief by the unassisted yes. For the full explanation of this theory, he reader is referred to Brewster's *Optics* and his ecent elegant work on the stereoscope; but the eneral nature of it may be understood from what llows. Every object is seen double and indisnct except the point on which the optic axes nverge, and it is only by a rapid movement or play" of the optic axes from point to point at a perfect impression of the whole of an bject or scene is presented to the mind. While is rapid *alteration* of the position of the point convergence of the optic axes is thus necessary give a correct and single impression, it is at e same time most useful in determining the rious *distances* of the different parts to which is directed, or of indicating their relief. Thus, fig. 3, the two eyes are represented as directed

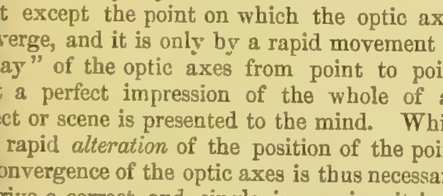


Fig. 3.

such a manner that the images of the object fall upon E and E′, the two points of distinct sion, or, in other words, the optic axes conrge on O. Any other object D will be seen uble and indistinct till, by an exertion of muscular effort, the optic axes are so turned that they converge on it, when its images *e* and *e′* move to E and E′, the points of distinct vision, and it is at once seen single and distinct. This muscular change is accompanied by a sensation which points out the alteration of the position of the point of convergence of the optic axes, and of course the nearness or greater distance of the point to which they are directed. In looking at the different distances imitated on a picture, no such change of the distance of the point of convergence is required, and hence the sense of flatness perceptible in looking at such a delineation. In ordinary vision, then, it is the opening out and closing in, so to speak, of the optic axes, which indicates the difference of distance or the degree of relief of objects one from another, and this is the chief reason why the eyes have been placed apart instead of being united like the two halves of the nose in the middle of the face. The explanation of solid vision, by means of the stereoscope, immediately follows from this as the stereoscopic pictures are nothing else than the two views as taken by the two eyes separately and laid near each other so as to be easily made to appear, by reflection or refraction, as if they came from the same object. The stereoscope then, so to speak, lays the one view over the other and produces the same degree of coincidence as in common vision, that is a coincidence of one point with the corresponding point of the other view.—The play of the optic axes from point to point accomplishes the successive unions of the duplicate impressions from the two pictures, as has already been explained, with regard to vision with two eyes. It is because of this *alteration* of the *distance* of the point of convergence of the optic axes, necessitated by the two distinct pictures, that the perception of relief arises. Sir David Brewster further shows how the exact amount of relief or difference of distance of objects can be determined. and denominates this the faculty of seeing *depth* or a third dimension in space. There is no doubt but in the main this is the true explanation of stereoscopic vision, but it would appear rather that the perception of difference of distance is got in this way, and that the amount of it is a matter of inference from the known nature of the objects and the other probabilities from the appearances which they present. This last surmise might be easily strengthened by arguments drawn from the fact that the same amount of change of the angle of convergence of the optic axes, might indicate in passing from a church tower to a mountain side, a distance of many miles, or in passing from one leaf to another in the near ground, a separation of only as many feet or yards, and also from the circumstance that even in ordinary vision, the mind is so exceedingly easily deceived in estimating distances by the impressions received by means of the eyes.—Sir D. Brewster has shown that the ancients

were to a considerable extent acquainted with some of the principles upon which the stereoscope is based, particularly with the difference between the impression obtained by the two eyes, but there can be no doubt but that it is to Mr. Wheatstone of London that the world is indebted for the first tangible discovery of the instrument.—In the year 1838 Mr. Wheatstone presented to the British Association his reflecting stereoscope, with views of geometrical figures drawn from two separate points of sight. These figures, being put into the instrument and viewed by means of the reflecting mirrors, appeared to occupy the same position, and presented the astonishing aspect of full and real relief, the aspect of a mere picture having vanished. This instrument, although now, and perhaps temporarily, superseded by the refracting stereoscope, is still for many reasons interesting. In fig. 4,

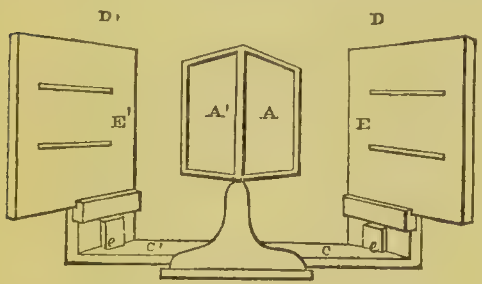


Fig. 4.

E and E′ represent the two pictures on their supports D D′. The mirrors A and A′, inclined as shown, are so placed that when the face of the observer is approximated to the line of their junction, one eye sees the one picture in M and the other sees the other picture in M′, and by a little adjustment the reflected images appear to emerge from the same position, and are easily combined into one, as has already been described. —The Lenticular stereoscope, as it has been named by its inventor, Sir David Brewster, is another form of the instrument, and even more simple in its construction. It is now made of

Fig. 5.

many different forms, one of which is represented in the annexed fig. It consists of a box, somewhat of the shape of a double opera glass, for holding the pictures and excluding stray light and extraneous objects from the view, and a pair of refracting eye-pieces for producing the approximation of the images of the two pictures. These eye-pieces are composed of a sliding tube containing half a lens, the lens being double convex and of about six or seven inches focal length. The semi-lenses are so placed that their two thin edges are next each other. In fig. 6, F and F′ represent the two pictures, L and L′ the

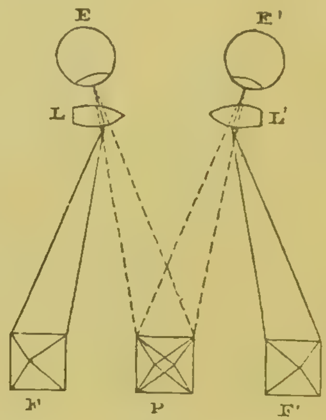


Fig. 6.

semi-lenses refracting the rays which pass through them in such a manner that they arrive at the eyes E and E′ as if they came from one single picture placed at P. In the best stereoscopes the eye-pieces are made to slide, that the lenses may be drawn nearer or farther from each other, so as to enable the eyes to take advantage of more or less powerfully refracting points of the surfaces, and thus produce the desired degree of superposition of the images.—Now that photography affords the means of taking two absolutely correct representations of any object or scene from two points related to each other in the same manner as the two eyes of an observer, nothing could be more perfect than the results accomplished by the stereoscope. Views of statues, gardens, forests, cities, glaciers, and the whole variety of natural scenery, from the tortuous ravine to the far extending plain or boundless expanse of mountain distance, can be presented to the stereoscopic observer, with an accuracy and fulness of impression which may be truly called wonderful. To those who complain of the flatness and want of relief in pictures, the marvel-working stereoscope will accomplish all they can desire.

Storms. These are of various sorts, viz., strong *gales* of wind, regular and rotatory *hurricanes*, such as *typhoons* and *thunder-storms*. See THUNDER-STORM and WINDS.

Strain. See ELASTICITY.

Strength. See ELASTICITY.

Stress. See ELASTICITY.

Sun. This majestic orb, the centre of all planetary movements—the source of the light and

heat enjoyed by the worlds that roll around it, has a diameter of no less than 882,000 miles—about 3·7 times our distance from the moon! Its density is comparatively small, owing, it may be, to the intense heat which must prevail through a large portion of its mass. That density is only one-fourth the density of the earth, so that the *mass* is not proportional to the *size*—being only 354936 times the mass or weight of the earth. Gravity at its surface is 27·9 times the amount of terrestrial gravity; in other words, a body estimated by a spring balance at one pound on the earth's surface, would at the surface of the sun exert a force 27·9 times as large. This great globe however, presents one important analogy with the planets; it rotates on an axis—making an angle of 7° 9′ with the plane of the earth's orbit—in twenty-five days, eight hours, and nine minutes. The amount of light sent forth by the sun is not exactly measureable. The amount of heat has, by aid of the Pyrheliometer of Pouillet, and the Actinometers of Herschel and other physicists, been approximately computed. The amount is certainly enormous. It is equivalent in mechanical effect to the action of 7,000 horse power on every square foot of the solar surface, or to the combustion on every square foot of upwards of $13\frac{1}{3}$ cwts. of coal, per hour! Concerning the relative heating powers of different parts of the sun's surface also, something definite can now be said. When an image of the sun is projected on a disc and accurately examined, it is found that the heat at the centre is almost twice as great as at the borders—a fact very intelligible if we can imagine the sun engirt by an extensive atmosphere; the rays of heat from the edges having to penetrate obliquely through that atmosphere, must in great part be obstructed. But there is besides a variation in heat according to the circles of declination. The solar equatorial regions are certainly hotter than the polar, and the determinations of Secchi would seem to establish that this law holds very much as the same thing takes place on the earth, *i. e.*, the heat diminishes according to a gradation, as the circle of declination is distant from the equator. This latter fact is of greatest importance—rendering it certain that something essentially akin to the systems of winds prevailing within the atmospheres of the earth and other planets, must also hold within the gaseous envelopes of the sun.—But we must proceed to minuter details, and arrange our account of them in logical order.

(1.) *Constitution of the Sun.*—The phenomena that have led to some knowledge of the structure of this vast orb, are those curious spots that appear and disappear so frequently on its surface, and which by their progressive motion across its disc revealed to their first discoverer, Galileo, the remarkable fact, that the sun rotates on its axis like the planets, carrying the spots along with it. The light, however, which these cast on the nature of the substance of this orb, was not realized until the times of Dr. Alexander Wilson and Sir William Herschel. The former astronomer, earliest beyond a doubt exhibited the cardinal fact of this Inquiry. Some remarkable features that invariably characterize a solar spot, had attracted his attention; and, *accident* as he modestly said, enabled him through them to reach the whole significance of the phenomenon. These features are as follows:—When the spot is near the middle of the sun it consists of a very dark centre, around which there is a comparatively dim envelope, called the umbra. Every spot exhibits this appearance. Now, from 22d March, 1769, Wilson had the opportunity of following a very remarkable spot in its course from one border of the sun to the other; and certain facts struck him as certainly fundamental in every theory as to the constitution of that globe. When the spot was in the centre of the disc, the whole encircling umbra, as well as the dark middle portion was distinctly visible: as the spot approached one border, the umbra on the side nearest the observer became gradually more and more fore-shortened, while the umbra on the other side grew broader and broader; and at length, as the spot was disappearing—that is, passing the edge of the limb—the near side of the umbra and the dark central region also vanished, nothing remaining except the opposite umbra, which continued distinct. And when the spot reached the sun's other border, and again approached its centre, there occurred precisely the opposite phenomena, viz., the gradual appearing, first of the dark centre, then of the near umbra, and its progressive widening, until when the object reached the middle of the solar disc, the umbra seemed a uniform girdle as before. Wilson found these changes of aspects repeated in the case of every spot he observed, and they are now known to be universal.—There is an easy means by which the significancy of the changes described may be made palpable. Let the reader place before himself a common terrestrial globe, rotating around its axis, and fancy a hole in one part of it, with shelving sides and a black bottom. When, in the course of its revolution, the visual ray through the opening reaches the centre of the disc, the *whole* would be seen (as the pit could then be looked down into directly)—the dark base as well as the shelving sides. But when placed on either edge of the disc, it is clear that the pit would assume a really different appearance—only the *opposite shelving side* would be visible in such situations: and this was exactly what Wilson always found in the aspect of a spot during the revolution of the sun. It was daring at the time to pronounce an opinion that these hitherto puzzling phenomena are really *holes in the surface of the sun's mass*—pits opened in it, revealing his internal constitution: but every new step of genius is a boldness, sometimes even a rashness! And Wilson saw

that he had discerned how the hitherto mysterious construction of that magnificent orb might at length be made known. Most worthy was this conquest, of the medal struck in honour of it, by the enlightened Danish Kings!—It followed at once, from Wilson's capital achievement, that our magnificent luminary is no chaotic conflagration as had hitherto been supposed by all men: and to the investigation of what it really is, the singular powers of the elder Herschel came quickly in aid of the efforts of his friend. With both of these illustrious men it seemed an unquestionable first impression, that the surface thus broken by chasms, must be an elastic or gaseous fluid; for, notwithstanding the magnitude of the spots—sometimes reaching even 50,000 miles in diameter—they open and close with a rapidity altogether marvellous, often surpassing the rate of a thousand miles a day. Granting then, first, that this light-giving surface is a phosphorescent gas, what is the umbra? —and what the dark internal space? And to these engrossing questions, Herschel, without delay, applied the energies of a mind that ever and anon was flashing into the unknown. It seemed to that penetrating genius—and no other theory will yet resolve the fact—that the sun consists mainly of a dark mass, like the body of the earth and other planetary globes, which is surrounded by two atmospheres of enormous depths—the one nearest him being less luminous, while the loftier stratum consists of those dazzling phosphorescent zephyrs that bestow light and heat on so many surrounding spheres. In this view the following is the real meaning of a solar spot. By some unknown force these atmospheres are disturbed and opened: the phosphorescent zephyrs being flung aside, we descry the shelving edges of the less luminous stratum —which is the *umbra;* and, below, the dark and sombre and more sheltered body of the great globe, as the central spot. These remarkable conclusions by Herschel have never been invalidated, but rather confirmed and added to by subsequent discovery. Beneath the umbral bed, Mr. Dawes has recently descried a thick stratum of comparatively faint luminosity, on which he bestows the name of the *cloudy stratum.* It gives the impression of considerable depth below the shelving sides of the umbra; and it appears not self luminous but rather of a nature to absorb a vast quantity of light and reflect little: its faint illumination is rarely uniform, presenting throughout the appearance of a mottled and cloudy surface. In the heart of this third structure lies the central black opening.—The thickness of the two first of these strata was approximately measured by Herschel. He estimates the thickness at several thousand miles; and it is probable that the inmost bed is thicker still. Conceive then of the sun as a solid mass, engirt at first by a bed of dense clouds, not dissimilar perhaps to the atmosphere of the earth. On the top of this, a floating phosphorescent or luminous mass of amazing depth, the *lower* part of it splendid, but the *upper*, of lustre altogether dazzling, and, from which, streams the flood of light that enlivens all surrounding spheres. Nor has discovery stopped even here. Beyond and above this sphere of light is another atmosphere, seemingly transparent, stretching away into space a distance that cannot yet be computed. This last or upper atmosphere cannot be seen in ordinary circumstances because of the overwhelming powers of the solar rays; but when the sun is totally eclipsed, its presence bursts upon the astonished beholder in the form of a *glory* surrounding the dark disc of the moon. And in the lower regions of that halo, portions of the light-giving mass are found floating, giving rise to those strange phenomena that on the occurrence of the two last total solar eclipses, so deeply interested all observers.—Such the leading phenomena that have thrown the earliest and faint though assured light on the physical structure of our vast central Luminary.

(2.) *Special Consideration of the Solar Spots.*—There appears no rest whatever in the atmosphere of the sun. Over all its surface waves of light seem to dart incessantly, assuming the most varied aspects. The bright part, as Herschel early discerned, is full of inequalities; showing certain parts more brilliant than the rest—sometimes round, sometimes elongated—as if they were mountain billows (a conception recently confirmed by the keen sight of Mr. Dawes), of various forms in that light ocean. Then there are large darker spaces, extending over immense tracts, but showing no dark centre—these are Herschel's *shallows.* After these must rank the true spots, or caverns, often surrounded by a ridge of intense light, like the wall of a lunar crater. When such objects are examined as a *class*, we discern several circumstances too remarkable not to lie among the foundations of all rational theories regarding them. *First*, though they have no relation to any individual point or portion of the sun's surface—the same spot never having been known to re-appear in the same place, or indeed to re-appear at all—they are yet closely related to a large *district*, for they are confined to a zone extending 30° on each side of the sun's equator, which may somehow connect them with the phenomenon of the rotation of that Orb. *Secondly*, they are not stable on the sun's surface, but enjoy proper motions apparently of a singular and strikingly symmetrical kind. According to the indications of M. Laugier, they would seem on each side of the equator to approach the nearest pole, and by paths wonderfully corresponding; so that, strange though it seems, even these chasms so tremendous and evanescent, are bound together, *as a system*, by some grand law. *Lastly*, they present in many cases very extraordinary aspects when they are just disappearing. Most frequently,

perhaps, a spot closes in, gradually, although with great rapidity; but often it *splits* before it closes —light ridges darting across the central space with a speed truly astonishing. The one vast chasm is thus resolved into many; and these have been known to separate from each other and dart along in new and disturbed paths, in a manner not unfitly compared to the fragments of a cake of ice which has been thrown with violence upon the smooth surface of a frozen pool. The play of sudden, tremendous, and evanescent forces, connected in some way with the Sun, and made apparent by these surgings and burstings of his atmospheres, has thus become an absolute fact. But whence and what are these forces?—whither shall the inquirer travel in search of indications of their nature? If, unappalled by the majesty of the orb in which they proceed, we propose, under consciousness of the all prevalence of law, to seek out something analogous in our own planet, we shall find that nowhere save among the phenomena of the WINDS. But the analogy is everywhere striking, probably most remarkable of all in respect of the more complex and imposing aspects of the sun. In reference to the *faculae*, or bright parts, which we deem heapings up of the shining atmosphere, as well as to the shallows, which, with the faculae, are interspersed over it, one's attention is invariably drawn to the great barometric changes that take place on the surface of our globe. Until recently these seem to have been attributed wholly to the internal changes of a column of almost unvarying height; and the possibility of irregularities of any degree of permanency on the surface of the atmosphere was not recognized. But in the present state of speculation they are considered consequences of the WINDS, which, while they induce internal changes in the column, must also cause vast surgings throughout the whole mass, and wholly disturb the contour of the surface. The disturbances are of great extent, and endure as long as the conflict of the aerial currents. What we only *infer* here, from the barometer, is thus *seen* in the sun: and the shallows in its highly elastic covering demonstrate the possibility of apparently the most paradoxical of all recent meteorological dicta—the existence of *atmospheric valleys*. If we are right then, the significance of these remarkable phenomena is clear. They indicate the surging of the solar aerial masses—they are, in part, the markings of the solar barometers; and through them, one day, may come to Science, a knowledge of how the winds roll there, and what are their grand periods. But let us hasten to what is far more striking; the *spots* of our *terrestrial* atmosphere. Who, in recent times, has not rejoiced that the destructive hurricanes of the tropics are at length reduced within law—and learnt without profound interest the mysteries of their constitution and course? It is thoroughly established that these are *mighty whirlwinds*, huge rotating cylinders of air, whose base is on the earth, and whose top may penetrate to our highest regions—cylinders revolving with terrific fury, and at the same time slowly advancing in fixed courses, along the surface of the earth. Now, observe the chief phenomenon of a whirlwind. Look at the dimple in a river—a little whirlpool: invariably its centre is hollow. In a small whirlwind too, straws and dust rise up through its centre as if it were a vacuum; and seamen have long known the dreadful meaning of that sudden calm after a storm, during which the barometer sinks at once: they have got into the *heart* of the whirling elements, and on no side is escape! If that hurricane, then, were looked at, or could be seen from above, it would have the aspect of a *hole* or *perforation* in the atmosphere moving slowly along in a certain path to which all on its side of the equator seems, *as in the case of the sun*, to conform in nature. Indeed, the thought is overwhelming, when from these, terrific as they are, we pass to tornadoes, apparently similar in the atmosphere of the sun, by whose inconceivable violence an opening of 50,000 miles in diameter may be made; extending, too, through the whole depth of that atmosphere, probably several thousand miles. And yet, is not the electric spark with which the child disports itself a key to the rending tropic thunder? Is not the power of life which sustains the smallest wild flower, exactly that which infuses strength into the giant pine, and causes it to evolve its mighty branches? But, further still, those tornadoes of ours pass away like the tornadoes of the sun—generally dying out in singleness, although often changing their forms, and sometimes separating first into minor furious hurricanes. A striking example, viz., the storm of December, 1821, has been of all others the most amply discussed—having attracted the attention and engaged the acuteness of M. Dové, one of the most distinguished of German meteorologists. Embracing an immense tract of country, it swept Europe as one single and tremendous whirlwind; but as the advancing circle struck on the peaks of the Spanish mountains and of the maritime Alps, new and smaller storms were developed; which, with the same characters, rushed with devastating fury through all the valleys—smaller in diameter but equal in energy to the original hurricane. And thus that tornado split up, and its fragments dashed away, also like a broken cake of ice, before the peace of our atmosphere was restored! Nor do we rest with mere vague analogies. One prime condition of the phenomena of these hurricanes is common to the earth and the sun. Our luminary, like its humble attendant, has a Torrid Zone; and therefore, those aerial currents which are produced by that distribution of heat and the rotation of the orb. But to the trade winds of the earth, we owe, if not the origin of hurricanes, at least their paths; and corresponding winds in the sun must correspondingly waft the

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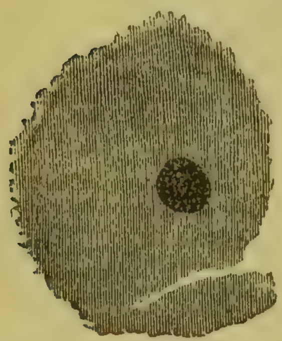
spots towards Northern and Southern Temperate Zones. Farther yet; one of the most promising observations of recent years has removed all doubt concerning the supposed *rotation* of these spots. One of the first contributions by Mr. Dawes, after he had obtained possession of his invaluable solar eye-piece, established this rotation. Below are two sketches of the same spot after an interval of six days. Seldom has



a more prolific observation been published. The dark spot had clearly rotated around the black nucleus; and farther, how tremendous the velocity of rotation! Once again, may we look for the opening of a new era in our knowledge of the sun; for photography is promising its all powerful aid. An apparatus is at present being erected at Kew, under the care of Mr. Ross. Others, it is hoped, will quickly follow it, so that the natural history of the surface of our luminary may soon be written by itself.

(3.) *Source of the Solar Heat: further Speculation concerning the Spots.*—We turn for a moment to another inquiry. The enormous amount of Heat dispersed from the sun has already been estimated, and we have given besides some indications of Law as to its relative intensities in different Solar Zones. Whence comes this Heat; what is its source or fountain? Speculating, as we must do, on the ground of known analogies, or rather on that sole ground of any cosmical speculation—viz., that the phenomena of the earth are the result of universal laws which meet and intermingle in that portion of space we term our globe,—we find only three possible sources of solar Heat. *First*, Heat would radiate from the sun's surface, if the globe were in process of cooling. But if the sun have no internal power from which to supply the deficiency caused by such radiation, it is demonstrable that long ago Heat would have disappeared from his surface, and that surface would have become dark. *Secondly*, Is the Heat evolved by chemical action at the sun's surface? There are two forms of such action—combustion of the sort that demands the presence and proportional supply of an aerial element (oxygen); or such combustion as occurs in the deflagration of gunpowder or of gun-cotton. As to the first, we may dismiss it as impossible; for no atmosphere could supply the enormous quantity of oxygen required to sustain the hourly burning of $\frac{4}{5}$ths of a ton of coals on every square foot of the solar surface. Nor are we better off on resorting to the other manner of combustion. If the sun had been a pure globe of gun-cotton, and been capable, as such, of furnishing the elements of this enormous heat, his mighty mass would have wasted and disappeared in the course of a brief four thousand years. *Thirdly*,—One resource remains; imagined by many physicists—held out by Sir John Herschel among others—but first methodically expounded by Mr. Waterston, and pressed with great force by Professor Thomson of Glasgow. Already this doctrine has been unfolded in the pages of our Cyclopædia. See Heat. Heat results from the stoppage of any mechanical force, in amount equivalent to the mechanical effect of that force. Reflect on a meteor or shooting star entering the earth's atmosphere. The velocity is checked by that atmosphere, and its *vis viva* converted into heat by an inexorable Law,—a heat which at last becomes so intense as to melt and dissipate the meteor into fragments often too small to be noticed when they reach the ground. Multitudes of such meteors exist in those regions of the planetary spaces within which the earth moves; multitudes infinitely greater must crowd the neighbourhood of the sun; nay, that visible annulus—the Zodaical Light—is simply a ring of such meteors, not differing in kind from the annular appendages of Saturn. But, how enormous the velocity of bodies revolving so close on the sun, and therefore how great their *vires vivæ!* The velocity of such meteors must be nearly 400 miles in a second of time; and the heat may therefore be calculated that would supervene on the extinction of their force as they fall on the surface of our Luminary. Should the planet Mercury—forced gradually inwards through effect of the resisting medium—ever reach the solar surface, as much heat would at once be generated, and flash forth into space, as the entire orb dispenses at present in the course of three years! If, then, the Zodiacal Light is

constantly yielding a meteoric rain to the surface of the sun, drawing inwards and inwards like Saturn's rings, how easily is the Heat accounted for! Nor can valid objections be brought from the consideration of a necessary augmentation of the sun's volume on account of such an operation. The entire supply would be realized by a new annual coating of *thirty feet* in depth—an amount which in two thousand years would have added only twenty-three miles to his diameter, and could not therefore have been discerned by the elescope. "The source of energy from which solar heat is derived, is thus—in the words of Professor Thomsön—undoubtedly meteoric. It s not any intrinsic energy in the meteors themselves, either potential or of mutual gravitation, or chemical affinities among their elements, or of actual or relative motions among them. It is altogether dependent on mutual relations between hose bodies and the sun. . . . The store of energy for future sunlight is at present partly lynamical—that of the motions of these bodies ound the sun; and partly potential, that of their gravitation towards the sun. This latter is being gradually spent, half against the resisting medium, and half in causing a continuous increase of the former. Each meteor thus goes on moving aster and faster, and getting nearer and nearer he centre, until sometimes very suddenly it gets o much entangled in the solar atmosphere as to egin to lose velocity. In a few seconds more it s at rest on the sun's surface, and the energy iven up is vibrated in a minute or two across he district where it was gathered during so many ages, ultimately to penetrate as light the emotest regions of space."—But it is not needul to suppose that these meteors reach the solar urface in the solid condition. If they did so, hat luminary would present an equatorial belt f dazzling brightness,—in the midst of Southern nd Northern regions comparatively dark. (Is his an indication of the cause of the bright quatorial belt of Saturn?) Long before they each that sphere of resistance sufficient to impede their velocities, and so to convert them into eat, they have probably been changed into neteoric vapour by the intensity of the solar eat, and have been revolving around him in a neteoric vortex, shaped as a spiral, and terminating in the solar atmosphere. This modification ffects in no wise the efficiency of these meteors o produce heat. Mingling with the sun's atmosphere and retarded by it, the foregoing effects must ensue, every one; but in the case of friction etween gaseous bodies, we can understand the iffusion of the resulting splendour across the hole solar surface, by effect of the resulting tmospheric agitations, and in obedience to the ws of the equilibrium of elastic fluids. From ich friction also, or rather from the meeting of ese aerial floods, eddies may arise, and other regularities; eddies that at times will result in ighty rotatory storms, like our own tropic hurricanes. Fortunate that, if the foregoing speculation be correct, there is thus a sure means of confirming it. Further observations of the sun's light atmosphere—that definite account of its aerial currents with which photography is sure to supply us—a thorough analysis of the critical forms and habitudes of the spots,—these will afford the means of divesting the remarkable inquiry of all that apparent vagueness which still undeniably attaches to it. Nor will another cardinal fact continue without significance. An eleven years period, in the number of the spots, seems already established; at least no other conclusion can be drawn from the laborious researches of Schwabe and Wolf. May not this be essentially connected with the nature or the nodes of the meteoric vortex, turning as a spiral around the sun?

(4.) *The Sun's Physical Influences.*—Besides being the controlling power of the motions of our system, and diffusing light and heat, the sun exerts other influences, varied and important. Indications of his chemical agencies—those on which the new and precious art of photography depends, and which are largely connected with the progress of vegetation—are found in various articles in this Cyclopædia: and under MAGNETISM TERRESTRIAL, we have shown the dependence of the magnetic variations upon his position relative to the Northern and Southern hemispheres of the earth. But his action in this respect is not altogether measurable by that relative position. A strong probability has appeared, that the position of the sun's poles, in reference to our globe, is not an indifferent element of these very complex phenomena: and the subject is rendered yet more mysterious by that happy suggestion by Colonel Sabine, of a connection between the decennial period of magnetic disturbance, discovered by Lamont, and the period or cycle of solar spots determined by Wolf and Schwabe. Nor must we forget that apparent polarity in the comets, so acutely detected and finely illustrated by Bessel.—At present this vast subject is only opening; but the prediction may be adventured on, that many years will not elapse, ere the activity, the zeal, and the intelligence of inquirers shall lead to further and momentous generalizations.

(5.) *The Sun's Motion in Space.*—A few brief words on a large and still more engrossing theme must close the present article. Ascending from contemplation of the sun as an individual orb, towards the idea of his companionship with the hosts of fixed stars, the question involuntarily springs up, is that companionship manifested by common motions?—does the sun move in some vast orbit through space? A mighty question!—already is so far answered in art. STARS. Sir Wm. Herschel was the first to demonstrate the existence of such a motion and to declare its direction. Observing that the fixed stars in one part of our firmament had for ages apparently been drifting

away from each other, while the stars in the opposite region had been proportionally closing in, he inferred that the sun is moving from one of these regions, and towards the other; and he fixed a precise spot in the constellation Hercules, as the point whither our luminary has recently been advancing. The point on which Herschel fixed, in 1783, was in 257° of right ascension, and 25° north declination. Observe how astonishing was this veteran's sagacity! As years rolled on, better and far more numerous determinations of the apparent motions of the fixed stars were obtained. Consideration of the great problem was then resumed; and yet the new results differ but by a very small comparative amount from what Herschel determined—viewing the problem *all in the rough.* The point of direction fixed on by Otto Struve, is this:—

R. A. .... 250° 9½′
N. D. .... 34°·36.

Not the direction merely, but the magnitude of this great solar motion, may now be declared. Our luminary sweeps through space, at the annual rate of *one hundred and fifty millions of miles.* But what is the nature of his path? Is it a mighty curve? If so, where its centre, what its vast radius, and what the duration of the grand SOLAR YEAR? Problems unquestionably resolvable, and for which Stellar Astronomy is slowly accumulating the adequate materials. The completion of this greatest of inquiries, may not indeed be very near at hand; nevertheless, it was assuredly most fitting that to him whose unrivalled sagacity penetrated the secret of the multiple stars, tidings should first come of the *Unity*—the perfection of our majestic system; tidings that among its innumerable hosts not one is solitary or apart, but an essential element of the universal scheme, exchanging sympathies and action with all, and by its motions visibly declaring them!

Suspension Bridges. See BRIDGE.

Sympiesometer. See BAROMETER.

Synthesis. See ANALYSIS and GEOMETRY.

Syringe, Condensing. See AIR PUMP.

Syzygies. The syzygies of a planet, or of the moon, are those points of the orbit where the body is in conjunction or opposition with the sun. The *quadratures* are the intermediate points.

T

Taurus. The second constellation of the zodiac. It is surrounded by Aries, Eridanus, Orion, and Perseus: its position in the heavens is at once obtained by the connection of its bright star Aldebaran, with the belt of Orion.

Telegraph. A telegraph (from τηλε, afar, and γραφω, to write) is an instrument used for the purpose of conveying intelligence to a greater distance than the sound of the voice can reach, and without the employment of a messenger.—From the most ancient times fires were used as distant signals. To these have been added the sounds of the trumpet, drum, gong, or other instrument, the discharge of guns, and the sending up of rockets. The Indians of America transmit information from hill to hill by placing themselves in various attitudes. Whatever signal was employed, its import was understood by previous concert, and, by any of these methods, considerable variety of intelligence might be conveyed; but that depended upon the ingenuity of the parties employed in each particular case, and it is surprising how long a period elapsed before any effort was made to establish an extensive and methodical system.—One of the earliest attempts at this was the introduction into our navy, by order of the Duke of York (afterwards James II.) of signal flags, to direct the manœuvres of the fleet, each flag conveying some peculiar intimation. In the year 1694 Dr. Hooke proposed a telegraph to be worked by suspending, on poles, objects of various shapes, representing the letters of the alphabet, and subsequently M. Amontons exhibited a similar system; but neither of these came into use.—In 1794 the *semaphore* (as it was called) of M. Chappe was introduced into public employment by the French government. It consisted of an upright post supporting a cross bar, in the shape of the letter T. The cross bar, turning on a pivot, could be placed at various inclinations, and had two smaller arms attached to its extremities, also capable of turning upon these, so as to form various angles with them. By independent movements of the different parts it was susceptible of being placed in one hundred distinct attitudes, giving that number of different signals.—As there are many ways in which visible signs may be made, a succession of inventors arose proposing a great variety of methods, more or less useful. In any of these the signals displayed represented either letters, numbers, words, or sentences, as might be thought desirable. The words and sentences were arranged in dictionaries, and found by means of the numbers.—Whatever method was employed, lights were used by night, arranged in different ways, to supply the place of the day apparatus, which was then invisible. It was necessary, of course, that the system of lights exhibited should correspond with the daylight arrangements.—The telescope was usually employed to assist the eye when the stations were far apart; but, even with that aid, all such telegraphs, consisting of objects to be seen at a distance, depended, for their success, on the state of the weather. A hazy atmosphere was fatal to their use, and, however long a fog might continue, all communication

was for the time interrupted.—Other mechanical processes consisted of ringing of bells, or making other signals, by means of wires, or by air or water enclosed in tubes; but, for great distances these were all found to be practically useless.—As soon as it was known that electricity could be conducted along wires, it began to be regarded as a possible means of conveying information, especially when it was ascertained, by various experiments, that the distance to which it might be led was apparently unlimited, provided that perfect insulation of the wire could be obtained. In 1747 Dr. Watson exhibited electrical effects, from discharged jars, at a distance of two miles from the source of excitement. —From that time down to 1823 many ingenious and partially successful attempts were made, both in England and on the continent, to render frictional electricity available for telegraphic purposes; and, in many instances, these contained the embryoes of modern processes. The indications were, in the greater part, used to represent the letters of the alphabet, being, in some cases given by sparks, in others by the deflections of pith balls, and in one of still later date, by perforations made in paper by the sparks. Sometimes a separate wire was employed for each letter, and at other times with a single wire, the particular letter was signified by the number of sparks given in close succession, or, as in Ronald's telegraph in 1816, by having, at the two stations, two circular plates revolving in exact co-incidence, by means of clock-work, so as to exhibit the successive letters of the alphabet simultaneously at both places, and by causing two previously electrified pith balls to converge at the instant the desired letter made its appearance. In one scheme the letters were displayed in a visible form, by means of sparks passing between successive pieces of tin foil properly arranged on panes of glass, the sparks themselves forming the letters, and each letter having its own wire.—The wires were insulated sometimes by being suspended in the air with silk threads, sometimes by being supported on posts of baked wood, and at other times by being enclosed in glass tubes placed in wooden troughs filled with pitch and sunk in the ground. But, with electricity of high intensity, perfect or even good insulation was extremely difficult of attainment. When electricity of low intensity was discovered, this difficulty was, to a great extent, removed; but others took its place, since galvanic electricity failed to deflect pith balls and emitted no sparks without previous contact. Instead of these manifestations of the electric agency, the decomposition of water, or of metallic salts, was substituted, but with unsatisfactory results. In fact no useful practical attainment was made in telegraphing by electricity until Professor Oersted's well-known discovery, in 1820, that the compass needle is deflected from its usual direction by an electric current flowing in a course parallel to it. That important fact formed the basis of many schemes for an electric telegraph, and, among others, for that of Messrs. Cooke and Wheatstone, who united their inventions, and subsequently committed them to the management of a company formed for the purpose, and known by the name of "The Electric Telegraph Company."—Our best method of proceeding will probably be to explain the instruments and processes of that company in the first place, since they have been brought to great perfection and are used throughout most parts of Great Britain. As we proceed, or subsequently, we shall briefly point out the various deviations from that system by other inventors.—It has been explained, under ELECTRICITY, that, with a galvanic battery consisting of one or more cells, in each of which is a pair of metallic plates with acid between them,* if a wire pass externally between the two extreme plates, a current of positive electricity flows along the wire from the less oxidizable metal to that which is more so, or, at all events, a certain influence to which the name of current has, by common consent, been given. By some it is considered that a current of negative electricity also flows along the wire in the opposite direction. Whether there are two currents, or only one, is a point not yet established; but since the results are the same on either supposition, we may confine our attention to the positive current alone or we may have regard to both. In either case that extremity of the battery which terminates in the less oxidizable metal (as copper, silver, or platinum) is called *the positive pole* of the battery, and that which terminates in the more oxidizable metal (usually zinc) is called *the negative pole.*—If, then, we have a battery, P N,

Fig. 1.

Fig. 2.

whose positive and negative poles are designated by the letters P and N, and if these be connected by a wire as in fig. 1, a positive current will flow in the direction marked by the arrow. But if, leaving the body of the wire undisturbed, we cross its extremities so as to connect them with the opposite poles, without permitting them

* It is different in the case of the old voltaic pile, which terminated in a superfluous plate of each metal at the two ends, and therefore emitted positive electricity from the zinc and negative from the copper. This diversity has introduced great confusion of expression. By "a pair of plates" we ought always to understand *the pair in the same cell.*

to touch each other, the positive current will then flow along the wire in the contrary direction to that in which it moved before; that is, in the direction marked by the arrow in fig. 2. —It is necessary, for telegraphic purposes, to have the means of turning the current instantaneously from the one direction into the other. It is easy to imagine a variety of ways in which this may be done. If, for instance, the wire were cut through at the points A and B, the ends of the outer portion terminating at these points in two brass pins, and the ends coming from the battery being left loose, nothing more would be necessary than to bring one of the loose ends into contact with the brass pin A or B, as might be required, and the other with the opposite pin. But the frequent bending of the wires may be avoided, and the process otherwise facilitated thus:—Let a cylinder C C be fixed between the two wires in such a manner that it may be free to turn on its axis, a handle K, being attached for the purpose of turning it. (Figs. 4 and 5 show the same instrument in different points of view.)

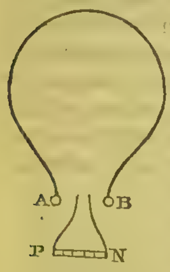


Fig. 3.

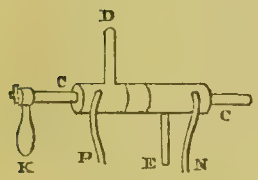


Fig. 4.

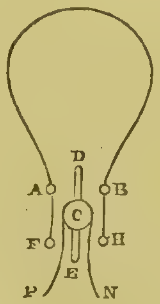


Fig. 5.

Let that cylinder consist of three united cylindrical portions, the two extreme parts being of brass and the middle one of ivory. Let the wire from the pole P rest on one end of that cylinder, pressing firmly upon it with a spring, and let the wire from N rest upon the other end. Let brass knobs D and E, project from these two ends of the cylinder, and on opposite sides of it, in such a manner that, when the handle is turned to the right, the knob D comes into contact with the pin A, and the knob E with B, or with another pin H connected metallically with B; but, when the handle is turned to the left, the knob D is pressed into contact with B, and E with A, or with its associate F. In the first case the positive current is sent along the wire from A to B, and in the second from B to A.—An instrument of this kind is called a *commutator.* Of these there are several varieties, unlike in appearance, but all on the same principle. That just described, with an addition to be pointed out afterwards, forms the mechanical or operative part of the telegraph of Cooke and Wheatstone.—Let N S be a magnetized needle, poised horizontally on a pivot, and pointing in the direction of the magnetic meridian, N being its north pole and S its south. Let a portion of a conducting wire, A B, from the battery be placed over the needle parallel to it. If, then, a positive current be sent along the wire from A to B, the needle will immediately deviate from the meridian, more or less according to the power of the current, the north end turning to the east, and the south end to the west. But if the positive current is transmitted from B to A, the needle turns the opposite way, the north end turning westward and the south end eastward. —If the wire is placed below the needle, the effect produced is the reverse of that which ensues when the wire is above it, whether the current pass along the wire in the one direction or in the other. A current, therefore, passing above the needle, in the one direction, has the same effect as a current passing below in the opposite direction. If, therefore, we bend the wire round the needle as in fig. 7, the one part of it being above the needle and the other below, the current, when transmitted along the wire, will produce the same effect upon the needle, while it passes along the upper portion of the wire, as it does in passing along the lower.

Fig. 6.

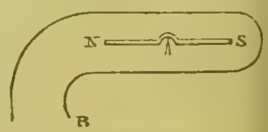


Fig. 7.

The effect of the two portions of the wire upon the needle will therefore be the double of that of a single wire not so bent. If we continue to bend the wire round and round the needle, an equal addition will be made, by every bend, to the force with which the needle is deflected, the length of the whole wire being supposed to remain unaltered. A wire so coiled is called a *multiplier.* By means of it, a current which singly is too weak to deflect a needle perceptibly, may have its force increased to almost any extent. If, however, the wire is lengthened for the purpose, and at the same time, made finer in order to permit a sufficient number of folds to be brought together, the effect will not be proportionally increased, and may even be diminished, unless, at the same time, the intensity of the current be also increased, in order to carry it through the longer and thinner wire. To keep the folds from coming into contact with each other, or with external objects, the wire is covered with silk, cotton, or woollen thread. This is also done with all the wires within the rooms in which the telegraph instruments are placed.—If two needles of equal power be fixed upon the same axis, so as to turn simultaneously, and with their poles pointing opposite ways, the two together will have no peculiar tendency to place themselves in the magnetic meridian, but will

remain indifferently in any position in which they may be placed. The pair thus united, constitute what is called an *Astatic Needle.* It may be placed for use either horizontally or vertically. If, then, one of the two needles be placed within the coil, and the other without, as in fig. 8, the current, as it flows along those parts of the wire which lie between the two needles, will tend to turn both in the same direction, the deflecting force being increased by the addition of the second needle.—The needle may be at any distance we please from the battery, if the wire extends from the one to the other, provided that the battery have sufficient intensity and that the wire be so well insulated that the electricity shall not escape during its passage. If the wire is thick, both the passage of the current and the insulation are facilitated. For these reasons, in the telegraphs constructed in East India under the direction of Dr. O'Shaughnessy, the wire is made of great thickness.—It is not necessary that the wire, after passing from the one pole of the battery to the needle, should return the whole way, since it may be turned into the ground after passing the needle, and the wire from the other pole of the battery being also made to terminate in the ground, the current will find its way between the two, through the ground, the earth itself taking the place of the returning wire.—In the instruments of the Electric Telegraph Company, two of these astatic* needles are placed with their axes projecting from a vertical board, the needles also, when at rest, standing vertically, from being made a little heavier at the one end than at the other. The coil and the needle within it are hidden from view behind the board, and the outer needle alone is seen. Two wires are employed, one extending to each of these astatic needles from a battery at a distant station. Upon transmitting a positive or a negative current along one of these wires, the needle connected with it is deflected to the right or to the left, at the will of the operator, and either needle may be made to turn while the other stands still, or both may be deflected at once, either in the same or in opposite directions. They cannot however be deflected in opposite directions unless separate batteries are used for the two wires.—It is necessary that the same current, which deflects the needle at the distant station, should similarly deflect another within sight of the operator—in other words, that the instruments at the two stations should

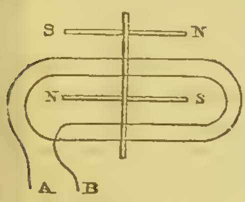


Fig. 8.

* Astatic needles are not so much used now as they once were. Instead of the outer needle is substituted a thin piece of talc or tortoise-shell, shaped in the needle form, while the inner needle is made short and broad, for rapid and steady movement.

work together, constituting what is called a *reciprocating* telegraph. The reciprocation is effected by causing the current, after passing round the needle of the nearer instrument, to proceed along the line to the more distant one. The use of the instrument at hand is to see if the wires have properly conveyed the signal given; for it will seldom happen that the instrument at the distant station has done its duty if the nearer one has not moved. Although the converse of that does not always hold good, yet the peculiar manner in which the needle works at the one station, will, to an experienced eye, indicate faults at the other, or damage or derangement in the conducting wires.—With these additional explanations we may now correct and extend the sketch in fig. 5, by showing the conducting wire W, after being coiled round the needle at K, passing on to the remote station S, and also by removing the return wire, and, instead of it, making a connection between the pins B, H, and the ground G. If, then, as described before, the knobs D and E be brought into contact with A and H, the positive current will be transmitted round the needle K, and then forward to the next station, while the negative current will be sent directly to the ground at G. But if D and E be brought into contact with B and F, the negative current will be transmitted to both instruments and the positive sent into the ground—So much for the *transmission* of a signal; but if a message is to be *received*, the handle (seen in fig. 4) is allowed to hang down, leaving the cylinder C in the position shown in fig. 9. In that position, since neither D nor E is in contact with any one of the four pins, no electricity can pass from the battery P N. But if a current were attempted to be sent from S, it would also be stopped at F. To enable it to pass on to the ground at G, an additional appendage to the instrument is required, viz., a small bar of metal fixed permanently between the pins A and B, touching both, but not joined to them, and out of reach of the knob D, by being placed higher. (It is exhibited in fig. 9 by the dotted lines.) By that cross bar the current from S, after giving its signal on the instruments at both stations, passes from A to B and thence to G. But, since that bar would interfere with the operation of *transmitting* a signal, as described above, by carrying the current from the battery P N at once into the ground, to prevent that, the pins A and B are made elastic, so that upon either of them being pressed by the knob D, its contact with the cross bar is broken, and the result is the same for the time as if that bar did not exist.—Since several stations are usually

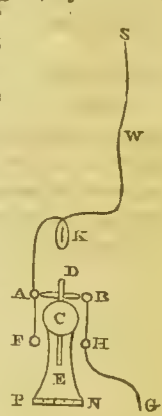


Fig. 9.

upon the same line, the same conducting wire passes from instrument to instrument, and the signals are read at all the stations, unless, as is often done, an additional piece of apparatus is added to each instrument, by means of which, on turning a key, the current can be sent past any instrument without working it, or, after working it, turned into the ground and prevented from proceeding to the stations beyond, making what is called *a short circuit.* Battery power is thus saved and other advantages gained at the same time. The key acts by turning a slip of brass, so as to bring it into contact at once with the entering and the departing wire, or with the latter and the ground wire. The key is turned by the operator at the station, when he has ascertained the source and the destination of the message.—The various modes in which the deflections of the needles take place, are used to indicate sometimes the letters of the alphabet, and at other times numbers, words, and sentences. The annexed representation of the face of one of Cooke and Wheatstone's instruments

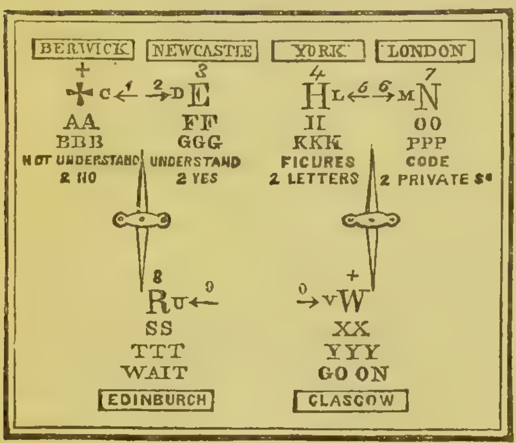


Fig. 10.

will show the signals given for single letters, numbers, and a few words in most frequent use. The letters above the needles are pointed out by the movement of one needle (that is of the top of the needle), towards that side of it on which the letter is placed. When the letter is marked single, as H, N, or E, one deflection of the needle is made; for a letter marked double, as A A, two deflections; and for a triple letter, as P P P, three. The letters C, D, L, M, require a double movement of the adjacent needle, first to the one side then to the other, concluding with that side on which the letter is placed. The letters below the needles are expressed by the simultaneous deflections of both needles, their lower ends turning towards that side on which the letter is placed, that is, both needles turn their lower ends to the left for the letters R, S, T, and to the right for W, X, Y. In this case also two turns to the same side are required for the letters marked double, as S and X, and three for those marked threefold, viz., T and Y, and turns to alternate sides for U and V. Q and Z are made by the two needles inclining opposite ways. — The numerical figures are indicated in the same manner; and the words marked on the board are also pointed out by single or double turns of one or both needles towards them. Thus a single turn of the left hand needle towards the right means "I understand," and two turns of the same needle to the left signifies "No," the figure placed before the word showing the number of turns.—When whole sentences are to be expressed by a few movements, it is done by a system of codes, contained in a book, the sentences being found by means of the letters of the alphabet. Thus the letters E L signify "the Edinburgh and Glasgow Railway," on that code which has reference to shares; but there must previously be two intimations,—first, that the code is about to be used, and, secondly, what code; each of these intimations being made by one or two signals.—Over the left hand needle a cross will be seen. That indicates a stop, which is made at the close of every word; and a second word is not sent until the left hand needle replies "understand." If the reply is "not understand," the word is repeated.—Besides this method, giving signals with two wires and two needles, various others have been devised, both before and since the patent, for the system just described, was obtained. By one plan there was a wire and needle for every letter of the alphabet; by another, five wires and five needles; by a third, three; and by a fourth, only one. The last mentioned, the invention of Mr. Bain, and named *the single needle system*, is still used even on the double needle telegraph, when one of the wires or needles is accidentally disabled. It also points out the letters of the alphabet by the deflections of the needle; but since, with one needle, there are only two variations of movement, a greater number of deflections are necessary than with two needles, and it is therefore more tedious.—In one telegraph a small horse-shoe magnet is used instead of the needle within the coil. In another a strip of gold leaf, forming part of the conducting wire, and a fixed magnet near it, perform the part of the needle and coil. In a third, the needles are of soft iron, rendered temporarily magnetic by the influence of a powerful permanent magnet, their deflections being produced by coils. In a fourth, the signals are made *audible* by the needles striking two bells of different tones, one on the right the other on the left. In a fifth, the letters of the alphabet are marked round a circular dial, and the letters desired brought round and exhibited at an opening. The movement is made by clock-work. The escapement, by which its motion is regulated, is detached by an electro-magnet at every successive transmission of the current, permitting one tooth of the escapement-wheel to pass, and the number of transmissions is made to suit the position of the letter on the

dial.—In addition to all these varieties, there is a whole class of what have been called *printing*, or more properly *recording*, *telegraphs*, that is, telegraphs which leave permanent marks on paper. Of these we may mention, first, one which is the same as that described last in the preceding paragraph, with the addition of a contrivance for making an impression of the letters as successively exhibited. It is truly a printing telegraph; but it is complicated and is never used for practical purposes. The second, that of Steinheil, has small tubes, holding ink, attached to needles deflected by coils, and marking their deflections on paper moving in front of them by wound up mechanism.—Of the others of that class, the two best known are those of Professor Morse and Mr. Bain. Like Steinheil's, they both employ a sheet or strip of paper, moving by clock-work, on which the impressions are made, and the alphabet of each is a system of dots and straight marks continued in a line along the paper. These, on Morse's plan, are made by a steel point pressing on the paper; and in that of Bain, by the electricity making coloured marks on the paper as it passes through it, the paper being previously chemically prepared. The steel point of the former is pressed upon the paper by an electro-magnet, whose attractive power is imparted to it by the electric current, transmitted along the line at proper intervals by means of a key raised and depressed by the finger of the operator. The transmission of the current in the latter is regulated by means of a long ribbon of paper passing over a metallic cylinder at the transmitting station. The ribbon has been previously punched with dots and lines corresponding to the marks intended to be made at the receiving station. The cylinder over which the ribbon passes is in connection with the positive pole of the battery. A slender elastic metallic tracer, in connection with the conducting wire, moves along the perforated paper. As long as it rests upon the paper, no electricity passes to it from the cylinder; but, when it finds an opening in the paper, it passes through it, comes into contact with the cylinder, and, receiving the electric current from it, transmits it along the conducting wire. From the conducting wire the current passes through the prepared paper at the receiving station to another metallic cylinder or disc upon which it is stretched, marking the paper as it passes through, and thence by another wire into the ground. The cylinder at the transmitting station and the cylinder or disc at the receiving station revolve uniformly by means of clock-work, the former carrying the perforated ribbon with it, and the latter the prepared paper. While the latter revolves, the marking style which rests upon it has also a small longitudinal movement given to it at each revolution, so as to bring it upon a new part of the paper.—There is a still more refined instrument of this class, the invention of Mr. Bakewell, which, having a written page of paper presented to it at the transmitting station, throws off a *fac-simile* of the writing at the receiving station. The process is founded upon that of Mr. Bain. The written sheet is spread upon a copper cylinder after being so prepared that the white paper is a conductor of electricity and the ink a non-conductor. While the tracer at the transmitting station is passing over the white paper, that at the receiving station is making a coloured line, and, when the former is passing over a written mark, the latter leaves a blank. The cylinders at both stations continue revolving uniformly, and at the same time advancing longitudinally, a short way at each revolution, by means of screws upon the axes, till the sheet of paper at the receiving station becomes densely covered with fine lines, except over the forms of the letters, which remain white. —The *batteries* used by the Electric Telegraph Company consist of pairs of zinc and copper plates in cells, the space between the plates in each cell being filled with pure sand saturated with diluted sulphuric acid, the sand preventing the transmission of the sulphate of zinc to the surface of the copper. Messrs. Brett and Little attain the same object by allowing fresh liquid to drop into the cells, the deteriorated liquid escaping from the bottom; and Mr. Highton, by using sulphate of magnesia or alumina as the exciting liquid. In some telegraphs, instead of the motive power being derived from a galvanic battery, magnetic electricity is employed, that is, a current generated by making or breaking the contact of magnets with each other, or with soft iron. Frictional electricity is now entirely abandoned.—On any system some *alarum* must be given to call attention. With telegraphs using frictional electricity that was done by the explosion of gas or the combustion of spirit by the spark. With galvanic or magnetic electricity the following is the usual method:—A pair of cylinders of soft iron are surrounded by coils of the conducting wire. When the current is transmitted the cylinders become magnetic and attract a slip of soft iron on the one end of a small lever. The other end, which has previously acted as a detent or catch, to a wound up spiral spring, is, by the motion of the lever, withdrawn, and the spring set free. The spring, in unwinding, turns a toothed wheel, and each tooth gives an impulse to a hammer, which keeps striking a small bell until the current is stopped, and then a fine spring restores the lever to its original place. There are other details connected with the instrument, and various forms of it, which our space does not permit us to describe. There are also other alarums of simpler construction but less perfect. Sometimes a separate wire is used for the bells, but when not so, the receiver of the message, as soon as his attention is gained, turns the current from the bell coil, by a short circuit, into the needle coil.—The conducting

wires between the stations are usually insulated by being suspended on posts, resting, at the points of suspension, upon glass or earthenware insulators. When it is necessary to carry them under ground, as in traversing streets, the method formerly pursued was to cover each wire with twine and pitch, then enclose all the wires together in a leaden pipe, the leaden pipes themselves being further protected against moisture by enclosure in iron pipes. It is now found better to cover the wires with gutta percha. The same covering is usually necessary in passing through tunnels, and always in submarine communication. The suspended wires are commonly of thick galvanized iron: those within doors, and those coated with gutta percha, are of thinner copper wire. For communication by sea, the rope of wire and gutta percha is, for protection, encased in a covering of galvanized iron twisted round it, forming a strong cable. It is one of the astonishing achievements of our day that insulation has been obtained for wires extending many miles under water, considering that the admission of salt water into the bundle of wires so as to form a connection between them at any one point, however minute, would destroy their action for the whole distance.—The extent of submarine communication has become very great and is almost daily increasing. It would be tedious to enumerate the lines of wire which now connect opposite coasts. A line has even been commenced intended to unite Europe with America, and 1,400 miles of it completed, extending from New York, partly by land and partly by water, to Cape Race in Newfoundland. From that to the west coast of Ireland an excellent channel exists for the deposit of a cable; but the distance of 2,000 miles at one stretch is a formidable obstacle. In considering the question of the practicability of completing the line, an unexpected difficulty has presented itself. In working the telegraph from Harwich to the Hague, it was perceived that the signals were given more slowly and less sharply defined than usual. Professor Faraday, having undertaken to investigate the cause, discovered that the gutta percha acted as a Leyden jar, the conducting wire serving for the interior coating, and the water, or the enclosing wire, for the exterior coating. By using Mr. Bain's process, the currents were made to write their own history, and were found to be retarded both in the commencement and in the duration of their effect, so that what ought to have been a dot was converted into a line, faintly marked at each end.

In the following year, 1855, Mr. Whitehouse showed the same results in a higher degree, by using a wire 1,125 miles in length. He found that the current produced by an instantaneous electro-motive force was detained in the wire so as to occupy more than a second and a-half in recording itself.—Professor W. Thomson had computed that the retardation would be directly proportional to the square of the length of wire, and inversely to the area of its transverse section for a given proportion between the area of the copper and the area of the gutta percha section. Thus, since the wire from Newfoundland to Ireland would be nearly double the length of that which Mr. Whitehouse employed, a signal would occupy four times as long, or more than six seconds, supposing the wire and its coating to be of the same sectional area. Mr. Whitehouse argued that if this law were true, the telegraph to America would be practically useless; and at the meeting of the British Association, in 1856, he described experiments from which he concluded that a double length of wire produced little more than a double retardation, instead of fourfold. Thomson, in reply (*Athenæum*, Nov. 1, 1856), pointed out that this conclusion was altogether unsupported by the facts adduced, and that "the law of squares" was untouched by Whitehouse's investigation. It has since received a very decided experimental confirmation in the rates of ordinary Morse signalling which have been found practicable through different lengths of the Atlantic cable before submergence, and by Mr. Jenkyn's recent experiments (communicated to the British Association, Aberdeen, 1859,) on the Red Sea cable—the best experiments yet made on any submarine telegraph, so far as illustration of the mathematical theory is concerned. In trials through portions of the Atlantic cable lying at Keyham during the winter and spring, 1857-8, when lengths of twelve or thirteen hundred miles were exceeded, the rates attained proved to be nearly in the inverse ratios of the squares of the lengths, as Thomson had anticipated in the year 1854; and through 2,500 miles or upwards no greater speed than one word a minute could be reached in the transmission of messages if received and recorded by instruments of the common construction. By the use of a new class of instruments, however, much more rapid and sure signalling was effected. After submergence, messages were transmitted between Newfoundland and Ireland at the rate of from two to two and a-half words per minute, and received with perfect distinctness on Thomson's mirror galvanometer, a wrong or a doubtful letter scarcely ever occurring, even when the cable was in a condition of so defective insulation that the ordinary recording instruments, which had been prepared for the Company, failed to give any intelligible signals whatever There can be no doubt but that a considerably higher speed would have been attained if the cable had not entirely failed before arrangements could be made to take advantage of the indications of the mathematical theory as to the best mode of *sending* as well as of receiving messages through a submarine line of so great length.

Our limited space has compelled us to be very brief on many points on which we have

touched; and to others, though replete with interest, we can only allude. These are the modes of suspending and connecting the wires,—the attainments of skill and experience in telegraph manipulation,—the system of relay batteries,—the interference of atmospheric electricity and of Aurora Borealis,—the effects of lightning upon the instruments and the means of guarding against it,—the various continental telegraphs,—the peculiarities of the Indian and American lines,—the use of telegraphs in conveying exact intimations of time and in determining longitudes,—the conflicting claims of inventors,—and, finally, the refined experiment of Professor Wheatstone for determining the velocity of the electric current. If further information is desired, the reader may consult the volumes of Walker, of Highton, and of Lardner, on the *Electric Telegraph;* the pamphlets of Cooke and of Wheatstone: the documents relating to the invention by Mr. Cooke; and the *Proceedings of the British Association* for 1855 and 1856; also ELECTRICITY, VELOCITY OF.

Several short papers on the mathematical theory of these phenomena and its application to the solution of practical problems, have been contributed by Professor W. Thomson to the *Proceedings of the Royal Society* (May 1855, May 1856, Dec. 1856), the *Philosophical Magazine* (vol. July to Dec. 1855), the *British Association Report* (Glasgow, 1855), and the *Athenæum* (Nov. 1, 1856). In the first of these the equations of electric conduction in a submerged wire were investigated, and the integrals, adapted to the expression of the most marked features of the phenomena which had attracted attention to the subject, were given. In the second (communicated to the *Philosophical Magazine*, June, 1854, and published about a year later) the electrostatic capacity of any portion of a submerged wire was investigated. In the third (*Proceedings of the Royal Society*, May, 1856) the equations of electric conduction through any number of wires insulated from one another in one mass of gutta percha, under a common metallic sheath, were investigated, and the proper modes of integration for the solution of practical problems were indicated. Among other conclusions, one of practical value was pointed out from this investigation—that, contrary to the expectations of some of the most eminent practical men, as exhibited in patented projects, and supposed to be verified by elaborate experiments, no diminution of inductive embarrassment could be obtained by the use of a complete metallic circuit of two wires separately insulated, beside one another in one mass of gutta percha;* a conclusion which is now generally admitted. In a letter to the *Athenæum* (No. for November 1, 1856), replying to Mr. Whitehouse, and in subsequent communications to the Royal Society, various practical conclusions regarding the rates of signalling attainable through air and submarine lines of different lengths are stated, and a new method of working, founded on a theoretical investigation of the operations best adapted for giving a highly condensed single electric pulse at the remote end of a long line of submerged wire, such as that by which it is proposed to establish electric communication between Ireland and Newfoundland, is indicated. In the first communication on the mathematical theory, consisting chiefly of two letters written to Professor Stokes in Oct. and Nov. 1854, and afterwards published (May, 1855,) in the *Proceedings of the Royal Society*, two general laws were investigated. The first of these, which has been called "the Law of Squares," is this:—The time required to charge to a stated proportion of the ultimate electrification producible by a given battery power, or to discharge a stated proportion of a given electrification, is proportional to the squares of the lengths in different cables of the same lateral dimensions. As a particular case, it was stated that the retardation, from the instant of applying an electro-motive force at one end, until a stated proportion of the maximum effect is experienced at the other end, is proportional to the square of the length of the cable.—The second was: That if at one end of an infinitely long submerged wire be applied an electro-motive force regularly changing from positive to negative symmetrically in equal successive intervals of time, electrical waves will be propagated along the wire at a rate which tends to perfect uniformity the greater the distance from the operating end, and with amplitudes rapidly decreasing, according to a law which tends to a geometrical progression at greater and greater distances in arithmetical progression. It was remarked that the law of this phenomenon is identical with that which Fourier, in one of the most admirable of all the beautiful applications he made of his mathematical theory of heat, found as the law of propagation of the summer heat and winter cold to different depths below the surface of the earth.—Mr. Whitehouse's experiments, referred to above, afford many interesting illustrations of particular fea-

* An Atlantic Telegraph had been projected, and the construction of a Mediterranean telegraph was on the point of being commenced, on this plan as a patented invention, in the year 1857, when the attention of the manufacturers was urgently called to the mathematical conclusion stated in the text; and reasons were given for not trusting to the supposed experimental evidence which had led them to the contrary conclusion. The result was, that they gave up their plan of a double wire telegraph, and made their Mediterranean and each subsequent cable with a single insulated conductor. It may be remarked that a metallic circuit of two well insulated wires (whether in one insulating mass of gutta percha, or with water and wet hemp between them, or in two separate cables) has the advantage of being quite free from the disturbance of earth currents, although no such arrangement can diminish *inductive embarrassment.*

tures of this law, and his supposed conclusion against the law of squares, is in reality a partial discovery, by experiment, of the uniform velocity which the mathematical theory had indicated as early as the year 1854.

Telescope. In the article Eye, it is stated that objects are rendered visible by means of the *images* of them formed on the retina or nervous curtain of the organ of vision. The distinctness of the perception depends on the size and clearness of that image. Distant objects are indistinctly seen, chiefly either because of the *smallness* of the image, or by reason of its *deficiency of light.* For example, a house seen at a great distance will only appear as a hazy spot, because, though all its parts must be depicted on the retina, they are so closely approximated to each other, as to render their separate impressions confused. In the case of the more distant stars, again, the image, though formed on the retina, is too feeble in its strength of light to be perceptible.—Any instrument which is intended to render distant objects more distinctly visible, must therefore have the power of enlarging the images of them formed in the eye, and also of increasing the brightness of these images. Such instruments are called telescopes, and their chief functions are the formation of large and bright images. The one property is called their magnifying power, and the other their power of illumination.—The chief parts of a telescope must obviously be those which cause more light to enter the eye, and those which spread that light over a larger surface. The two modes of changing the direction of the rays of light are reflection and refraction, and these accordingly constitute the bases on which all telescopes are constructed. One part of the instrument forms the image, and the other part magnifies it. In Refraction, Lens, &c., will be found descriptions of the manner in which images are formed, and the precautions that are necessary to insure their perfection. The accompanying figures, figs. 1 and 2, illustrate the general arrangement of the most simple form of the refracting telescope. In fig. 1, the cone of light

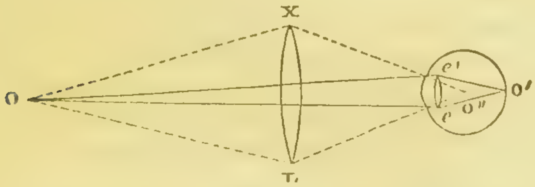


Fig. 1.

e o e′ represents all that could enter the pupil from the object o to form the image o′. But by the interposition of the lens L, the larger cone L o x is enabled to enter, and by refraction at the lens of the eye itself, to form the bright image at o″. It is, however, necessary for distinct vision, that the image should be formed on the retina at o′. This can be accomplished by putting the lens L farther from the eye, so as to form the image at F in fig. 2, and then by interposing the second lens L′ in course of the divergent rays

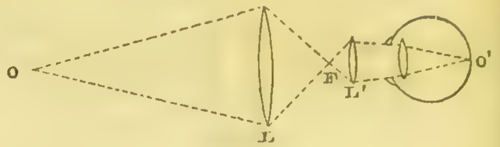


Fig. 2.

after they have passed F, to cause them to enter the eye in a nearly parallel condition, so that the refracting power of the crystalline lens itself should be sufficient to form the image on the retina as at o′. Such an arrangement is named the astronomical telescope, from the circumstance that, as it inverts the apparent position of objects, it is unfit for ordinary purposes, but can be used successfully only for such a case as celestial observation, where inversion produces no inconvenience. The lenses L and L′ are respectively named the object-glass and the eye-glass. The use of the object-glass is to collect a large quantity of light to a perfect focus at F. The eye-glass, then, after the rays have crossed at F, by its refracting power renders them nearly parallel, so as to be in a fit state for the eye to form a distinct image exactly on the surface of the retina. Instead of placing a convex lens L′ behind the focus F, so as to gather the rays together to parallelism, the same purpose can be served by a concave lens *c*, fig. 3, placed in the course of the rays from L before

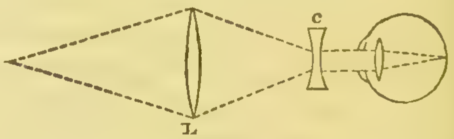


Fig. 3.

they have crossed at the focus. This arrangement was that adopted by Galileo, in the telescope first invented by him, and it is that still used in the common opera glass. It does not invert the image, and has advantages in point of simplicity over other non-inverting instruments. In cases, also, where the main purpose is to search for objects only feebly illuminated, the astronomical form of the instrument is used, as, for example, in the night glass of the sailor, by means of which he searches the horizon for rocks or breakers. In such a case the inversion produces no inconvenience, provided sufficient light is collected to render visible the objects looked at. The terrestrial telescope, which is the one in general use, has a means of reinverting the image, and thus showing it in its true position. This is accomplished by means of two additional converging lenses placed between the object-glass and the eye-glass, as represented in fig. 4, where one inversion occurs, as before, at the focus of the object-glass F, and another inversion at the focus *f*, of the two additional lenses *l l′*, interposed between L and L′. The rays proceeding from the

erect image at f, are then put into a proper state for vision by the eye-glass L′, that is to say, the

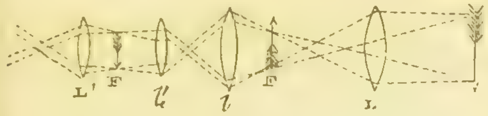


Fig. 4.

rays which emerge from each point are rendered parallel among themselves, so that they can be brought together to a corresponding point on the retina by the converging power of the lens of the eye itself. This is necessary, as will be seen by reference to EYE, where it is shown that distinct vision cannot take place except the pencils, which enter the eye, consist of rays approaching to parallelism, the power of adaptation of the eye being limited.—Such are the forms of the refracting telescope. By the refinements of modern workmanship, and the investigations of optical science, great improvements have been, of recent years, made on the different parts of the instrument, particularly with reference to the achromatism and perfection of figure of its object-glass, as also its size, and the forms of the eye-pieces. The reader is referred to ACHROMATISM for some information on these points. The celebrated practical optician, Frauenhofer of Munich, has erected several refractors of great size and perfection. The Dorpat telescope is of 9 inches clear aperture in the object-glass, and 14 feet focal length. Sir James South possesses an object-glass of 11½ inches diameter; and that of Mr. Cooper of Sligo, has 12½ inches clear aperture, being the largest hitherto completed, with the exception of that recently constructed by Mr. Ross, and erected on Wandsworth Common, near London, which has an aperture of two feet. Great practical difficulties are experienced in the construction of large object-glasses, not the least of which is the inequality in the texture of the glass itself, no process having hitherto been discovered which can insure uniformity of refracting power over so large a mass.—The chief functions of a telescope, as has already been explained, are the collecting of light, and the magnifying of the image. The first is called the illuminating, and the second the magnifying, power. The illuminating power evidently depends on the size of the object-glass; and supposing that all the light which fell on it were so refracted as to enter the eye, the illuminating power would be to that of the eye in the proportion of the surface of the object-glass to the surface of the pupil. Thus, if the pupil of the eye be supposed to have a diameter of ¼ inch, and the object-glass a diameter of 6 inches, or a surface 576 times as great as the pupil, the image formed by the telescope would be 576 times brighter than that formed by the unassisted eye, and the illuminating power would be 576. In general, the illuminating power is said to be in the proportion of the squares of the diameters. Sir W. Herschel, in his astronomical researches, first used telescopes as a means of measuring depth in space. This property of the instrument he named *space-penetrating power*. By reference to LIGHT, it will be seen that the intensity of light diminishes as the square of the distance increases; that is, that at twice the distance only ¼ of the light will fall on any surface, and to render the illumination equal in such a case, the light from four times as much area would require to be collected on the remote surface. This would be done by a lens of four times the surface, or of twice the diameter. If an object as seen from the distance 1, in such an instance, had been barely visible, at the distance 2 it would not be seen till the light had been collected by a lens of four times the surface, and when this was done, the illumination would be exactly equal in the two cases. Conversely, if an object, after being removed to a certain distance, where it was barely visible by the unassisted eye, were again to be removed to such a distance that a lens of four times the surface, or twice the diameter of the pupil, held before the eye, again rendered it barely visible, it might be inferred that the distance in the second case was exactly twice that of the first. By similar reasoning, it may be shown that an object-glass of three times the diameter would penetrate into space three times as far, and one of 100 times the diameter would show objects at a distance 100 times as great as the naked eye, or have 100 times as great a *space-penetrating power*. This determination is founded on the supposition, that the glasses of telescopes transmit to the eye all the light which falls on them. This is never completely realized, even by the most perfect attainable polish and transparency, and therefore a small deduction is to be made in the foregoing calculations. Sir W. Herschel considered that his ten feet telescope had a space-penetrating power of twenty-eight, and that his forty feet instrument could descry an object removed 192 times as far into space, as the utmost reach of human vision. These last-mentioned instruments were reflectors, and their powers are calculated in the same way as the refractors, only that a somewhat greater deduction must be made on account of loss of light in reflection.—The *magnifying power* of a telescope can easily be deduced from optical principles. The simplest case is that of the astronomical telescope, fig. 5.

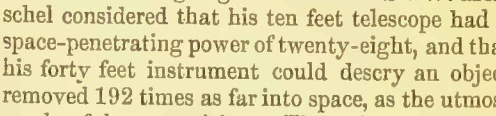


Fig. 5.

The apparent magnitude of any object as seen by the eye, depends on the size of the "visual angle" under which it is seen; that is, of the angle contained between the rays from its two

extremities, as they enter the eye. For instance, the angle A′ E O′ is the visual angle for the image A′ O′ as seen from E, and if E were removed to twice the distance from the image, this visual angle would be reduced to one-half; so that the greater the visual angle, the larger and nearer the image appears. If A O be the object, and A′ O′ its image in the focus of L, then it is obvious, from the equality of the visual angles, that an eye placed at *c* would see the object A O and the image A′ O′ of exactly the same size, and there would be no magnifying power; but if the eye be brought nearer to the image, for instance to the point E, then, the visual angle A′ E O′ being greater than A′ *c* O′, in the same proportion the image would appear larger, and the proportion of these two angles would be the magnifying power. But the two angles bear to each other the proportion of the distances inversely, so the inverse proportion of the distances of the image from the object-glass, and from the eye-glass, will be the magnifying power. But these distances are the *focal distances* (in the case of distant objects) of the lenses; hence the rule, that the *magnifying power* is the ratio of the focal distance of the object-glass to the focal distance of the eye-glass. For example, if the focal distance of the object-glass be 3 feet, and that of the eye-glass ½ inch, then the magnifying power will be 72. It follows from this, that it is possible, indefinitely, to increase the magnifying power, either by increasing the length of the object-glass, or by diminishing the focal length of the eye-glass. Most telescopes are provided with eye-glasses of different focal lengths, which are hence called *powers*.—The slightest consideration will show that, in proportion as a higher power is used, in the same proportion more light will be required, as the image is spread over a larger surface, and that, for very high powers, large object-glasses for collecting a great amount of light are necessary. Hence the explanation of the fact, that while small telescopes can only be used with low powers, say of 50 to 100 diameters, such instruments as Cooper's great refractor may have powers of 1,000 applied to them.—The greatest barrier to the perfecting of the refracting telescope, was the dispersion of light into colour, and the consequent haziness of the image which accompanies refraction. To obviate this, and at the same time to get rid of the spherical aberration, Huyghens employed object-glasses of very long focus, in which, of course, the refraction and consequent dispersion was correspondingly small, and in this way he, to some extent, overcame the difficulty in his "aerial telescopes," of 100 or 150 feet in focal length. These, however, were cumbersome, and Newton, who at this time saw no possibility of constructing achromatic object-glasses, abandoned refraction altogether as a means of primarily gathering the light to form an image. In its place he substituted the reflecting mirror, or speculum, properly figured, as the laws of reflection demand. In SPECULUM will be found details concerning the making and figuring of specula. All that relates to the space-penetrating and the magnifying powers of such instruments, may be learnt from what has already been stated. The principal forms of the reflecting telescope, as at present used, are the three varieties named after their inventors, the Newtonian, the Gregorian, and the Herschelian. In fig. 6, S represents the speculum

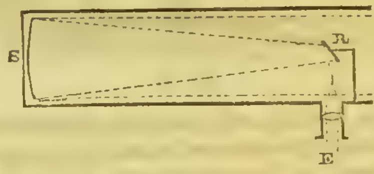


Fig. 6.

in its tube, and R the small plane secondary speculum by which the rays are reflected in such a manner that the image may be formed in an aperture pierced in the side of the tube where the eye-glass is fixed, by which the rays in the individual pencils are rendered parallel, so as to be fitted for the eye placed at E. This is the Newtonian form of the instrument. Instead of the secondary reflector R, placed so as to turn the rays aside to a position where the observer can station himself without interfering with the course of the light in its progress from the object to the principal speculum, a totally reflecting prism is sometimes used with advantage, as by this means there is less loss of light. This is the form used in the great instruments of Lord Rosse, Mr. Lassel, and that erected by the munificence of the Marquis of Breadalbane, at the observatory of Glasgow. The speculum of the latter instrument was originally cast by Mr. Ramage of Aberdeen. It is twenty-two inches in diameter, and was ground by him to a focus so long as to be nearly unmanageable. Some years ago it was purchased for the Glasgow observatory. Since accurate modes of grinding were introduced by Lord Rosse, it has become possible to approximate so closely to a parabolic figure that a much shorter focus could be given to such a size of metal, and yet good definition be obtained. Seeing the desirableness of this, the Marquis of Breadalbane had it re-ground, polished, and fitted up as an equatorial. It now stands in the great dome of the building in a perfect state, fitted with a clock-work movement, and in a condition in every way calculated to do honour to the nobleman who so generously, for the sake of science, undertook its completion. The great telescope of Lord Rosse has a speculum six feet in diameter, and four tons in weight. The tube is of wood hooped with iron, and is fifty-two feet in length. It rests at its lower end on a universal joint, and is suspended by chains, so as easily to be lowered or elevated. A wall is built on each side of it for protection, as is represented in fig.

7. Stairs and galleries are erected for the accommodation of the observer. The whole mechanism,

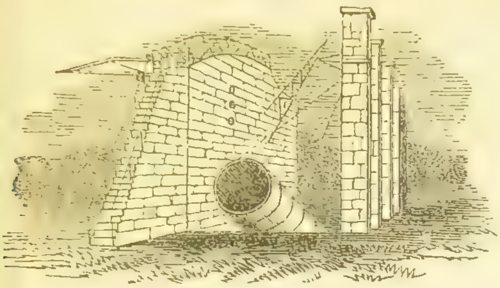


Fig. 7.

from the casting and polishing apparatus to the completion of its smallest balancing wheels, shows an amount of theoretical and practical science which has rarely been equalled in any human invention. It has already, in disclosing the structure of the spiral nebulæ, accomplished marvels in astronomy. For further details the reader is referred to SPECULUM.—The Gregorian form of the Reflecting Telescope is that used, chiefly for smaller instruments, on account of the circumstance that it is more easily directed to the object to be examined. In this form the secondary speculum is concave, and is placed so as to throw the light which comes from the primary reflector through an opening in the latter, behind which the eye-piece is placed. Fig. 8 exhibits in sec-

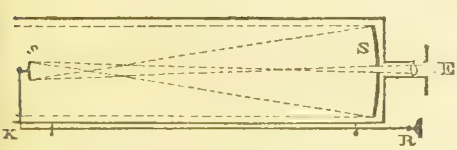


Fig. 8.

tion, the general arrangement of the Gregorian telescope. S is the primary and *s* the secondary speculum, R K is a rod by which the secondary speculum is pushed nearer to or farther from the primary, till the rays of the different pencils, after passing through, are rendered parallel among themselves, which is perceived by the eye placed behind E, when it sees the image distinctly. In both the Newtonian and the Gregorian forms, a portion of the light which would otherwise fall on the principal speculum, is kept back by the secondary placed in front of it. By this loss of light the image is rendered less bright, a circumstance which in the case of objects, such as faint stars and nebulæ, is a decided disadvantage. To obviate this loss of light, and also that which occurs in the second reflection, Sir Wm. Herschel made use of the arrangement known by his name, and sometimes also termed the Le Mairan telescope. Fig. 9 exhibits a section of this form; at S is placed the speculum, and at E the eye-glass. Rays O P, O′ S, from the object pass down the tube to the speculum, and are thence reflected along the lines P F, S F, to the principal focus F, where they cross each other, and in a divergent state proceed to pass through the eye-glass at *e*,

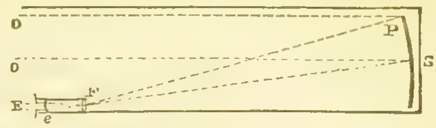


Fig. 9.

by which they are refracted to parallelism, so as to be fit to enter the eye, and by its lens to be brought to a focus on the retina. The great forty feet reflector, constructed by Herschel, was mounted in this way, and, from the quantity of light collected by it, he was enabled to apply a magnifying power of 6,000 in examining the stars.—It is said to be the intention of Lord Rosse to adopt this mode of viewing the image in the great instrument above described, and if his anticipations are realized, its gigantic powers will be yet further increased. Cassegrain's telescope differs from the Gregorian only in the secondary speculum, being convex instead of concave. Mr. Nasmyth of Manchester, in his instrument, fixes the secondary speculum or prism at such a position that the rays reflected from it pass through the axis on which the tube is made to swing. In this axis the eye-piece is fixed, so that the observer does not require to change his position while the telescope is directed to different altitudes. The whole platform on which the instrument and observer are placed, is moved round on a turn table, thus giving greater facilities for sketching, or careful and continued observation, than the common form of mounting.—In all that has been stated in regard to the telescope, it has been supposed that the eye-glass consisted only of a single lens. In many cases, however, it is found preferable, for distinctness and extent of field, to use a combination of two or more lenses forming a compound eye-piece. For a description of the modes of combining these so as to produce the best effects, and for further details, the reader is referred to writers on optics, or works on *Practical Astronomy*.

Temperature. A term usually signifying the comparative amount by which the thermometer is affected by surrounding bodies or circumstances. In the present article we shall speak solely of questions connected with the temperature of the globe.—The temperature of any sphere situated in space as the earth is, must be determined by three elements—viz., the temperature belonging to itself; the temperature proper to the *space* within which it is resting; and the amount of heat it receives from the sun. The causes of the temperature peculiar to different localities on its surface, are found in certain specialities of those localities. We shall survey very briefly these several subjects:—

(1.) *The Proper Temperature of the Globe.*—Whatever may have been the original condition of our globe, or whatever its present internal

constitution, all effect on the temperature of its surface, as arising from that condition, has long ceased. Taking even the extreme supposition—viz., that the earth was once liquid through fusion, it is indubitable that since the consolidation of the outer strata, the heat communicated by solar action, and that lost by radiation, balance each other. Still, the question as to internal temperauure, is an important one; and we cannot solve it, in any direction, without reaching conclusions that must largely influence our views concerning the history or the determining causes of the past states of our globe. The facts are these:—The heating power of the sun reaches only a certain depth below the earth's surface. In our latitudes, that depth is about eighty feet —of course it is more at the equator and much less at the poles. If a solid were described through all these points of ultimate solar effect, its surface would give us an *invariable iso-geothermal* stratum; and penetration below that stratum will begin to indicate the state of the earth's interior. Now, it cannot be doubted, from the temperature of deep mines and the temperature of hot springs, that the earth's heat increases as we descend below this invariable stratum, or pierce into the solid we have imagined. These two sources of intelligence combine, in so far as they have gone, in establishing an increase of 1° of Fahrenheit for a descent of sixty or seventy feet. This subject has been fully discussed by Cordier Bischoff, and Kupffer, and we owe a very interesting memoir on hot springs to Hallmann. Kupffer especially has established the course of the iso-geothermal surfaces over a considerable extent; and he has found that they vary with the longitude as well as with latitude, being subject to very anomalous bendings.—But the point of greatest apparent interest connected with this question is the light it may be supposed to throw on the interior of the globe. If the foregoing rate of increase of temperature should continue uniform, we would reach a temperature at the depth of fifty miles sufficient to fuse every known material element of the earth's mass; so that the sphere on which we live would really be a molten sphere covered by a very thin solid crust. But under PRECESSION, we have seen the utter incompatibility of this, with the rigorous and irreversible demands of the law of gravitation. There are two sets of considerations at present under scrutiny, by either of which the contradiction may be resolved. Mr. Hopkins, who suggested the difficulty as to precession, has pointed out, that we do not know *the point of fusion under great pressures.* And in connection with Mr. Joule, and our eminent engineer Mr. Fairbairn, he has been conducting experiments at Manchester, calculated to determine this point. These experiments have already established *an increase in the temperature of fusion proportional to the pressure to which the fused mass is subjected.* This inquiry has not proceeded far enough to sustain definite universal conclusions; but we should venture to doubt whether it will enable us to bestow on our globe a theoretical solid crust adequate to the exigencies of precession. The second solution of the difficulty is that by Poisson. This eminent geometer thinks that a gradual cooling does not necessitate the result of a fluid or red hot nucleus. And he disputes the inference from the temperature of pits and springs, that there must be a regular increase of temperature the deeper we descend. In Poisson's view, the *temperature of space* has played a most important part in the previous history of our planet. Swept onwards as a dependent on the sun, in virtue of the great motion of translation of that orb, it is probable that we must pass through regions of space of very various proper temperatures. If we have come from a hot region into the midst of one comparatively cold, the phenomena of mines and springs might be readily explained, irrespective of those larger conclusions regarding the primal state of our planet.—We leave the interesting subject as in the meantime far from freed of doubt; —one thing, however, seems very clear,—modern theoretical geology assumes a great deal too much, in referring the comparatively slight unevenness of the earth's contour to agitations of its internal, and, under any tenable hypothesis, very remote central mass.

(2.) *Temperature as Dependent on the Sun.*—The effect of the direct radiation of the sun was first determined experimentally by M. Pouillet, by aid of his *Pyrheliometer.* The atmosphere being almost entirely diathermanous to radiant heat there is no great difficulty in determining this important element. It is thus given by Pouillet: —"If the total quantity of heat received by the earth from the sun were uniformly distributed over all portions of the globe, and employed, without loss, to melt ice, it would melt every year a stratum or envelope of ice, around our whole globe, of a thickness somewhat upwards of 100 feet. This is the simplest expression of the total quantity of the heat that the earth annually receives from the sun." But, as Mr. Hopkins has recently shown, the whole effect of solar heat at any proposed place is nearly double that due to the immediate and direct effect of solar radiation. The atmosphere, although diathermanous, is heated by the earth through effect of *conduction, connection, and radiation, to small distances;* and, being thus heated, it reacts on terrestrial temperature, influencing every part of the globe, according to certain laws, in the manner above stated. Mr. Hopkins has entered on several ingenious speculations as to the influence of atmospheres of different heights. It appears that the temperature of any planet might be so largely modified in this way, that no absolute conclusion can be drawn from the mere position of our companion orbs, as to the climates that pre-

ail over their surfaces. The memoir to which ·e refer was read very recently to the Cambridge 'hilosophical Society.

(3.) *Temperature of Space.*—Abstraction made f the calculated effects of the two foregoing rimal causes of terrestrial temperature, there emains the temperature of the spaces amid which the Earth now performs its motions. This emperature may arise from stellar radiation, from ie proper heat of some medium filling the intertellar spaces, or from agencies explicable by the iodern or dynamical theory of heat. Under § 1), of this article, reference has been made to ie important place given by Poisson, to the ariation of one of these agencies, as affecting ie history of our globe; and his views ought) be accepted as indicating a *vera causa*. The xtent of the efficacy of such a cause, remains iatter for conjecture; but we are well aware iat the influence of stellar radiation must be ery variable, on account of the unequal density ith which the stars are distributed through pace. Who shall estimate how great that radition might be, were the earth and the system) which it belongs immersed in one of the dense gglomerations of the *Milky Way*?—As to the roper temperature of the spaces at present around s, our determinations greatly differ. Mr. Hopins has recently adopted —38°·5 Centigrade; 'ourier estimated it at —50° C.; while, according) Pouillet, it is so low as —142° C. It cannot e doubted that Captain Back found the theriometer at Fort Reliance down at —56°·7, and olds of —60°, —66°, and —70°, have been eached in Siberia. The accurate determination annot be said to have yet been attained; but the stimate by Mr. Hopkins seems the preferable one.

(4.) *Actual mean Temperature of different 'arts of the Earth's Surface.*—The determination f this temperature by observation, is one grand bject of meteorological research. Under CLIIATE, MEAN, and ISOTHERMALS, the methods nd results have in so far been exposed. But a iw recently discovered by Professor Dové has astly facilitated our discovery of the mean temerature of different localities, for any special seaon of the year. The following is his own exposiion of it:—"The temperature of any particular ionth varies very much in different years; its rue value can therefore only be concluded from bservations during a long series of years, and we ossess such for so few places, that if we were to mit ourselves exclusively to them, the points irough which the isothermals are drawn would e too few in number. It was therefore necesiry to find some means of correcting observaons which extend over only a few years, so iat they might, in some degree, be equivalent to onclusions drawn from a longer period. This 'ould be impossible, if the variations in different ears were local in a very restricted sense, and n inquiry into this point was therefore the rst thing required. The thermic march of the weather during an interval of 115 years, from 1729 to 1848 inclusive, was sought to be determined in four memoirs on the non-periodic variations of temperature on the Earth's surface. This was done by forming tables of contemporaneous series of observations for a considerable number of years, and deducing the variations of the months in single years from the means of the same months drawn from many years. It thence appeared that important variations are never merely local, but that the same character of weather prevails over large portions of the globe; that the anomaly reaches its maximum in one spot, in receding from which it lessens more and more until passing through places where the thermic conditions are in their normal state, an opposite extreme is reached which so compensates the first that the general sum of warmth distributed over the earth at any particular time of year is the same in different years, although the values which make up the sum may be very different. Knowing the prevailing character of the weather in particular places in the different years, we are enabled to deduce from the deviations at a few normal stations, where the observations extend over a long series of years, the quantitative corrections to be applied to the results of observations continued for only a few years. The fourth memoir contains the corrections calculated for nineteen such normal stations—Madras, Palermo, Milan, Geneva, Vienna, Regensburg, Stuttgard, Carlsruhe, Berlin, Copenhagen, Torneo, London, Kinfauns Castle, Zwanenburg, Paris, Salem, Albany, Golthaab, and Rykiavig. These four memoirs also contain the complete data derived from observations at 700 stations, or the *monthly means* during the respective years of observation.—The second necessary correction is that required for eliminating the diurnal variation, and reducing the observations made at particular hours to the mean of the whole twenty-four hours, as it is only at a few stations that observations were made hourly. These latter stations—twenty-nine in number—supply the values required to reduce the observations at any particular hour to the mean of the twenty-four hours, and are given in the memoir entitled, 'On the Diurnal Variations of the Temperature of the Atmosphere.' They are:—Rio Janeiro, Trevanderum, Madras, Bombay, Frankfort Arsenal, Toronto, Rome, Padua, Kremsmünster, Prague, Muhlshausen, Halle, Göttingen, Salzuflen, Brussels, Plymouth, Leith, Greenwich, Apenrade, Christiania, Drontheim, Helsingfors, Petersburg, Catharinenburg, Barnaoul, Nertschinsk, Matoschkin, Schar, the Kariangate, and Boothia Felix.—It still remained to deduce from single years the monthly means for periods of many years. The temperature tables in the volume of the *Transactions of the Berlin Academy for* 1847, contain the means for the months, for the seasons, and for the year, as they follow directly from the observations, without correction

for diurnal variation. These tables have also been calculated in Fahrenheit's scale, and are published in the Report of the seventeenth meeting of the British Association, held at Oxford, 1847. Since the publication of this work several stations have been added, and for other stations the means have been determined from longer series of observations.—Lastly, it remained to fill up the wide intervals between the stations by the help of points in the intervening seas. This last work consumed a great quantity of time, as, generally speaking, the single observations are not even put together in daily means; and besides the mean place of the ship must be determined for each occasion from the continually varying latitudes and longitudes. It is only in Beechey's *Narrative of a Voyage to the Pacific and Behring's Straits* (which is a true model in point of reduction), that this has been done. Besides the above work, I have made use of the following, viz., *The United States Exploring Expedition* (in which, however, as the distinct meteorological appendix has not yet been published, I could only employ the notices found in the text); Captain James Ross's *Voyage of Discovery and Research in the Southern and Antarctic Regions;* and Dumont d'Urville's *Voyage au Pole Sud et dans l'Oceanie sur l'Astrolake et la Zélée.* These three works, with Clerk's *Daily Abstract of Meteorological Observations made on board the Pagoda*, and King and Fitzroy's *Narrative of the Surveying Voyages of the Adventure and Beagle*, describing their examination of the southern shores of South America, have rendered it possible to deduce the isothermals of the Southern Hemisphere much more extensively than could have been done a short time ago, and thus to obtain an approximate determination of the temperature of the southern half of the globe. . . . Although, by reason of the smaller variation of the temperature on the surface of the ocean, observations even of very short periods give approximate results, yet the mass of materials which one fancies, at first sight, one has at command, contracts exceedingly in its dimensions on a nearer inspection: for, as on land, stations of observation are unnecessarily crowded in some places and altogether wanting in others, so also at sea, there are much frequented routes, and, on the other hand, extensive tracts which are hardly ever traversed. The influence of season encounters the inquirer the more frequently in sea observations, because the prevailing winds of different parts of the year determine the most favourable season of navigation for particular routes. Against this inconvenience we may place the advantage which sea observations possess of getting rid of the often very uncertain correction for the influence of elevation."—The actual mean temperature thus determined, M. Dové has published a number of graphic representations of it of the most valuable description. These have been described under ISOTHERMALS; but he has further added a series of charts of what he terms ISABNORMALS, which are likewise of greatest importance. Were the Earth's surface uniform, each place on that surface would have a mean temperature depending solely on its *Latitude*, and lines thus indicated are termed by Dové the *Thermic Normals:* curves drawn through points of equal deviation from the thermic normals are the *Thermic Isabnormals.* A chart of this kind is given in Johnston's new edition of the *Physical Atlas.* The deviation depends on many causes already indicated under CLIMATE and ISOTHERMALS. Only a few remarks, therefore, are requisite here; and these we shall arrange under two heads:—

a. Thermic Divergences arising from Elevation of the Place above the level of the Sea.—The physical character of our atmosphere necessitating a decrease of density as we ascend, involves also a diminution of temperature. Taking the mean of observations made on the flanks of mountains by Saussure, Kaemtz, Bravais, Schow, Martins, Humboldt, Boussingault, and the recent French Commission of the North, the diminution is one degree of Fahrenheit for every 303 feet of ascent. The decrement on mountain sides, however, cannot be taken as the rate of decrement in the free air. This rate has been variously estimated. Atkinson in his elaborate memoir, in vol. ii. *Astronomical Society's Transactions*, estimates it at one degree of Fahrenheit for every 250 feet; but Mr. Welsh, who has recently made four balloon ascents with Mr. Green, nearly the height of 22,940 feet, estimates the rate at one degree for every 248 feet—thus very nearly agreeing with Saussure.—Generally speaking, the *limit of perpetual snow*, although varying because of many circumstances whose separate effects cannot always be accurately assigned, follows the law indicated by the following numbers:—

At the equator, that height is	15,700 feet.
At lat. 20°	15,000 ,,
,, lat. 45°	8,400 ,,
,, lat. 65°	4,900 ,,

Many circumstances besides latitude, however, affect this quantity. For instance, on the northern slopes of the Himalayas, the limit of perpetual snow is higher than the equator, being upwards of 17,000 feet, while at the southern flank it is 3,600 feet lower. Anomalies of this sort are far from uncommon.—An elaborate attempt to give a theory of the decrease of temperature as depending on elevation, is made by Professor Challis in the *Transactions of the Cambridge Philosophical Society*, vol. vi., to which we refer the reader.

b. Other Causes of Thermic Divergences.—These are mainly the extent, form, and exposure of continents, the neighbourhood to oceanic currents, and the character of the prevailing winds. The general effect of such circumstances has already been fully noticed. It is sufficient to destroy utterly the effect of latitude in many

cases; for instance, when the centigrade thermometer is at —39° at Jakoutsk, it may not stand lower than —19° in Nova Zembla, although the latter is farther north by 10°. Sometimes the concurrence of extremely favourable circumstances, creates a climate fit for an Eden, or the Hesperides of any mythology. Bougainville compares Otaheite to the Elysian fields; nor does our brave Cook disagree with him. Within Europe itself, or in its immediate neighbourhood, there are spots on which Nature seems to have showered most prodigally her choicest gifts —witness Italy, Sicily, Greece, the Azores, Madeira, the Canaries, the coasts and islands of Asia Minor:—

> O ubi campi,
> Spercheosque, et virginibus bacchata Lacænis
> Taygeta! O qui me gelidis in vallibus Hæmi
> Sistat, et ingenti ramorum protegat umbra.

"To see Naples, and then to die!" So exclaims the enraptured traveller when first he experiences the skies and climate of ancient Parthenope.—Alas! that climate and the noblest gifts of nature cannot bring civilization!

(5.) Respecting the possible, if not probable, *temperature of our companion globes*, we again refer to the memoir by Mr. Hopkins, already quoted.

Tenacity. A property of material bodies by which their parts resist efforts to tear them asunder. It is a result of the unknown corpuscular forces which act between the molecules of a body at insensible distances. It differs with the material; and for the same material under differences of circumstance, as of temperature, &c. The tenacity of materials is a chief element of their strength, and great pains have been taken by engineers to procure numbers which should accurately measure it. The tenacity of wood is much greater (apparently about ten times) along the grain than transversely. With metals, the processes of forging and wire-drawing greatly increase the tenacity. Mixed metals have, in general, greater tenacity than simple metals.

Tension. The name given to the force by which a bar or string is pulled when forming part of any system, in equilibrium or in motion. Thus when a string supports a weight, the tension of the string is the weight suspended by it. Every point of the string, since it is in equilibrium, may be considered as the point of application of two forces, each equal to the weight, but acting in opposite directions.

Thalia. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Themis. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Thermanism. A ray of heat is said to be thermanized when it assumes peculiar properties, on transmission through media that permit its transit. See DIATHERMANISM.

Thermo-Electricity. Electrical excitement has been found to originate from purely thermal action in a variety of ways, all of which fall under one or other of two very distinct classes of phenomena.

I. Pyro-electricity, or the thermo-electricity of non-conducting crystals.—II. Electro-motive forces occasioned by differences of temperature in metallic conductors.

I. PYRO-ELECTRICITY.—The name *Pyro-electricity* has been given to this division of the subject by some writers, but for no better reason than to leave the second division in undisturbed enjoyment of the name thermo-electricity.—Theophrastus and Pliny both mention the lyncurium, as a stone which, like amber, manifests the property of attracting light bodies; but neither appears to have been aware that the stone can acquire the property by heat alone, which is only developed in amber by friction; and Pliny ascribes to the *power of heat*, the attractive force observed in each case. About the end of the seventeenth century, Dutch merchants brought the tourmaline from Ceylon, where it was known to the natives as first attracting, and then repelling away, hot ashes when placed among them. From that time to this, the pyro-electric property of the tourmaline, and of other natural crystals in which the same quality has been found, has been regarded with peculiar interest by all who have been interested in electrical science. The most celebrated electricians of last century contributed important elements towards an understanding of the true nature of the phenomenon. In 1757, Æpinus showed that the two electricities are always to be found on opposite sides or ends of the same piece of tourmaline excited by heat. Canton and Bergmann showed that a tourmaline when kept hot loses the electro-polarity which it acquires in being heated, and acquires the reverse polarity when it is cooled towards its primitive temperature. Canton found that when a long crystal of tourmaline, apparently unelectric, is broken across in any part, the separated surfaces exhibit contrary electrifications. The pyro-electric property was found by various observers in many other crystals besides tourmaline. Thus we have the following list of electric crystals:—

Boracite.	Mesotype.	Calamine.
Topaz.	Tourmaline.	Sphene.
Axinite.	Prehnite.	

The electric property manifested by all these crystals consists in the appearance of vitreous electricity on one side or end, and the appearance of resinous electricity on the other side or end, when the substance is raised in temperature. The substance which manifests it has therefore *a dipolar quality*. Haüy *proved that electric crystals exhibit different sets of crystalline faces on the two sides on which the contrary electric effects are observed.* This dipolar crystalline asymmetry, along with the dipolar electric quality with which it is associated, constitutes the only dipolar quality which has been found to be in-

trinsically possessed by any substance, either of natural crystals or of artificially prepared bodies. It has not yet been found whether magnetic crystals (magnetic iron ore for instance) are essentially dipolar in their magnetic quality, or simply possess this quality in virtue of an inductive effect they may have experienced, and, like steel, may be capable of losing it or of having it reversed in them.

The most probable account that can be given of the pyro-electric quality of dipolar crystals is, that these bodies intrinsically possess the same kind of *bodily electro-polarization* which Faraday in his *Experimental Researches*, has clearly proved to be temporarily produced by electric force in solid and liquid non-conductors; and that they possess this property to different degrees at different temperatures. The inductive action exercised by this electro-polar state of the substance, on the matter touching the body all round, induces a superficial electrification which perfectly balances its electric force on all points in the external matter: but when the crystal is broken in two across its electric axis, the two parts exhibit as wholes contrary electrifications, not only by the free electro-polarities on the fractured surfaces, discovered by Canton, but by the induced electrification on the old surface, belonging to the old state of electric equilibrium, and gradually lost by slow conduction, while a new superficial distribution of electricity on each fragment is acquired which ultimately masks all external symptoms of electric excitement. When the temperature of the substance is changed its electro-polarization changes simultaneously, while the masking superficial electrification follows the change only by slow degrees: more or less slow according to the greater or less resistance offered to electric conduction in the substance or along its surface.

II. Thermo-Electricity of Metallic Conductors.

(1.) *Direct Experimental Results as to Electric Effects of Thermal Inequalities.*—When the junctions of different metals in a heterogeneous metallic circuit, or when different quarters of a circuit or block composed of one crystalline metal, are maintained at different temperatures, a delicately suspended needle placed in the neighbourhood is generally found to be deflected, as if it were placed in the neighbourhood of a magnet. This phenomenon, which has sometimes been called *thermo-magnetism*, was discovered by Seebeck in 1822, and was by him correctly explained, in accordance with Oersted's then very recent discovery of electro-magnetic force, by attributing it to continuous electric motion sustained in the metallic substance by the agency of heat. Yelin and Sturgeon examined the "thermo-electric" currents in variously shaped specimens of simple metals, and were led to attribute them to crystalline structure. Becquerel found electric currents indicated by a delicate galvanometer when the circuit of the galvanometer coil was completed through an arc of wire of one metal with a knot tied upon it when heat is applied on one side or the other of the knot. Magnus (Poggendorf, *Annalen*, Aug. 1851) has shown that the thermo-electric action in this case is due to a *hardening* or other change of temper produced in the wire by the stress applied to it in making the knot, and not to unequal thermal conduction towards and from the knot; and he has investigated the effects of hardening wires by wire-drawing, in giving them, for longitudinal conduction, the thermo-electric qualities of altered substance. Svanberg has shown that a crystal of one metal (of bismuth for instance, or of antimony), when placed in a circuit with its crystalline axis in the line of conduction, and again with its crystalline axis perpendicular to the line of conduction, exhibits the difference of thermo-electric quality found between different metals. Professor W. Thomson has found that metals when subjected to stress (that is dilating, compressing, or distorting force), experience alterations in their thermo-electric qualities which they lose when the stress is removed; that when the stress is not a uniform dilatation or condensation in all directions, the alteration of thermo-electric quality is different for different directions of the line of conduction; and that with a simple stress either of dilatation or of condensation parallel to one line, this thermo-electric alteration is in general of contrary characters, when the line of conduction is parallel, and when it is perpendicular to the direction of stress. He has also shown that the alterations of thermo-electric quality discovered by Magnus are contrary to those which would be found by putting the same wires into a circuit in such a manner as to have the electricity conducted through them perpendicular to their lengths; and he has investigated allied cycles of thermo-electric effects produced by various modes of hardening or tempering metallic pieces of various shapes. Thus, he finds that the thermo-electric effect of hardening a metallic wire by transverse hammering, is the same as that of hardening it by mere longitudinal tension, and is contrary to the thermo-electric effect of hardening by compressing longitudinally a little pillar, or by dilating laterally a slip, of the same metal, when the length of the wire, of the pillar, and of the slip, is in each case taken as the line of conduction. He has found also that magnetization either during or subsisting after the application of the active magnetizing force, alters the thermo-electric quality of unmagnetized iron, steel, and nickel; that the alteration is, at least for iron, of a contrary character for conduction along and for conduction across the lines of magnetic force or of magnetization; and that the thermo-electric alteration in the one case tested for nickel—that is for conduction along the lines of magnetic force—is contrary in character to the alteration experienced in the corresponding circumstances in iron.

THERMO-ELECTRIC TABLE.

−20° Cent.	0° Cent.	40° Cent.	100° Cent.	200° Cent.	300° Cent.	High Temperatures. (Limit unknown.)	Temperatures from 0° to 100° Cent.
Antimony. Iron. Platinum (1).* Cadmium. Silver. Gold. Zinc. Platinum (2).* Copper. Lead. Tin. Mercury containing zinc. Platinum (3).* Brass. Pure mercury. Palladium. Nickel. Bismuth.	Antimony. Iron. Cadmium. Gold. Silver. Platinum (1). Zinc. Copper. Platinum (2). Lead. Tin. Brass. Platinum (3). Mercury containing zinc. Pure mercury. Palladium. Nickel. Bismuth.	Antimony. Iron. Cadmium. Gold. Silver. Zinc. Platinum (1). Copper. Lead. Brass. Platinum (2). Tin. Aluminium Platinum (3). Mercury. Palladium. Nickel. Bismuth.	Antimony. Iron. Cadmium. Zinc. Gold. Silver. Copper. Brass. Platinum (1). Lead. Tin. Platinum (2). Platinum (3). Mercury. Palladium. Nickel. Bismuth.	Antimony. Cadmium. Iron. Zinc. Gold. Silver. Copper. Brass. Lead. Tin. Platinum (1). Platinum (2). Platinum (3). Mercury. Palladium. Nickel. Bismuth.	Antimony. Cadmium. Zinc. Gold. Silver. Copper. Iron. Brass. Lead. Tin. Platinum (1). Platinum (2). Platinum (3). Mercury. Palladium. Nickel. Bismuth.	Brass. Copper. Silver. Iron. Platinum. Mercury. Nickel. Palladium.	Antimony. Longitudinally magnetized iron. Iron under longitudinal traction. Iron under transverse compression. Iron hardened by longitudinal compression. Iron hardened by transverse traction. Soft unmagnetized iron. Transversely magnetized iron. Iron under longitudinal compression. Iron under transverse traction. Iron hardened by longitudinal traction. Iron hardened by transverse compression (hammering). Iron slips or wires hardened by sudden cooling. Longitudinally magnetized steel. Soft unmagnetized steel. Cadmium (temperatures above 57°). Steel hardened by sudden cooling (neutral to cadmium at 57° Cent.) Cadmium (temperatures below 57°). Copper hardened by longitudinal traction or by lateral compression. Soft copper. Copper under longitudinal traction. Cast iron. Unmagnetized Nickel. Longitudinally magnetized Nickel.

* Three different specimens of platinum wire, probably alloyed to different degrees with palladium or other metals.

Thermo-electric Inversions were discovered by Cumming as early as 1823, but have attracted little attention until quite recently; and phenomena of this class described by Becquerel, have been either denied or explained away erroneously by various writers. The original observations, in reality worthy of perfect confidence, are readily verified by experiment, and may easily be exhibited in a striking manner by the aid of any ordinary galvanometer adapted for illustrating thermo-electricity. A great number of new cases of thermo-electric inversion, at ordinary temperatures, between ordinary metals, have been found by Prof. W. Thomson, and the temperatures of neutrality between the two metals determined in many of the cases. Instead of a single thermo-electric list such as is usually given to express the thermo-electric relations of different metals, he gives a diagram of lines, showing the orders in which they lie at different temperatures, and the temperatures of neutrality for different pairs. The table on the preceding page is deduced from the most recent edition of that diagram (*Phil. Trans.*, 1856), taken in connection with other experimental results by the same author alluded to above, and with an observation on the thermo-electric quality of cast iron recently communicated to him by Mr. Joule. —The relations expressed by this table are such, that if arcs of any two of the metals be put together so as to make a single metallic circuit, and if either junction be slightly varied from the temperature to which any column belongs, a current will flow through the circuit in the direction from the metal whose name stands lower in that column, across the warmer junction, into the other metal.

(2.) *Thermo-dynamic Relations.*—In a "Mechanical Theory of Thermo-electric Currents," communicated to the Royal Society of Edinburgh in 1851, W. Thomson taking up a hint thrown out by Joule,* that the cooling effect discovered by Peltier as produced by an electric current in flowing from bismuth to antimony across a surface of contact, is to be regarded as exhibiting the source of energy drawn upon in a thermo-electric current, in a circuit of these metals, showed that by using Carnot's principle as modified by Clausius and himself, in connection with Joule's law of the mutual convertibility of heat and mechanical power, the energy of a thermo-electric current may be distinctly traced to certain absorptions and evolutions of heat in different parts of the circuit through which the current passes. According to Peltier's discovery there is essentially an absorption of heat at the warmer junction, and an evolution of heat at the colder junction when, in a closed circle of two metals thermo-electrically different from one another, the two junctions are maintained at slightly different temperatures. If to this thermal agency alone is to be attributed the energy of the current, the quantities of heat absorbed at the warmer junction must exceed that evolved at the cooler junction, by an amount equivalent in heat to the work of any kind whether thermal, chemical, or common mechanical (weights raised), which the current performs. But if this supposition were true, it would result as a consequence, that the electro-motive force in a thermo-electric pair would follow the same law of variation with the difference of temperatures of the two junctions for all pairs of metals,—would be in fact simply proportional to their difference of temperatures on the absolute thermo-dynamic scale for estimating temperature recently proposed by the author in conjunction with Mr. Joule,* and demonstrated (by their experiments on the thermal effects of forcing air through porous plugs) to agree very closely with the ordinary standard of the air thermometer. Now many experimenters have found that the electro-motive force in some thermo-electric pairs increases more and more, and in others increases less and less, with equal additions to the temperature of the hot junction; and the mere fact of thermo-electric inversion occurring between any two metals is a signal violation of any such law as that of a simple proportionality of electro-motive force to difference of temperatures, or of any law whatever of dependence on temperature alone, common to all thermo-electric pairs. Hence it was concluded that the Peltier effect (heating or cooling by a current in crossing the surface of contact between two metals) cannot be the sole origin of the energy of a thermo-electric current. Mr. Thomson was thus led from thermo-electric facts by thermo-dynamic theory, to the discovery of the electric convection of heat, which he has since amply confirmed by direct experiment. The first metals in which he looked for this effect were iron and copper, the fact of the thermo-electric inversion between them discovered by Cumming having afforded ground for the theoretical conclusion that there must be a very sensible *difference* between the convections in those two metals. The conclusion was verified: —in the least expected of all the possible ways (of which three were indicated). The direct experiments actually showed that, in an iron conductor kept hot in its middle and cool at its ends, electricity (the "vitreous" being nominally "positive") flowing from *hot* to *cold* produces a *cooling* effect, and in a copper conductor similarly circumstanced, electricity flowing from *hot* to *cold* produces a *heating* effect. The phenomenon in iron is exactly such as might be expected if resinous electricity were a *fluid*, and vitreous electrification a deficiency of the natural quantum, while the phenomenon in copper is such as would be expected if, with Franklin, we gave vitreous

* On the Calorific Effects of Magneto-Electricity and the Mechanical Value of Heat. *Phil. Mag.*, 1843, vol. July to Dec.

* *Philosophical Transactions*, June 1853 and June 1854.

electricity the positive character and considered resinous electricity as less than nothing! Direct experiments on platinum and brass have given further confirmations of the theory. An examination of the results of experiments on thermo-electric inversions, represented graphically in the diagram alluded to above, taken along with those direct experiments on convection, have led to the conclusions collected in the following table (see Appendix, p. 830), in which the different metals are arranged in order of the *amounts of the electric convection of heat* which they experience, or in the order of the values of "the specific heat of electricity in them."

(3.) *Mathematical Theory* (Part vi. of "Dynamical Theory of Heat," *Trans. Roy. Soc.*, *Ed.*, and *Phil. Mag.*, vol. Jan. to June, 1856.)—In this paper the mechanical theory of thermo-electric currents in linear conductors of non-crystalline substance, first communicated December 15, 1851, is extended to solids of any form and of crystalline substance.—It is first proved, that if a solid be such that bars cut from it in different lirections have different thermo-electric powers elatively to one another, or to other linear onductors, forming part of a circuit, there must, or every bar cut from it, except in certain paricular directions (principal thermo-electric axes), e a new thermo-electric quality, of a kind quite listinct from any hitherto known; giving rise to a reciprocal thermo-dynamic action, which consists of *a difference in temperature maintained y sources of heat and cold, at the sides of he bar causing a current to flow longitudinally, hen the two ends, being at the same temerature, are connected by a uniformly heated onductor; and a current through the bar causing n absorption and evolution of heat at its two sides, hen these are kept at the same temperature.*—The nost general conceivable thermo-electric relations f a crystalline solid, or body possessing, inducively or structurally, different physical properies in different directions, are next examined. t is shown how a metallic structure may be ctually made up of pieces of different non-crysalline metals, which, taken on a large scale, ompared with the dimensions of the heterogeeous elements of which it is composed, will be ound to exhibit the most general type of thermo-electric directional relations indicated by the bstract investigation; and it is inferred that it ould be wrong to limit the general expressions y any particular assumption, even if we only iscover simpler types of thermo-electric relations n natural crystals.

(4.) *Temporary Thermo-electric Effects Consequent upon Rapid Changes of Temperature.*—All e thermo-electric effects referred to above are ermanent, as long as the differences of temperaure in different localities of the conductors are aintained. There is another class of thermoectric effects in metals, of which only a few olated facts are yet known. Thus, for instance, if two ends of a copper wire, one first heated, and the other kept cool, be suddenly put in contact, a current sets and flows for a very short time through the circuit thus formed in the direction from the cold end across the surface of contact to the hot end. The same phenomenon is observed with wires of German silver, pure Silver, and Tin. The reverse is observed with wires of Platinum, Gold, Cadmium, Brass, Silver alloyed with 25 per cent. of copper, and Lead; and with liquid mercury contained in glass tubes.—*Magnus*, *Poggend.*, *Ann.*, *Aug.*, 1851.

(5.) *Application to the Measurement of Temperature.*—By connecting the free ends of a thermo-electric arrangement with a galvanometer, the deflections of the latter may be used to determine the difference of the temperatures of the junctions. The instrument generally employed is the invention of Cumming, improved by Nobili by the introduction of astatic needles. By placing it in the vacuum of an air-pump, and diminishing the action of the earth's magnetism by an adjacent steel magnet, Mr. Joule has increased its sensibility so far as to indicate, by means of a single junction of bismuth and antimony, a change of temperature amounting to no more than one thousandth part of a degree centigrade. The thermo-multiplier was applied by Melloni to prove the instantaneous transmission of heat through glass and other bodies, and led him to the discovery of the extraordinary thermal transparency of rock-salt. It also enabled Forbes to demonstrate the polarization of heat, which had been unsuccessfully attempted with ordinary thermometers. Besides the extreme sensibility of the thermo-multiplier, the fineness of the wires used for the junction allows it to be employed where an ordinary thermometer would be unavailable. Thus it has been applied, without damage or serious inconvenience, to the measurement of the temperature of the muscles of the living human body. Recently also it has been successfully employed by Mr. Joule in confirming the deductions of Professor Thomson's theory of the thermal effects of stretching solid bodies. A single junction placed in contact with a rod either of gutta percha, iron, copper, or lead, indicated a cooling effect whenever the rod was stretched, and a heating effect on the removal of the stretching weight, the quantitative effect being strictly conformable with the theory. A result even more interesting was observed by him in the case of vulcanized India rubber. This material was found to be *heated* by stretching and to be *cooled* when the weight was removed—a result which in Professor Thomson's theory could only be accounted for by the supposition that the application of heat shortens vulcanized india rubber stretched by a weight. Accordingly, on making the experiment, Mr. Joule found this was the actual fact, and that if a strip of vulcanized india rubber is stretched by a weight which doubles its length, an elevation of its

temperature by 50° cent. shortens it by as much as one-tenth of its whole length.

Thermo-Magnetism. (1.) *Experimental Facts.*—Gilbert found that if a piece of soft iron between the poles of a magnet be raised to a bright red heat, it loses all its ordinary indications of magnetism, and it only retains (Faraday, *Exp. Res.*, 2344—2347), slight traces of the para-magnetic inductive character. Nickel loses its magnetic inductive capacity very rapidly as its temperature rises about 635° Fahr., and has very little left at the temperature of boiling oil. Cobalt loses its inductive capacity at a far higher temperature than that of either, near the melting point of copper. Of the three metals, iron remains nearly constant, nickel falls gradually, and *cobalt actually rises* in inductive capacity, as the temperature is raised from 0° to 300° Fahr. (Faraday, *Exp. Res.*, 3428; *Phil. Trans.*, Nov. 1855). Cobalt, of course, must have a maximum inductive capacity, at some temperature intermediate between 300° Fahr. and the temperature of melting copper. Crystals, when their temperatures are raised, have their magnetic inductive capacities in different directions of the crystalline substance rendered less unequal, and, in general, to a very marked degree. Thus Faraday found the difference of inductive capacities in different directions in a crystal of bismuth (a diamagnetic crystal) reduced to less than half when the temperature was raised from 100° to 280°. In carbonate of iron (a paramagnetic crystal) the difference of inductive capacities in different directions was reduced to one-third when the temperature was raised from 70° to 289° Fahr., and was tripled when the temperature was again brought down to 70° (*Exp. Res.*, 3400 and 3411.)

(2.) *Thermo-Dynamic Relations.*—The theory of the mutual convertibility of heat and mechanical work in reversible operations (Thomson, *Dynamical Theory of Heat*, §§ 11, 12, 13, 20, and 101), when applied to these phenomena, proves—1. That a piece of soft iron at a moderate or low red heat, when drawn gently away from a magnet, experiences a cooling effect; and when allowed to approach a magnet experiences a heating effect; that nickel at ordinary temperatures, and cobalt at high temperatures, within some definite range below that of melting copper, experience the same kind of effects when subjected to similar magnetic operations. 2. That cobalt at ordinary atmospheric temperatures, and at all temperatures upwards to its temperature of maximum inductive capacity, experiences a cooling effect when allowed to approach a magnet slowly, and a heating effect when drawn away. 3. That a crystal in a magnetic field experiences a cooling effect when its axis of greatest paramagnetic or of least diamagnetic inductive capacity is turned round from a position along to a position across the lines of force, and a heating effect when such a motion is reversed.

Thermometer. One of the three instruments, on the indications of which meteorological science reposes.—There is no difficulty in finding an instrument to indicate that the medium amid which it is remains at a certain fixed point, or has ascended or descended to some other fixed point, indicated by the co-presence of some physical event; but it is not an easy achievement to reach the power of saying that certain changes of state have been accompanied by proportional changes as to heat. The indication of change as to temperature usually adopted as a measure, is the dilation or expansion of some fixed substances that may be *Air*, or *Alcohol*, or *Mercury*. A given quantity, let us say of mercury, will always dilate or expand a certain proportion, with the same addition of Heat; but it is not true to say that the same increment of Heat necessarily produces the same dilatation, whatever be the temperature of the mercury to which that increment has been applied Dilatations of this kind accurately measured, certainly enable us to ascertain that we have reached the same advance, or rather the same step, in the advance of temperature: they do not indicate anything farther. An *absolute* thermometer must start from an absolute *zero*; and starting from the *zero*, the Modern Dynamical Theory of Heat furnishes propositions on which a scale might be constructed so that it be an absolute measure. For instance, with regard to an Air Thermometer, the absolute *zero*, or the zero of gaseous tension, is, according to Rudberg, —461° of Fahrenheit. Commencing to reckon at that point, we may state the following propositions.—(1.) When air is compressed or dilated, the absolute temperature varies as the cube root of the density; and the tension, as the fourth power of the absolute temperature, or the cube root of the fourth power of the density.—(2.) The mechanical force exerted by a given quantity of air while expanding from one density to another, is proportional to the difference of the cube roots of their densities, or to the difference of their absolute temperatures: hence the fall of temperature is proportional to the force expended.—(3.) The mechanical force exerted upon a given quantity of air while expanding from one density to another, is proportional to the difference of the cube roots of their densities, or to the difference of their absolute temperatures: hence the rise of temperature is proportional to the force exerted.—(4.) The total mechanical force exerted by a volume of air of a given tension while expanding indefinitely, is equal to that tension through three times the volume.—(5.) The total mechanical force exerted by a volume of air while expanding indefinitely, is proportional to its absolute temperature.—(6.) A given quantity of air while expanding under a constant pressure from one temperature to another, exerts a mechanical force equivalent to one-third the difference of temperature; and the quantity of heat required to change the temperature of air

under a constant pressure is four-thirds of that required to effect the same change of temperature under a constant volume.—As will be readily inferred, no thermometers whose scales indicate the equal dilatations of two different substances are exactly comparable. Between the air thermometer and the mercurial thermometer, (within the range included by the freezing and boiling points of water, or 0° and 100° Centigrade,) there is a marked difference. This difference, according to Regnault's observation, and those of Dubug and Petit, may be expressed by the following formula:—

$$T_m = \frac{B.T_a}{A-T} - \frac{T_a{}^3}{C^3} - \frac{T_a}{D}$$

where T_m is the temperature by the mercurial thermometer, T_a the temperature by the air thermometer, and A, B, C, D as follows.

$$A = 4539°{\cdot}617$$
$$\log B = 3{\cdot}7145723$$
$$\log C^3 = 6{\cdot}43303$$
$$\log D = 0{\cdot}78587$$

For further elucidation of this important subject, see a memoir by Mr. Waterston, in the *Philosophical Transactions* for 1853.—Passing, however, from these purely theoretical considerations, we shall lay down such practical rules and illustrations as are needful for the construction and comparison of thermometers as we usually find them. These have been embodied in an excellent report by Mr. Welsh of Kew Observatory; and may be divided into two heads.

I. General Rules and Practices.—"The plan of operations hitherto adopted has been that proposed by M. Regnault, and consists essentially of the following steps:—1st, Calibration of the tube: 2d, Graduation of the scale: and, 3d, The determination of the scale co-efficients.

"1. *Calibration.*—A tube having been selected as being tolerably free from all visible defects, a short column of mercury, generally less than one inch in length, is introduced. The tube is then attached to the frame of Perreaux's dividing engine, and by means of flexible tubing is put in connection at both ends with india rubber bags, the pressure upon which can be regulated by means of screws. The mercury is then brought to the part of the tube where the graduation is proposed to commence. The cutting frame of the engine carries also a small microscope with cross wires in its focus; on turning the dividing screw, the microscope wire is brought to coincide with the first end of the mercury, and the screw is then turned forward until the wire reaches the second end; the length of the column is thus given in revolutions of the screw. By means of the india rubber bags, the mercury is made to move along the tube until the first end coincides again with the microscope wire; the length of the column is again measured, and the mercury again moved forward; the same process being repeated until the column has been measured for each length of itself through the whole extent of the proposed scale. Permanent marks are made on the glass at the points of commencement and ending of the calibration. If the progress of the numbers shows any considerable irregularity in the tube, and as a verification of the first set of measures, it is well to repeat the calibration, commencing in this case at a point one-half the length of the column in advance of the original starting point. A series of measures interpolated from the two sets may then be adopted. Some experience is necessary in order to bring with facility the end of the mercury exactly to the wire of the microscope; but when care is taken to use very pure mercury and clean tubes, the operation can generally, after a little trouble, be accomplished with much accuracy. M. Regnault recommends that the motion of the mercury should be regulated by the breath, a drying substance being interposed to prevent moisture entering the tube. This method was employed for some of the first instruments made at Kew, but was abandoned in favour of the elastic bags.

"2. *Graduation.*—The measured lengths of the column of mercury in its successive steps along the tube correspond to equal volumes. Assuming that the calibre of the tube does not vary throughout the small length of the calibrating column, if we divide the spaces occupied successively by the mercury into an equal number of parts, it is evident that the divisions will represent the same *capacity*, although they may be of very different *lengths.* Before making the tube into a thermometer, the divisions of the scale may be verified by introducing a longer column of mercury, and examining whether the column occupies an equal number of divisions in different parts of the scale. If there should be any irregularity, a table of corrections may readily be formed. It will generally be found, however, that if the operations have been performed with care, and the tube is not very faulty, no correction will be necessary. The divisions are cut with a fine needle point upon a coating of engravers' varnish, and afterwards etched with fluoric acid. The required dimensions of the bulb may be found approximately by weighing a measured length of the mercurial column, and from the known expansion of mercury and its specific gravity computing the capacity of the bulb.

"3. *Determination of the Scale Co-efficient.*—The thermometer having been filled with mercury, we have an instrument the divisions of whose scale represent equal increments of the volume of the fluid, but are entirely of an arbitrary value. If now we determine the points of the scale at which the mercury stands in freezing and boiling water, we can immediately convert the arbitrary scale readings into degrees of the ordinary scale sof temperature. If a be the scale reading for the freezing point, and b that for the boiling point, the temperature by Fahrenheit's scale corresponding to any reading

$$n = \left(\frac{n-a}{b-a}\right)180° + 32°.$$

The freezing point is determined by placing the thermometer in finely pounded ice, from which the water is drained off as it melts. The boiling point is ascertained by the form of apparatus employed by M. Regnault; the temperature observed is that of steam, whose elasticity is the same as that of the atmosphere. A small siphon water gauge communicating with the interior of the vessel gives notice to the observer when the ebullition is being carried on too rapidly. The steam is generated from distilled water. The height of the barometer is observed at the time of the experiment, and the correction to a uniform height of 30 inches (reduced to 32°) is found from Regnault's table. In determining the fixed points, the stems of the thermometers are kept vertical; if the subsequent comparisons with other instruments are made in the same position, no error will arise from the expansion of the bulb caused by the pressure of the column of mercury. If, however, the thermometers are intended to be used in any other than a vertical position, it becomes necessary to determine the fixed points also in a horizontal position.—In accordance with the plan here sketched, fifteen thermometers have been completed with arbitrary scales. About thirty more tubes have been calibrated, and the bulbs attached and filled, but the scales not yet divided. The principal object in graduating the tube with an arbitrary scale is the convenience it affords of testing the divisions before it is converted into a thermometer. It is now proposed to divide the scale at once into Fahrenheit degrees after the thermometer has been made, and to test the accuracy of the divisions afterwards by detaching a portion of the mercurial column and making it move along the tube. If the scale should not then be found correct, a table of its errors can be formed and furnished with the instrument, or the thermometer rejected. The scales of these thirty thermometers have not yet been proceeded with, as it is desirable, before doing so, to allow the freezing point to have attained a permanent position. A few divisions have been cut on the tubes near the freezing point, and the reading with reference to the short arbitrary scale taken from time to time in melting ice. The period elapsed since the construction of the thermometers has been too short to afford as yet much information as to the probable constancy of the freezing points. They have, however, already shown generally a tendency to rise, in some cases to the extent of nearly 0°·3 Fahr., but in most of them it does not yet exceed 0°·1 or 0°·2. Another peculiarity in connection with the freezing point has shown itself in almost all the thermometers yet tried. After a thermometer has been exposed for some weeks to the ordinary temperature of the air, if its freezing point be ascertained, and it be then suddenly exposed for a short time to the temperature of boiling water, and again immediately placed in ice, it is found that the latter determination of the freezing point will be *lower* than the former by a very appreciable amount, generally between 0°·1 and 0°·2 Fahr. The freezing point does not recover its previous position for some time, probably two or three weeks. This peculiar displacement of the freezing point has been found to take place also in the case of a standard by Troughton and Simms belonging to the Royal Society. The freezing point of this instrument, before being raised to the temperature of boiling water, was 32·25, afterwards it had fallen to 32·15. This displacement of the freezing point has been remarked by Mr. Sheepshanks in the course of his experiments on standard thermometers. From the experiments now in progress, it is to be hoped that, after a time, some approximation may be made to the laws of these perplexing phenomena.—The apparatus employed for comparing the indications of different thermometers, consists of a cylindrical glass vase fifteen inches deep and 8½ inches in diameter, —a stand for supporting the thermometers under comparison, and a means of agitating the water in such a way as completely to assimilate the temperature throughout the vessel. The stand for the thermometer is a vertical rod, supported by a small tripod resting on the bottom of the vase. The thermometers are suspended from hooks sliding on this rod, and adjustable to any height; they are arranged, with their bulbs at the same height in a circle three inches diameter round the rod, and kept fixed with sufficient firmness below by being strapped with elastic bands against a projecting six-rayed frame attached to the supporting rod. Six thermometers of almost any form and length can thus be compared at once. The agitator is a flat ring of tinned iron, about two inches broad, fitting easily within the vase, and connected by four light rods with a similar ring at top, which serves as a handle. A packing of india rubber is placed on the outer rim of the plunger to prevent jarring against the glass. The flat tin ring is cut half across at several places, and the corners bent in various ways, so that when moved upwards and downwards the water is driven in *all* directions. The dimensions of the agitator are so arranged, that no part of it can possibly touch the thermometers when in operation. The vase, containing water, the stand with thermometers, and the agitator, is mounted upon a wooden revolving stand. The depth of water in the vase is always sufficient to include the whole of the column of mercury, the scales being observed through the water. In taking the observations, the observer, after agitating the water briskly for some time, turns the revolving stand till each thermometer is brought successively opposite to his eye, reading off the scales as quickly as possible to an assistant, who writes down the numbers. Proceeding in this way, it is found that six thermometers

can be read off and recorded easily in twenty seconds. It is of course desirable to make more than one set of readings for each temperature; and in order to avoid as much as possible the changes which may occur during the reading off, it is well to reverse the order of observing the instruments, that is, to read them alternately in the order one to six, and six to one.—The following table contains the results of comparisons of six thermometers, and will show the accuracy which may be obtained by the method of comparison just described; it will also exhibit the accordance in the indications of instruments graduated according to Regnault's process. Each result is the mean of six comparisons. No optical assistance was used in reading off the scales. The freezing points of all the instruments were determined on the same day, after the comparisons were made.

TABLE I.

Results of Comparisons of various Thermometers, March 19, 1852.

Standard Thermometers.							Barrow, E.I.C. S 7, No. 4.		Newman (Makerstoun).		Troughton and Simms (Royal Society).	
Kew No. 4.		Kew No. 14.		Fastré 231 (Regnault).		Temperature from mean of standards.						
Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.		Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.
°	°	°	°	°	°							
						32°·00	32°·05	+0°·05	32°·05	+0°·05	32°·25	+0°·25
38·69	—0·02	38·73	+0·02	38·72	+0.01	38 71	38·91	+0·20	38·86	+0·15	38·96	+0·25
45·05	+0·01	45 03	—0 01	45·03	—0·01	45·04	45·30	+0·26	45·18	+0.14	45·30	+0·26
49·96	0.00	49·97	+0·01	49·96	0·00	49 96	50·34	+0·38	50·23	+0·27	50 23	+0·27
55·33	—0·02	55·35	0·00	55·37	+0·02	55·35	55 87	+0·52	55·75	+0·40	55 62	+0·27
60·07	+0·01	60·06	0·00	60·05	—0·01	60·06	60 65	+0·59	60·58	+0·52	60·34	+0 28
65·39	—0·01	65·39	—0·01	65·41	+0·01	65·40	65·99	+0·59	66·03	+0·63	65·65	+0·25
69·93	0·00	69·92	—0·01	69·95	+0·02	69·93	70·57	+0·64	70·67	+0·74	70·22	+0·29
74·69	0·00	74·68	—0·01	74·69	0·00	74·69	75·39	+0·70	75·54	+0·85	75·02	+0·33
80·08	+0·02	80·03	—0·03	80·06	0·00	80·06	80·78	+0·72	81·00	+0·94	80·44	+0·38
85·30	—0 01	85 30	—0·01	85·33	+0·02	85·31	86·10	+0·79	86·25	+0·94	85·75	+0·44
90·50	0·00	90·49	—0·01	90·51	+0·01	90 50	91·36	+0·86	91·47	+0·97	90·87	+0·37
95·29	+0·04	95·23	—0·02	95 24	—0·01	95·25	96 15	+0·90	96.32	+1·07	95·72	+0·47
101·78	+0·01	101·76	—0·01	101·77	0·00	101·77	102·71	+0·94	103·04	+1·27	102·26	+0·49
109·21	+0·05	109·11	—0·05	109·15	—0·01	109·16	110·08	+0·92	110·62	+1·46	109·58	+0·42
						212·00					212·47	+0·47

The thermometers 'Kew No. 4' and 'Kew No. 14,' were graduated on the stems by Mr. Welsh, with arbitrary scales: the bulb of No. 4 is spherical, and is about $\frac{3}{4}$ inch diameter; that of No. 14 is cylindrical, $\frac{3}{4}$ inch long and $\frac{1}{4}$ inch diameter, and very sensitive. 'Fastré No. 231 (Regnault)' is a standard by Fastré of Paris, also graduated on the stem with an arbitrary scale according to Regnault's process. This instrument was examined and approved by M. Regnault; the determination by him of the scale co-efficient agreed closely with that afterwards made at Kew. The bulb is cylindrical, about $1\frac{1}{2}$ inch long and $\frac{1}{4}$ inch diameter. 'Barrow, E.I.C., S 7, No. 4,' is one of a number of thermometers made for the East India Company, and sent to Kew for examination. Its scale is of brass, divided to degrees. 'Newman (Makerstoun)' is the instrument which was supplied to the Makerstoun Observatory as a standard, and to whose indications the results of the temperature observations made there since 1841 have been 'corrected.' It was sent to Kew by Sir Thomas Brisbane for comparison with our standards. 'Troughton and Simms (Royal Society)' is a standard belonging to the Royal Society. As its scale extends to above 212, its boiling point was examined in the same apparatus employed for the Kew standards, its brass scale remaining attached to the tube. It was found to read 212°·7 when the barometer, reduced to 32°, stood at 30·136 inches.—The errors of a thermometer which has been already carefully examined between 32° and about 100°, may be obtained with considerable accuracy for temperatures below 32°, without using a freezing mixture, by the following process. Detach from the column of mercury a portion which will occupy about 40 or 50 degrees of the scale: bring this column within the known part of the scale. Let a, b be the readings at the upper and lower ends respectively; α, β the index errors at these points as determined by comparison with a

standard. Move the column until its lower end coincides with some degree below 32°, the upper end being within the compared portion of the scale. Let c, d be the scale readings for the upper and lower ends in the new position, γ being the scale error corresponding to c. The error of the scale at d will then be

$$d-\left\{\overline{c-\gamma}-\left(\overline{a-\alpha}-\overline{b-\beta}\right)\right\}.$$

The true length of the detached column may be obtained with increased accuracy by taking a mean of several measures within the known part of the scale. This method was adopted for 'Newman (Makerstoun)' and 'Troughton and Simms (Royal Society),' and the following errors obtained:—

Newman (Makerstoun). Temperature.	Error.	Troughton and Simms (R.S.) Temperature.	Error.
0·7°	—0·05°	5·1°	+0·14°
6·2	—0·08	10·0	+0·17
10·7	—0·12	15·0	+0·16
14·6	—0·10	20·0	+0·16
20·2	—0·04	24·8	+0·16
25·8	0·00		

The error of Newman had been previously found, by comparing with a standard in a freezing mixture at —3°, to be inappreciable."

II. Example of the Mode of Testing Thermometers.—The example about to be given is the history of the testing of thermometers at Kew, for the Arctic Expedition under Sir Edward Belcher.—"These instruments were twelve in number, seven mercurial and five spirit thermometers, graduated for low temperatures. The processes adopted for the two kinds of instruments being different, I shall describe them separately.

"1. *Mercurial Thermometers.*—These were divided into degrees of Fahrenheit's scale in the following manner:—The tube was first calibrated in the way already described in Mr. Welsh's former report on the construction of thermometers; marks being made on the tube at each extremity of the calibrated space. The bulb was then made and the mercury introduced by the glass-blower, the dimensions of the bulb and the quantity of fluid being made as nearly as possible to correspond with the scale proposed to be made. The thermometer was then placed in melting ice and the freezing point approximately set off with an ink mark; a similar mark being also made for a temperature of between 95° and 100°. A short arbitrary scale of four or five divisions was then divided at each of those points. The thermometer was then again placed in ice and the freezing point determined accurately with reference to the lower short scale; and comparisons with two standard thermometers in water determined the value of the upper short scale. Let O P be the calibrated portion of the tube, O being

O F H P

the point of commencement, F the freezing point as determined by means of the short arbitrary scale, and H the higher point of the scale similarly obtained. Let the distances O F, F H, H P be measured by the screw of the dividing engine. Let R_1, R_2, R_3, &c., R_f, &c., R_h, &c., be the lengths, in revolutions of the dividing screw, of the calibrating column of mercury for each successive step in its progress along the tube during the process of calibration; R_f being the length of the step in which the point F occurs, and R_h that in which H occurs. The values of R_1, R_2, &c., have been registered in the process of calibration; O F and O H have been obtained independently; the second measurement of O P, when compared with the sum of all the R's, will show with what exactness the column of mercury has been passed through its own length in its progress along the tube. Let r_f be the number of revolutions between the first end of the step f and the point F, and similarly r_h for the step h. We have then

$$\text{O F}=R_1+R_2+R_3+\&c.+R_{f-1}+r_f$$

$$\text{and O H}=R_1+R_2+\;\ldots\ldots\;+R_{h-1}+r_h;$$

whence we obtain r and r_h. Let K be the number of degrees equivalent to one length of the calibrating column,—this being of course constant for each length along the tube on the supposition of equal increments of volume for equal increments of temperature. Also, if we suppose that the capacity of the tube does not vary throughout the length of a single calibrating step, $\frac{r_f}{R}$ and $\frac{r_h}{R}$ will give the fractional parts of a step by which the points F and H are respectively in advance of the first ends of the steps f and h. We have then

$$\text{O F}=\left(\overline{f-1}+\frac{r_f}{R_f}\right)K,\quad \text{O H}=\left(\overline{h-1}+\frac{r_h}{R_h}\right)K;$$

$$\text{and F H}=\left(h-f+\frac{r_h}{R_h}-\frac{r_f}{R_f}\right)K=T_h,$$

the higher temperature —32°;

$$\text{whence K}=\frac{T_h}{h-f\;\frac{r_h}{R_h}-\frac{r_f}{R_f}}.$$

The degree corresponding to the point O is 32° $\left(\overline{f-1}+\frac{r_f}{R_f}\right)K$. The length of one degree for any individual step x is $\frac{R_x}{K}$.

From the quantities thus obtained, a table may readily be formed showing the value in revolutions of the dividing screw of one degree at all parts of the scale, and the graduation may then be proceeded with accordingly. The graduation is carried from —40° to + 120° or 130° Fahr.

"2. *Spirit Thermometers.*—In the graduation of mercurial thermometers, the practice is to con-

sider the increments of volume to be proportional to increments of temperature. If this were assumed in the case of spirit thermometers, very serious errors would be the result, even within moderate ranges of temperature. Dr. Miller having considered alcohol, as on the whole, the best fluid for thermometers intended to measure very low temperatures, Mr. Welsh was supplied by him with some which he had prepared with great care, its specific gravity being 0·796 at 60° Fahr. The first step to be taken was the determination of the *law* of expansion of the fluid in glass, as compared with that of mercury. For this purpose a tube was calibrated and divided with an arbitrary scale according to Regnault's process: its divisions were found, upon verification, to be of exactly equal capacity throughout. The tube was then furnished with a bulb of the same dimensions as those intended to be supplied to the Admiralty, and filled with the alcohol. This thermometer was marked S. 9 E. Comparisons were then made between the readings of this instrument and those of a standard mercurial thermometer, through as large a range of temperature as was found practicable. The comparisons above the freezing point were taken in water, in the apparatus described in my former report; those below 32° were taken in freezing mixtures of ice and salt or chloride of calcium. The following table contains the results of two series of experiments; the numbers in the first two columns are differences from the freezing point; those in the first being Fahrenheit's degrees; and in the second and third columns, the arbitrary scale divisions of the spirit thermometer S. 9 E.

TABLE II.

Table, containing results of comparisons between a Standard Mercurial Thermometer, and a Spirit Thermometer with an arbitrary scale of uniform capacity.

First Series.			Second Series.		
Standard mercurial thermometer.	Spirit thermometer S. 9 E.	S. 9 E. Observed *minus* calculated.	Standard mercurial thermometer.	Spirit thermometer S. 9 E.	S. 9 E. Observed *minus* calculated.
°	Scale div.	Scale div.	°	Scale div.	Scale div.
+69·95	+209·5	+0·2	+65·76	+196·4	+0·2
+66·93	+199 7	—0·1	+60·04	+178·3	0·0
+53·15	+156·7	—0·3	+52·04	+153·5	—0·1
+40·53	+118·2	—0·3	+37·72	+110·3	+0·2
+20·83	+ 60·1	+0·2	+24·05	+ 69·8	+0·4
+17·80	+ 51·0	—0·1	+16·01	+ 46·7	+0·8
—18·44	— 50·5	+0·9	—16·38	— 44·8	+0·9
—36·15	— 98·0	+1·2	—29·00	— 79·7	+0·4
—43·14	—117·9	—0·2	—36·33	—100·2	—0·5
			—44·72	—123·0	—1·1

To deduce the law of expansion from these comparisons, the numbers were arranged in equations of the form

$$A\,T + B\,T^2 - N = 0, \qquad (1.)$$

where T is the number of Fahrenheit's degrees from 32°, N the corresponding number of divisions by thermometer S. 9 E., A and B being the constants whose value is to be ascertained: the constants depending on higher powers of T than the second, were not considered.—The values of A and B were obtained from the equations by the method of least squares, and were as follows:—

From first series.............. $A = 2{\cdot}8203$ .. $B = 0{\cdot}002455$
From second series .... $A = 2{\cdot}8377$ .. $B = 0{\cdot}002221$
The mean of both series giving $A = 2{\cdot}829$ ... $B = 0{\cdot}002338$

The numbers in the columns 'Observed *minus* calculated,' are obtained by taking the difference between the observed readings of the spirit thermometer, and the numbers calculated from the *mean* values of A and B just stated.—Having determined upon the adoption of the law of expansion stated above, the graduation of the spirit thermometer was proceeded with as follows.—The process of calibrating the tubes was the same as for the mercurial thermometers; as in these, also, the freezing point and a temperature of 90° or 95° were determined with reference to short scales on the stems; the distances O F, O H, (fig., p. 735) were also measured; and by comparing these measurements with the numbers obtained by calibration, they were expressed in terms of lengths of the calibrating column. The equation (1.) may be put under the form $N = A\,(T + \theta\,T^2)$ by making $\theta = \frac{B}{A}$. Let f and h be the distances O F, O H expressed in steps of the calibrating column; $FH = h - f$. Let T_h be the number of degrees above 32° corresponding to H, and let α_0 be the value, in terms of a calibrating step, of

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one degree at the temperature 32° : we have then, according to the fundamental equation (1.),

$$h - f = \alpha_{\circ}\,(T_h + \theta\, T_h{}^2) \text{ or } \alpha_{,} = \frac{h - f}{T_h + \theta\, T^{h2}}$$

We may in general, without sensible error, assume that the value of one degree is uniform throughout the length of a single calibrating step, or if the column of mercury has been rather too long, we may subdivide the steps by interpolation. From the value of α_0, now obtained, we can find with sufficient exactness the temperature corresponding to the middle of the step f. It will now be convenient to make use of a table, derived from the values of A and B, showing the relative lengths of one degree at different temperatures on the supposition of uniform capacity of the tubes. The following are the values for every ten degrees, from —70° to + 100 Fahr. :—

Temp Fahr.	γ.	Temp. Fahr.	γ.
—70°	0·831	+20°	0·980
—60	0·848	30	0·997
—50	0·864	40	1·013
—40	0·881	50	1·030
—30	0·897	60	1·046
—20	0·914	70	1·063
—10	0·930	80	1·079
0	0 947	90	1·096
+10	0·964	100	1·112

The value in degrees of the step $f = \frac{1}{\alpha_f} = K_f$.

Then calling the numbers in the table γ since, $\frac{\alpha_0}{\alpha_f} = \frac{\gamma_0}{\gamma_f}$, we find $K_f, = \frac{1}{\alpha_0} \cdot \frac{\gamma_0}{\gamma_f} = \frac{K_0}{\gamma_f}$. This gives us the temperature corresponding to each end of the step f, and we may then proceed in like manner to find the values of the neighbouring steps, and so obtain successively the value throughout the whole range of the thermometer. The temperature corresponding to the point O in the figure is found by subtracting the sum of all the values of K between O and F from 32°. The length, in turns of the dividing screw, for any degree x is $\frac{R_x}{K_x}$, where R is the length of the step in which x occurs, and K_x the equivalent number of the degrees. A table can then be constructed, showing the lengths of each successive degree, commencing from the point O, by the aid of which the graduation may be performed. The scales extend to —75° Fahr.—The mercurial thermometers were, after their graduation, compared incidentally at two or three different temperatures, and found to agree generally to 0°·1 Fahr. They were all placed in melting ice, where it was found that four of them read exactly 32°, the other three,—viz., Nos. 34, 46, 47, were about 0°·1 too low. In a few of these thermometers the column of mercury could be readily broken: when this column was moved to different portions of the scale, it was found to occupy precisely the same number of divisions. This was the case with four of the instruments; the other three not having been tested in this way.—The five spirit thermometers were compared at four different temperatures with a standard mercurial thermometer. The comparison at 0° being taken in ice and salt, is not very trustworthy. Their errors were as follows :—

Temp by mer stand.	S. 2.	S. 4.	S. 6.	S. 7.	S. 8.	Mean of errors.
65°	+0·8°	—0·3°	—0·2°	+1·3°	—0·1°	+0·30°
52	+0·8	—0·2	—0·3	+1·4	0·0	+0·34
32	+0·8	—0·1	—0·3	+1·4	—0·3	+0 30
0	+0·6	0·0	0·0	+1 7	+0·2	+0 50

The numbers in this column 'Mean of errors' seem to indicate little error of a systematic nature. In the case of Nos. 2 and 7, the index error is very large: this, it is believed, is owing to some of the vapour of alcohol having become condensed in the upper portion of the tube before the fixed points were determined, and having escaped my notice; in fact the greatest attention is required to avoid errors from this source. These spirit thermometers cannot by any means be considered as standard, although they are doubtless more trustworthy than most of those usually made."

Attention to the previous admirable set of directions may guide alike maker and observer in the solution of any practical question that can occur in the construction and use of his instrument. It is impossible to overrate the advantages accruing to meteorological science from the establishment at Kew, and from the assiduity, care, and skill of Mr. Welsh and Mr. Ronalds.

III. *Thermetograph, or Registering Thermometer.*—Thermetographs have until lately been confined to the attempt to reach the maximum and minimum of diurnal temperature. Those best known in this country are Sixe's, Rutherford's, and, recently, those by Negretti and Zambra. They are described by Mr. Drew as follows :—

(1.) *Sixe's Register Thermometer.*—Mr. Sixe, of Colchester, described his register thermometer originally in the *Philosophical Transactions*, vol. lxxii. It is, in fact, a spirit of wine thermometer, with a long cylindrical bulb, and a tube bent in the form of a siphon with parallel legs, and terminating upwards in a small cavity. A portion of the two legs of the siphon tube, from *a* to *b* (fig. 1), is filled with mercury, the bulb and the whole of the rest of the tube with spirits of wine; the double column of mercury gives motion to the two indices *c* and *d*, each of which is a piece of iron wire capped with enamel at each end; they would move freely in the tube and rest on the mercury, were it not for springs, made of a thread of glass, or a hair, which, surrounding them, presses against the side of the glass with sufficient power to keep either index stationary in the spot where it is left by the retreat of the mercury. The action of the instrument is as follows :—When the increase of

temperature expands the spirit in the lengthened bulb G, the mercury in the leg of the siphon, a, is depressed, and a corresponding rise takes place in the leg b; in its rise the mercury urges before it the index d, which, being retained at its highest point by the spring, does not follow the retreat of the mercury, as the temperature decreases and the spirit in G contracts; this shows then the maximum heat in any determined period of time. When the spirit contracts, the mercury descends in the tube b and proportionally rises in a, urging before it the index c; leaving on the increase of heat, its lower extremity exactly at the highest point to which the column (the degrees decreasing upward) had risen on that side; pointing out, in fact, the minimum degree of temperature attained.—To prepare the instrument for future observations, the indices are brought down, by a magnet, to touch the mercurial column. If this instrument be read every twenty-four hours, it will evidently give the greatest and least temperature during the day.—Unfortunately this elegant instrument can hardly be trusted for very nice observations, and is very liable to get out of order. The use of two liquids, both expanding in different degrees, is a defect; and although it may, in part, be remedied by very nice dividing, and comparing the scale with a standard for every 5° or 6°, yet this process would increase considerably the price. The other defect arises from the liability of the springs to get out of order—a glass one by breakage, and the hair by losing its elasticity after long immersion in the spirit; while, from the perpendicular position of the tube, agitation from the wind is very likely to cause the indices to slide down the tube, and thus the observations would be lost. For these reasons Rutherford's maximum and minimum thermometers are preferred for registering the greatest and least degrees of heat; these we shall proceed to describe.

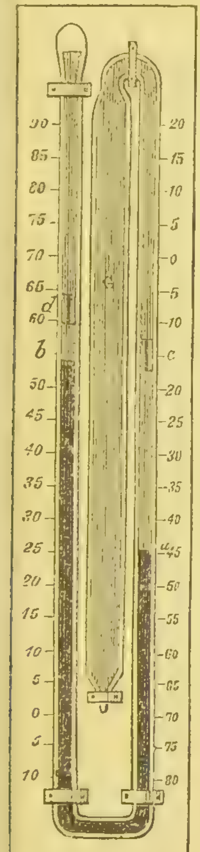
Fig. 1.

(2.) *Rutherford's Register Thermometer* (fig. 2).—A represents a spirit thermometer, B a mercurial, each furnished with a scale and fixed horizontally on the same plate of boxwood or metal; B contains within it a steel index, c, which is urged forward as the mercury expands by heat, and is left to indicate the highest temperature attained when the metal again contracts.—The spirit thermometer, A, contains a glass index, n, half an inch long, with a small knob at each end; it lies in the tube and allows the spirit freely to pass it as it expands; when contracted by cold, in consequence of the attraction between the spirit and the glass index, the last film of the column of spirit is sufficient to overcome the slight friction of the index on the inside of the tube, and to carry it backwards towards the bulb; it will rest, on the spirit again expanding, at the lowest degree of temperature attained within a given period. After reading off and to prepare the instruments for future observations

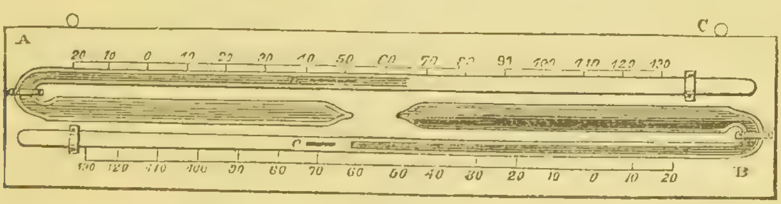
Fig. 2.

both indices are brought to the extremities—the one of the column of spirit, the other of that of mercury, by gently inclining the plate (at C) on which the thermometers are fixed, downward from the horizontal position. This must be done with some care, or the indices will get entangled with the liquids, from which they will be with difficulty extricated. Mr. Drew states that for three years he has used a register thermometer of this construction without any mishap, though he ruined many before he discovered the careful treatment they required.

(3.) *Negretti and Zambra's Maximum Thermometer.*—The maximum thermometer of Messrs. Negretti and Zambra is strongly recommended: fig. 3 will explain its construction. The tube is originally straight throughout; in this state a small piece of enamel, a, is introduced down it to within a short distance from the bulb; by means of a spirit-lamp, the tube is then bent just at the point where the enamel rests, and the heat required for this purpose is sufficient to cause its adhesion to the glass. The enamel does not fill the tube, but allows the mercury to

pass freely above it; on the decrease of heat, all that part of the mercurial column which has passed the enamel is left in the tube, while that portion nearer the bulb is separated and withdraws from it: the maximum heat will therefore be shown by the extremity of the detached

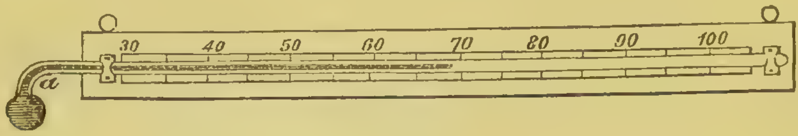

Fig. 3.

mercurial column: after this is registered, by depressing the bulb, the detached column may be made to reunite with the rest of the mercury; and thus the instrument is prepared for another observation. The advantage of the instrument seems to be, that there is no index to get out of order; the disadvantage that there is some considerable trouble in accurately determining the corrections to bring its readings in unison with a standard thermometer.—In a maximum thermometer devised by Professor Phillips, a portion of the mercurial column was separated from the rest by the intervention of a small bubble of air; this portion, as in the case of Negretti's, remains in the tube on the contraction of the mercury in the bulb, and thus serves to mark the highest reading attained: by the inclination of the tube it is then brought to its original position preparatory to another observation. This thermometer does not seem to have received the attention it deserves: its construction is simple and its indications sure, as those can testify who have used it. —But in recent years the mode of photographic registration has come into use in observatories, and promises to supersede wholly all other methods of observation. The only objection is the difficulty of integration. Nothing can surpass photographic registration in manifesting apparent anomalies. But if integration cannot be effected mechanically by some *planimeter*, such as that of Mr. Sang, or Professor Clerk Maxwell, it is unquestionably to be wished that resort be had, for the determination of mean values for long periods, to the aid of uncompensated pendulums.

Thermostat, or **Heat-Governor.**—A contrivance for regulating the processes of vaporization and distillation. It depends on this principle —when two bars of different metals are soldered together and heated, they expand differently with a separating movement, which may be used to open or close *valves*, *dampers*, &c., and so to regulate the process according to any preconceived mode.

Thetis. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Thunder-Rod. See LIGHTNING-CONDUCTOR.

Thunder-Storm. Under CLOUDS, and ATMOSPHERE ELECTRICITY OF, we have already given most of what is known concerning the development of the polar or opposite electricities within our aerial envelope. It remains simply to add something concerning the causes of actual *thunder-storms.* These are two:—1. An ascending current of vapour is subjected to rapid condensation within a colder region. 2. Two currents of air, from different quarters, and in opposite electric states, meet each other. —The former of these causes originates thunderstorms during summer; our winter thunderstorms are due to the latter. But in either case, the ultimate cause is a rapid condensation of vapour. It does not follow, however, that a rapid condensation of vapour must always produce a thunder-storm. Such condensation is invariably accompanied by the evolution of electricity, but its tension is generally too small to permit the production of a *spark*, or of lightning. In our climates these storms almost always follow a few calm and hot days, and a clear sky. If the causes giving rise to them are purely local, the elements of the storm formed within a circumscribed space, are transferred by the winds, burst out on their passage, and soon terminate their power: but if the originating causes embrace a large extent of country, the electric clouds, as first circumscribed, spread out in all directions, and affect very extensive areas. Such storms are more violent and frequent under the tropics during the rainy seasons and when these winds change, than in any other district of the earth. In our climates they occur generally —we had almost said solely—in the hot season; and when we pass into the interior of the Continent, their number appears to hold a relation with the quantity of rain that falls. Approaching more nearly to the poles—for example, into the Greenland Seas, their number strikingly diminishes. It appears, further, that an *oceanic atmosphere*—other things being the same—is less favourable than a continental one, to the generation of such storms. The curious reader is especially referred to the interesting essay by M. Arago, in the *Annuaire* for 1838,—now republished in the edition of his collected works.

Tide Mill. A mill in which the rising and falling of the tide either directly at the sea-shore, or in a tidal river, is made the motive power. It has never been much used in this country. There are two principal varieties:—1. In which the water-wheel rises and falls, and turns one way with the rising, and another with the ebbing tide. 2. In which the axle of the water-wheel neither rises nor falls, and in which it always moves in the same direction.

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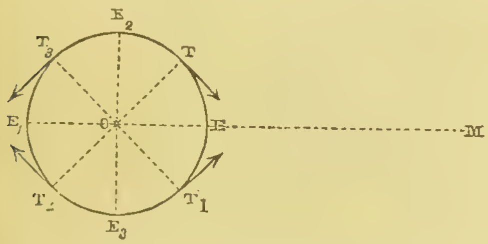
Tides. The nature of the force or forces giving rise to the oceanic waves, which pass by the name of Tides, will be readily understood by the student who is acquainted with the elements of the LUNAR THEORY.—The principles developed in pages 441, 442, and 443 of our Cyclopædia are indeed identical with those to which reference must now be made. Suppose $E\ E_2\ E_1\ E_3$ a section of the spherical mass of the earth, and that the moon is in the direction of M. The mass of our satellite must be regarded as a disturbing force tending to affect the perfect sphericity of



this section; and—should its surface be covered with a liquid—to impress on this liquid covering a new form or distribution. The disturbing action is twofold. *First*, we have a diminution of gravity in the direction of the radius of the earth, at E and E_1, and an increase of gravity in the same direction, at E_2 and E_3. If therefore canals were dug from these four points to the centre O, it is clear that they would not be in *equilibrio* under such circumstances, unless the canals $E\ O$ and $E_1\ O$ were longer than those at $E_2\ O$ and $E_3\ O$. The waters on the surface would therefore take on a *spheroidal* shape, the longest axis being $E\ E_1$, and the shortest axis $E_2\ E_3$. Thus we would have a greater depth of ocean at E and E_1 and a comparatively shallow ocean at E_2 and E_3—a conclusion indicating that twice during every rotation of the earth with respect to the moon, or rather, twice during every apparent daily revolution of the moon, we must have a high or full tide and a low tide. This peculiar influence of the moon is termed its *lifting power*, and in its absolute magnitude it is very considerable; but as its action is in all circumstances counteracted by terrestrial gravity, its positive effects are small. The lifting power would not raise the waters or produce a tide of more than ·07 of an inch, were the ocean 10,000 fathoms deep. This cause then, although a *vera causa*, is not adequate to evolve the tidal effects with which every one is familiar. But there is another element of the moon's disturbing force, viz., her *tangential force*. That tangential force acts in the direction of the arrows in the cut—manifestly tending to draw down the waters from T, T_1, &c., and therefore from E_2 and E_3, and to heap them up at E and E_1. The absolute disturbing energy of this force is, at its maximum, only three-fourths of the maximum lifting force; nevertheless as its influence is in nowise counteracted by gravity, there is no difficulty in seeing that it must be the main or efficient cause of the tides. In case of an ocean of the depth specified above, this tangential force would elevate the waters nearly *five feet*.—There is a further analogy between the problem before us and that of the lunar inequalities. The moon cannot act with equal energy at the two regions E and E_1. The effects at the two places will correspond in kind but cannot be identical in degree. Just as with the *Lunar Parallactic Inequality*, the disturbance will be somewhat larger at E than at E_1—on which difference is based the distinction between the *diurnal* and the *semi-diurnal* tide.—But the simplicity which at first sight characterizes the problem of the tides, disappears as we scrutinize it more narrowly. In the *first* place, the moon changes in declination, and no such change can take place without influencing the form of the consequences of her disturbing action. *Secondly*, another powerful disturbing energy is at work, viz., the Sun—not in accordance with the Moon, but often in opposition, and with a proper force also varying according to the declination of that orb. There is thus a solar tide as well as a lunar tide, and the actual tide is the composite or resultant of the two waves. At definite seasons, viz., at new and full moon, the two systems of waves concord, and at these periods we have the highest or *spring tides*. When the moon is in quadrature, on the contrary, the two systems conflict, and then we have the minimum rise of the waters, or our *neap tides*. In this case, as with astronomical inequalities, we separate the effects depending on the concurrence of the two influences, from those that arise from their conflict; and to these latter—as if they were distinct tidal waves—long and appropriate periods are assigned. According to this view, then, the apparent height of the tide at any moment is made up of three parts, depending on three astronomical causes:—

1.—The diurnal luni-solar tide,

2.—The semi-diurnal luni-solar tide.

3.—Tides of long periods depending on the change of position of the sun and moon, or the semi-menstrual and semi-annual tides.

To which the operation of two other physical causes must be added, viz.:—

4.—The elevation or depression of the water due to slow changes of barometric pressure; and

5.—Abrupt changes due to Wind.

It must not be supposed, however, that the foregoing elements are more than the barest elements, entering into the solution of the actual problem of the tides. They give us a definite conception, indeed, of the forces that originate the tidal waves; but according to what laws are these waves propagated? What retardation is due to the inertia of the liquid? What apparent acceleration to the persistency of a wave? And more important than all, how shall we estimate the influences of the shape, depth, and distribution of our terrestrial seas and oceans? Dif-

ferent views have been taken of the conditions of fluid motion, viz., what is called the Equilibrium Theory, Laplace's Dynamical Theory, and Mr. Airy's Theory of Canal Waves. No problem in hydrodynamics can be practically solved at present on the ground of pure *à priori* principles; nevertheless these *à priori* formulæ are an important guide to the observer. Perhaps no greater gain has recently been obtained in this direction, than that distinct separation of the lunar and solar tidal effects, which we owe to Mr. Haughton: that the problem, in itself, is not yet one for deductive science has been established beyond question by the utter incongruities between the calculations of Laplace himself and the facts of the tides at Brest; but Science unquestionably owes her willing acknowledgments to Dr. Whewell, for the energy and success with which, supported by the *British Association*, he urged forward the discovery of *empirical laws*. The inquiry, indeed, is of that nature which excludes hope of theoretical advance, unless through aid of such laws. And it is due to the Master of Trinity to allege, that no other physicist has wrought so well in a direction, the successful promotion of which demanded great perseverance as well as a high sagacity. We cannot follow out the inquiry here. The student is referred to the various papers and reports of Dr. Whewell; to the map of Co-tidal Lines, by Mr. Scott Russell, in Johnston's *Physical Atlas;* and very especially to the memoirs by Mr. Haughton in the first vol. of *Philosophical Magazine* for 1856. The meaning of all technical terms will be found in any elementary book on Navigation.

Time. The interval between two successive returns of the vernal equinox to the same meridian is called a *sidereal* day.—The interval between two successive returns of the sun to the same meridian is called a *solar* day.—The sun completes an apparent revolution about the earth in one year, or 365 days 5 hours 48 minutes and 47·57 seconds; so that the sun's mean daily motion is found by the proportion,—one year : one day : : 360° : daily motion = 59′ 8″·33.—This motion is not uniform, but is greatest when the sun is nearest the earth. Hence the solar days are unequal; and to avoid the inconvenience resulting from this fact, astronomers have recourse to a *mean* solar day, the length of which is equal to the mean or average of all the apparent solar days in a year.—The length of the mean solar day is different from that of the sidereal, because when the mean sun, in its diurnal motion, returns to the meridian, it is 59′ 8″·33 advanced eastward in right ascension.—An arc of the equator, equal to 360° 59′ 8″·33, passes the meridian in a mean solar day, while only 360° pass in a sidereal day. To find the excess of the solar day above the sidereal day, expressed in sidereal time, we have the proportion

360° : 59′ 8″·33 : : one day : 3m. 56·555s.

Hence 24 hours of mean solar time are equivalent to 24h. 3m. 56·555s. of sidereal time. As we have frequent occasion to convert intervals of mean solar time into intervals of sidereal time, suitable tables have been constructed, from which such intervals are found by mere inspection.

The methods of determining true local time by observation are four:—

(1.) *By equal altitudes of a star on opposite sides of the meridian.*

Observe the times when the star has equal altitudes before and after passing the meridian; the arithmetical mean between these times is the time of the star's passing the meridian. By comparing this time with the known place of the star, we may obtain the error of the clock.

(2.) *By equal altitudes of the sun.*

Since the declination of the sun changes from morning to evening, the time of the sun's arriving at a given altitude is affected by this motion, and we must compute the correction to be applied to the mean of the times observed.

(3.) *By a single altitude of the sun or a star.*

Let P Z H be the meridian of the place of observation, P the pole, Z the zenith, and S the place of the sun or star. If the zenith distance, S Z, has been measured and corrected for refraction, then in the spherical triangle, Z P S, the three sides are known, viz.,

P Z = the co-latitude = ψ;
Z S = the true zenith distance = z;
P S = the north polar distance of the star = d.

In this triangle we can compute the angle Z P S, which is the distance of the star from the meridian.—Whence,

$$\sin \tfrac{1}{2} \text{A} = \sqrt{\frac{\sin (\text{s} - b) \sin \text{s} - c)}{\sin b \sin c}}.$$

Put $2\,\text{s} = z + d + \psi$; then

$$\sin \tfrac{1}{2} \text{P} = \sqrt{\frac{\sin (\text{s} - \psi) \sin (\text{s} - d)}{\sin \psi \sin d}}.$$

It may be found convenient to employ in our computation ψ, the latitude of the place, and d the declination of the star, rather than the co-latitude and polar distance. For this purpose, we have only to substitute in the preceding formula d for $90° - d$, and ϕ for $90° - \psi$, and we shall obtain $\sin \tfrac{1}{2}$ P =

$$\sqrt{\frac{\sin\left(\frac{z + (\phi - \delta)}{2}\right) + \sin\left(\frac{x - (\phi - \delta)}{2}\right)}{\cos \phi \cos \delta}}$$

(4.) *To determine time by the transit instrument.*

The instant of the sun's passing the meridian is the time of apparent noon; and hence, if we compare the sun's passage over the meridian with a chronometer, we shall obtain the deviation of the chronometer from apparent solar time. If to this we apply the equation of time with its proper sign, we shall obtain the error of the chronometer in mean time.

Time Ball. A contrivance by means of which the exact time can be communicated over a wide space by the clock of an observatory. It is essentially this:—On the top of an elevated place an upright rod is adjusted, along which a large ball or sphere can be make to slide up and down. A short time previous to the hour to be indicated, the ball is raised to the top of the rod, from which it is dropped at the exact hour. This used to be effected by actual interference of the observer; but now the movement is *automatic*, the ball being dropped through the agency of an electro-magnetic apparatus in connection with the astronomical clock. The same clock may drop any number of balls at distant places provided they are in electro-telegraphic correspondence with it. A large extension might be given to the system of time balls, with much practical advantage to our shipping.

Torricelli, Theorem of. A general principle in *Statics*, announced by Torricelli, as follows:—"When a system of bodies is in equilibrium, its centre of gravity is either at the highest or lowest point that it can attain." It is needful to combine with this, certain principles referred to under "STABILITY."—The theorem fully generalized and freed from specialty of expression—as it has been under more advanced statics and analysis—is of much importance in facilitating the general solution of problems relating to heavy bodies—*e. g.*, the problem of the Catenary. Maupertuis subsequently generalized this theorem, and gave it the name of the *Law of Repose.*—Like all such theorems in Statics and Dynamics, Torricelli's has been superseded or been overlooked, because of the wide grasp of our general methods.—For discussion of the conditions of stable and unstable equllibrium, to which such statical theorems refer, see "STABILITY."

Torsion. The force with which a string or thread returns or tends to return to a state of rest, after it has been twisted. The force of torsion has often been referred to in this Cyclopædia, and many of its practical applications noticed. It may be compared as follows with the force of gravity:—The time in which a pendulum of the length l moves in a small arc, is known to be $\pi \cdot \sqrt{\frac{l}{g}}$. The time in which a torsion wire vibrates once on its axis being made equal to the time in which a simple pendulum vibrates, we have

$$\frac{K^2 M}{n} = \frac{l}{g}$$

Therefore as the momentum of inertia for a torsion wire suspending a body of a given form, can be computed, and as l may be found from the observed time of a vibration, the value of n—the co-efficient of the force of torsion—can readily be ascertained from this equation. M is the mass suspended, and K is radius of gyration. Most valuable applications of the force of torsion have been made to physical research,—for instance, *Coulomb's Torsion Balance*, and the *Bifilar Magnetometer* of *Harris and Gauss.*

Trade Wind. See WINDS.

Trajectory. The path of a body's motion is called the trajectory. Thus the trajectory of a heavy point projected in vacuo under the action of gravity, is a parabola.

Transit Instrument. A most important instrument in practical astronomy. Its general construction will be best understood by reference to the figure already given under CIRCLE (fig. 4, p. 111).—The object of the instrument being to detect the transit of a star across the meridian of a place, it is clearly necessary that it move in a meridian circle, or so that its line of collimation sweep along that circle. To accomplish this object, the axis of the telescope must plainly be *horizontal*, and made to rest therefore on the top of strong stone pillars, not connected with the floor on which the observer walks. It is impossible to secure *absolute stability* to the horizontal axis, whatever be our precautions. The two pillars vary, expanding or contracting by some quantity, in consequence of the alternations of summer and winter temperature. But, generally speaking, two massive and corresponding blocks of stone, founded deeply in the earth, will prôduce all the stability that can be attained. It is assumed, of course, that the other mechanical adjustments of the instrument are also carefully provided for,—viz., that the pivots on which it turns are round, and that the telescope tube be at right angles to the horizontal axis. The exactness or failure of the pivots may readily be ascertained by turning the instrument when its great level is placed on it; and the perpendicularity of the tube is also readily tested on reversing the axis,—that is, placing the west end to the east, and *vice versa.* These mechanical essentials being supposed secured, it next becomes necessary to determine how far the place of the instrument is in error, and therefore what are the corrections that must be applied to the observed transit of any star, so that the true time of its transit be deduced. There are three errors of place. *First*, the axis may not be exactly horizontal—an error at once detected and valued by reversal of the level; *secondly*, the line of collimation may not be correct—an error detected and valued by a reversal of the axis of the instrument; or, better still, by Bohnenberger's method, as described under CIRCLE; and, *thirdly*, there may be an error in *azimuth*, *i. e.*, the collimation line, although describing a great circle passing through the zenith point, may not describe the *meridian circle.*—There are three methods of determining the error in azimuth. (1.) Observe two successive transits (the upper and lower), of the pole star, or any close circumpolar star, then the azimuthal deviation of the

transit is equal to half the difference between the observed interval and twelve hours (in seconds), multiplied by the coseant of the latitude, and the cotangent of the star's declination. (2.) Observe the transits of two stars (their places being known), that differ considerably in declination, but very little in right ascension; the azimuthal error will be

$$a = \frac{\Delta \cos \delta \cos \delta'}{\cos \phi \sin . (\delta' - \delta)}$$

where δ, δ' are the declinations, ϕ the latitude of the place, and Δ the difference of the observed times, *minus* the difference of right ascensions. (3.) Observe two triangular stars at opposite culminations—the one above, the other below the pole,—the error in azimuth will be

$$a = \frac{\Delta \cos \delta' \cos \delta}{\cos \phi . \sin (\delta' + \delta)}$$

where Δ is the difference of the observed times, *minus* the difference of the right ascensions, neglecting the twelve hours. These corrections being found, the time of the transit of any star may be changed into its right ascension by the following formula.

$$\text{R A} = (\text{T} + d\,t) + a . \frac{\sin (\phi - \delta)}{\cos \delta} + b . \frac{\cos (\phi - \delta)}{\cos \delta} + \frac{c}{\cos \delta}$$

where T is the observed time, $d\,t$ the error of the clock, and a, b, c, the azimuthal, level, and collimation errors respectively. The errors above referred to, must be constantly watched, for even the best and most solid instrument is liable to very irregular fluctuations. Very full instructions as to the discovery of these corrections and the general management of the transit instrument, will be found in any good work on practical astronomy; but we think it right to transfer from the treatise by Professor Loomis, the following special account of the application of electro-magnetism to the exacter record of transits that recently came into use in America, and is being tried at Greenwich. It promises to supplant altogether the former mode of eye-estimation:—

This application involves two contrivances entirely distinct from each other. The first is a method by which an astronomical clock may be made to break the electric circuit at the end of every second; and the other is the register, for recording not only the beats of the clock, but also any other arbitrary signals at the pleasure of the operator.

1. *The Electric Clock.*—The electric circuit may be broken every second, by means of a clock, in a variety of ways. Dr. Locke introduces into the astronomical clock a wheel with sixty teeth, which makes one revolution per minute. Each tooth, in succession, strikes against the handle of a platinum tilt-hammer, A C, weighing about two grains, and knocks up the hammer, which almost immediately falls to a state of rest on a

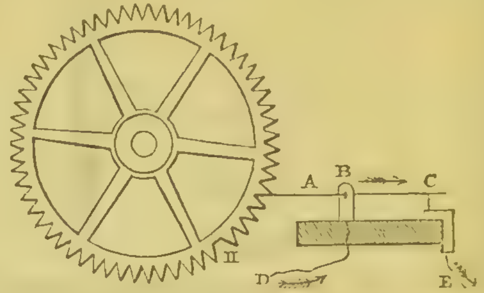


Fig. 1.

bed of platinum. The fulcrum B of the tilt-hammer, and the platinum bed, rest, severally, on a small block of wood. Each is connected by wires D and E, with a pole of the galvanic battery, and the circuit is alternately broken and completed by the rising and falling of the hammer. The circuit is open about one-tenth of a second, and closed the remaining nine-tenths of each second.—Professor Bond insulates the axis of the escapement wheel, and also the axis of the steel pallets, by a ring of shellac. Wires from the two poles of the battery are connected with each axis, so that when either pallet comes in contact with an escapement tooth, the galvanic circuit is closed; and when the contact is broken (as it must be at every oscillation of the pendulum), the galvanic circuit is opened.—At the Washington Observatory the same object is accomplished in the following manner:—A small piece of metal M, is attached to the back of the clock, near the lower extremity of the pendulum, and upon it is placed a small globule of mercury, so that the index B, attached to the lower extremity of the pendulum, may pass through the globule of mercury once every vibration. A wire from one pole of the battery is connected with the supports of the pendulum C, and another wire from the other pole of the battery connects with the metallic support of the mercury globule. If now the pendulum were at rest with the pointer B, in the mercury, it is evident that the electric circuit would be complete through the pendulum. If then the pendulum be set in motion, it will break the circuit whenever it passes out of the mercury, and restore it again as soon as it touches the mercury.—Mr. Saxton employs a small tilt-hammer, like Dr. Locke, but he breaks the circuit by means of a small glass pin projecting from the pendulum.—A B C (fig. 3) represents a platinum wire, mounted upon a pivot at B, the end A being somewhat heavier than the other, and resting upon a metallic bed D. At C, the

Fig. 2.

wire is bent so as to form an obtuse angle. The wire E goes from D to one pole of the battery,

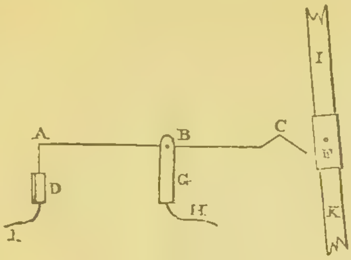


Fig. 3.

while the wire H, from the other pole of the battery, communicates with the metallic support G, and thence with the wire A B. When the end A of the platinum wire rests upon the support D, it is evident that the electric circuit is complete. This apparatus is placed near the middle of the pendulum (a portion of which I K, is represented in the cut), and just in front of it, so that the pendulum may swing behind it without obstruction. A small glass pin F, about half an inch in length, is attached to the pendulum in such a position that, at every vibration of the pendulum, the pin shall slightly impinge upon the angle C of the platinum wire, and force up the end A. As soon as the pin has passed the point C, the end A falls back again upon its support D. Thus, at every vibration of the pendulum, the end A of the platinum wire is lifted about a tenth of a second, and rests upon D during the remaining nine-tenths of the second; that is, the electric circuit is closed about nine-tenths of every second, and is open during the remaining tenth.—By either of these methods, as well as several others, the electric circuit may be broken every second by means of a clock.

2. *The Register.*—The most obvious mode of registering the beats of the clock is upon a long fillet of paper, after the ordinary method of telegraphic communications. If the paper be allowed to run through an ordinary Morse registering apparatus, and the circuit be broken every second by the clock, the graver will trace upon the paper a series of lines of equal length, separated by short interruptions, thus:—

It is easy to reverse the action of the graver, so that when the circuit is complete, the paper shall be entirely free, and a dot be made by the breaking of the circuit. A paper graduated into seconds by this arrangement, exhibits dots with long intervening spaces, thus:—

instead of long lines with short blanks, as shown before.—In order to indicate the commencement of the minute, a dot may be omitted at the end of every sixty seconds. This is accomplished in Dr. Locke's clock, by omitting one tooth in the wheel which breaks the circuit, as shown at H, in fig. 1, page 743.—The mode of using the register for marking the date of any event, is to tap on a break-circuit key simultaneously with the event. The beginning of the short line thus printed upon the graduated scale of the register, fixes, by a permanent record, the date of the event. Thus A represents such a record printed upon the graduated paper.

By tapping upon the key at the instant a star is seen to pass each of the wires of a transit instrument, the observation is instantly and permanently recorded. The usual rate of progress of the fillet under the pen is about one inch per second, and the observations are read off by means of a graduated transparent scale, about an inch square, as represented in the annexed cut, consisting of equidistant and parallel lines, ruled upon a piece of glass by means of a diamond, or etched with fluoric acid. If the interval between the second dots be greater than the breadth of the scale, the scale is turned obliquely across the fillet, until the first and last divisions exactly comprehend the space between the two second dots. Let the distance from 4s to 5s, on the above scale, be the distance on the fillet between the fourth and fifth seconds, and let the dot *a* between them represent the observation. It appears, by inspection, that the observation was recorded between 4·7 and 4·8 seconds. The distance of *a* from the nearest scale division may be estimated to tenths. Thus time is accurately measured to tenths, and may be estimated to hundredths of a second. On some accounts, it is more convenient to employ a scale consisting of diverging lines, as represented in the annexed cut, so that the breadth of the scale may always exactly comprehend the interval between the second dots, which intervals must necessarily vary somewhat in length.—This method of recording transits not only possesses the advantage of precision, but also of performing vastly more work in a given time. Fifteen seconds is the ordinary equatorial interval for the wires of a transit instrument; but when the transits are printed on paper, in the manner now described, this interval may easily be reduced to two or three seconds. The value of a night's work with the transit instrument is thus increased many fold.—To obviate the inconvenience of a long fillet of paper, Mr. Saxton has substituted a

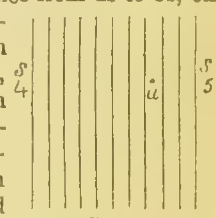


Fig. 4.

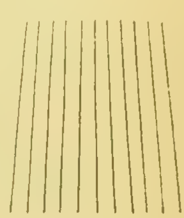


Fig. 5.

cylinder, about eight inches in diameter and two feet long, enveloped with paper, which may be removed at pleasure. This cylinder is made to revolve, with a uniform motion, upon a screw axis, so that the recording dots are made upon a perpetual spiral. One sheet, filled in this manner, will contain about two hours' work with a transit instrument.—In order to secure the full advantage of the preceding method, it is important that the paper which contains the register be made to advance with entire uniformity. The Messrs. Bond have invented for this purpose a machine which they call the Spring Governor, consisting of a train of clock-work connected with the axis of a fly-wheel. It has an escapement-wheel, into the teeth of which pallets are operated by the oscillations of a pendulum, as in ordinary clocks, the wheel being so connected with its axis by a spring as to allow the axis to move while the wheel is detained by the pallets. The register is made upon a sheet of paper wrapped round a cylinder.

Translation. As distinguished from *rotation*, consists in the movement of a point from one position to another. There is a simple movement of translation in a body when its centre of gravity is moved, and when the body is rigidly connected. The translation for every point is identical with that for the centre of gravity.

Transparency. The property of bodies which enables light to pass through them. No body is perfectly transparent; there are none in which a portion of the light rays are not absorbed. Again, no body is perfectly opaque—even gold, for example, if thin enough will transmit a dull light.

Transversals. A general method of treating many geometrical subjects, brought into notice by Carnot. The following elementary illustrations are copied from Mulcahy:—

LEMMA 1.—If three right lines $A\,A'$, $B\,B'$, $C\,C'$ be drawn from the angles of a triangle $A\,B\,C$ to meet in any point O, the segments of one side of the triangle will be in a ratio compounded of the ratios of the segments of the other sides, that is,

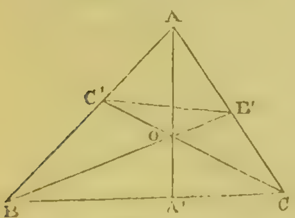


Fig. 1.

$$B\,A' : A'\,C :: \left\{ \begin{array}{l} B\,C' : C'\,A \\ A\,B' : B'\,C \end{array} \right\}$$

For, $B\,A' : A'\,C :: \Delta\,A\,B\,A' : \Delta\,A\,C\,A'$; also, $B\,A' : A'\,C :: \Delta\,B\,O\,A' : \Delta\,C\,O\,A'$; therefore $B\,A' : A'\,C :: \Delta\,A\,B\,O : \Delta\,A\,C\,O$, or

$$\left\{ \begin{array}{l} \Delta\,A\,B\,O : \Delta\,C\,B\,O \\ \Delta\,C\,B\,O : \Delta\,A\,C\,O \end{array} \right\}, \text{ that is,}$$

$$:: \left\{ \begin{array}{l} A\,B' : B'\,C \\ B\,C' : C'\,A \end{array} \right\}. \quad Q.\,E.\,D.$$

This relation may be otherwise expressed as follows:—

We have (*Euclid*, b. vi., prop. 23) $B\,A' : A'\,C :: A\,B' . B\,C' : B'\,C . C'\,A$: and, therefore, $A\,B' . C\,A' . B\,C' = A'\,B . C'\,A . B'\,C$, that is, *the continued products of the alternate segments of the sides are equal.*—Similar results will hold good when the point O is outside the triangle. In that case *two* of the points A', B', C' will lie on the sides produced.—Conversely, it follows, *ex absurdo*, that if the relation above-mentioned obtains, amongst the segments of the sides of a triangle made by lines drawn from the angles (under the restriction just stated, relative to a point outside), those lines will meet in one point.

LEMMA 2.—If a right line $C'\,A'$ be drawn, cutting the three sides of a triangle $A\,B\,C$, their segments so formed will have a relation similar to that expressed in the former Lemma.

For, draw $C\,O$ parallel to $A\,B$ (see figs.); then

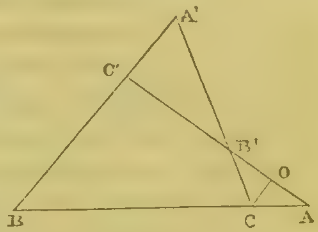


Fig. 2.

$$B\,A' : A'\,C :: B\,C' : C\,O :: \left\{ \begin{array}{l} B\,C' : C'\,A \\ C'\,A : C\,O \end{array} \right\};$$

but $C'\,A : C\,O :: A\,B' : B'\,C$; therefore

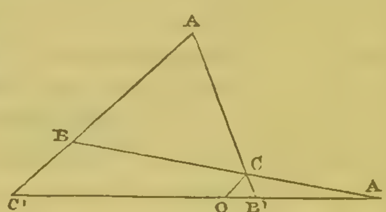


Fig. 3.

$$B\,A' : A'\,C :: \left\{ \begin{array}{l} B\,C' : C'\,A \\ A\,B' : B'\,C \end{array} \right\}.$$ And so on as before.

Conversely, if a point be taken on each side of a triangle (or the side produced), so that the segments thus formed shall satisfy the relation above referred to, those three points will be in one right line, provided that an *odd* number of the points shall lie on the productions of the sides.—These Lemmas being premised, we shall now explain the harmonic properties of a triangle.—"Let three right lines $A\,A'$, $B\,B'$, $C\,C'$, be drawn (as in Lemma 1) from the angles of a triangle, to meet in a point O, and let the lines $A'\,B'$, $B'\,C'$, $C'\,A'$ be produced to meet the sides $A\,B$, $B\,C$, $C\,A$, respectively, in C'', A'', B''; then, 1°, all the lines on the figure so formed are cut harmonically, and, 2°, the points A'', B'', C'', are in one right line."

1°. By the foregoing Lemmas $B\,A' : A'\,C :: B\,A'' : C\,A''$; that is, $B\,A''$ is cut harmonically.

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A similar proof applies to $B\,C''$ and $A\,B''$. Again, if $A\,A''$ be joined, as $B\,A''$ is cut harmoni-cally, so also is $C'\,A''$; and in like manner $C''\,A'$ and $B''\,C'$. For similar reasons $B\,B'$ is cut harmonically, and also $A\,A'$ and $C\,C'$.

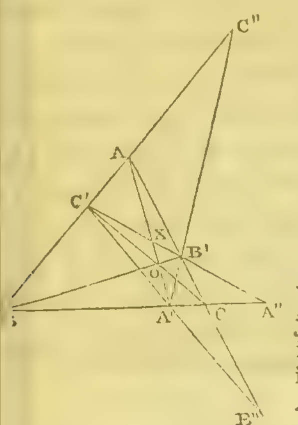


Fig. 4.

2°. Join $B'\,B''$, now, if the three lines meeting at B'' be taken as three legs of an harmonic pencil ($B''\,B$ and $B''\,C$ being supposed conjugate), the fourth is determined; but it must pass through A'' and C'', since these points are harmonic conjugates to A' and respectively; therefore B'', A'', C'', are in ·e same straight line.—*If from the ends of ·e base of a triangle two lines be drawn to ·e opposite sides, so as to intersect on the per- ·ndicular to the base, the lines joining the ·ot of the perpendicular to their intersections ·th the sides, make equal angles with the base.* ·r, if $A\,A'$ be perpendicular to the base (see ·st figure), A' is the vertex of an harmonic ·ncil two of whose alternate legs are at right ·gles, and therefore the angle $B'\,A'\,C'$ is bi- ·cted, and the proposition is proved.—Some of ·ese conclusions may be otherwise expressed. ·e following definition must be first laid down: ·the opposite sides of a quadrilateral be pro- ·ced to meet, the line joining their intersections ·y be called *the third diagonal* of the quadri- ·eral. Thus (see last figure), $B\,C$ is the third ·gonal of $A\,C'\,O\,B'$; again $C'\,B'$ is the third ·gonal of $A\,B\,O\,C$; and considering $C'\,B\,B'\,C$ ·o as a quadrilateral, $A\,O$ is its third diagonal. · have then the following proposition:— ·he *three diagonals* of any quadrilateral belong ·o to two other quadrilaterals, whose sides are ·ments respectively of those of the first quadri- ·ral: and each of them is cut in conjugate ·monic points by the other two." The figure ·ned by three such quadrilaterals is called by ·not a *complete quadrilateral*—Given in posi- · one pair of opposite sides of a quadrilateral, the point of intersection of the other pair, to the locus of the intersection of the diagonals ·t is, the diagonals as commonly understood). · $A\,B$ and $A\,C$ be the lines given in position, A'' the given point (see last figure); then, ·intersection of the diagonals of the quadri- ·ral $C\,B'\,C'\,B$ will evidently lie on the har- ·ic conjugate of $A''\,A$, with respect to $A\,B$ and · namely $A\,A'$. This line is then the required ·s.—*Let a right line revolve round a given point ·nd cut two given right lines* $V\,A'$ *and* $V\,B'$ *in* ·d B; *let a portion* $O\,X$ *be taken on it, such that its reciprocal shall be equal to the sum of the reciprocals of* $O\,A$ *and* $O\,B$ (*more concisely thus:*

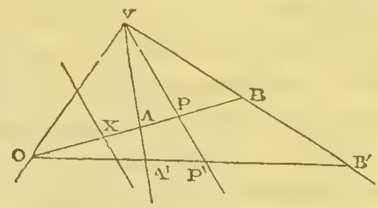


Fig. 5.

$\left.\frac{1}{O\,X} = \frac{1}{O\,A} + \frac{1}{O\,B}\right)$; *required the locus of the point* X. ($O\,X$, $O\,A$, $O\,B$, are supposed to be taken in the same direction.) Draw any line from O, and let A' and B' be the points where it cuts the given lines; cut $A'\,B'$ in P', so that $A'\,P' : B'\,P' :: O\,A' : O\,B'$ (*Euclid*, b. vi., prop. 10); join $O\,V$ and $V\,P'$; then *a right line bisecting* $O\,V$ and parallel to $V\,P'$ *will be the locus required.* For, as $O\,B'$ is cut harmonically, $O\,B$ is cut harmonically; therefore

$$\frac{1}{O\,A} + \frac{1}{O\,B} = 2\,.\,\frac{1}{O\,P};$$

but by the definition of a *reciprocal* (*q. v.*) it is plain that the reciprocal of half a line is double the reciprocal of the whole; therefore, as the line drawn parallel to $V\,P$ bisects $O\,P$, this parallel passes through the point X in all its positions. *Q. E. D.*—If the revolving line take such a position that B comes to the other side of V (A remaining on the same side as before), it will be found, by a similar proof, that it is cut by the same parallel in a point X, so that $\frac{1}{O\,X}$ equals the difference of $\frac{1}{O\,A}$ and $\frac{1}{O\,B}$. In order to reconcile this with the former result, we must recollect that $O\,B$ and $O\,A$, being now measured from O in opposite directions, must have different signs.—Let us now suppose the re-

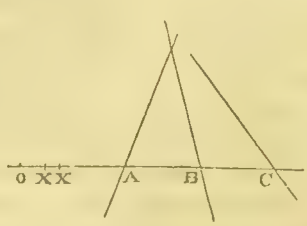


Fig. 6.

volving line to cut *any number* of given right lines in A, B, C, &c., and let $O\,X$ be taken (in the same direction with $O\,A$, $O\,B$, &c.) so that $\frac{1}{O\,X} = \frac{1}{O\,A} + \frac{1}{O\,B} + \frac{1}{O\,C}$ + &c.; the

locus of X *is still a right line.* For if O X′ (see fig.) be taken such that $\frac{1}{OX'} = \frac{1}{OA} + \frac{1}{OB}$ the locus of X′ is a certain right line, which may be considered as replacing the two right lines on which A and B are supposed to move; that is, the original condition now becomes

$$\frac{1}{OX} = \frac{1}{OX'} + \frac{1}{OC} + \&c.$$

If then the number of given lines is three, the locus of X is a right line by the last article; if four, the question is reduced to the case of three by a similar process, and so on for any number. —It cannot easily be rendered clear how very far this simple method may be extended. The general nature of the subject, however, may be gathered from the foregoing cases.

Triangulation. In surveying, where it is necessary to measure many distances, the following process is adopted. A base line is measured with all imaginable precautions. Then some point whose distance we wish to know, if it be visible, (if not, some point in its direction) is taken as the vertex of a triangle of which this line is the base; the angles of the triangle are measured by the theodolite, and the sides calculated. Hence we have two new bases; and we can evidently go on multiplying these to any extent, until we get the successive distances which we desire to measure, as sides of triangles whose base and whose angles are known. The process of measurement of *lines* on the earth is so long, requires so many precautions, and is, after all in many cases so uncertain, that surveyors replace it, as much as possible, by the simple measurement of *angular space.*

Trigonometry. The name given to that portion of mathematics whose object it is to determine the unknown, from adequate known portions of triangles. Plane trigonometry has reference to the case of plane rectilinear triangles. Spherical trigonometry to the triangles formed by the intersection of three great circles on the surface of a sphere. The latter, indeed, should rather take its name from the nature of the problem which it really undertakes to resolve,—viz., to ascertain all the relations incident on the varying intersections of *planes.* The methods and contents of both portions of the subject, must be sought in specific treatises, of which there are now very many of high excellence. The *Arithmetic of Sines*, or analytical trigonometry, as it is called, is an elementary portion of analysis, quite indispensable.

Tropical Year. See CYCLE and CHRONOLOGY.

Tropics. The circles of the earth 23½° on each side of the Equator are called the Tropics. The upper is that of Cancer, the lower that of Capricorn.

Tubular Bridge. See BRIDGE.

Turbine. See WATER WHEEL.

Twilight. The earth as it turns on its axis exposes different parts of its surface successively to the radiation of the sun. The rays of light proceed in straight lines, and if no reflecting or refracting medium interfered in their course, they could only illumine a hemisphere of the globe at a time, the other half being involved in the shadow. The line which separates these divides the day from the night, and, in one of its halves, constitutes the dawn, and in the other the sunset. As the earth rotates on its axis in twenty-four hours, the lines of illumination must pass round the whole circumference in the same time, or at a rate of 15° in the hour. If the sun were always in the prolongation of the plane of the equator, and the earth were stripped of its atmosphere, the days and nights over the whole globe, and at all times, would be equal, and total darkness would instantaneously succeed to the moment of sunset. Instead of this, nature supplies the variety of the seasons and the gentle progression of twilight, the effects of which are so admirable, and their recurrence of such infallible regularity, that no one can be indifferent as to the causes by which they are produced. In fig. 1, let *c* repre-

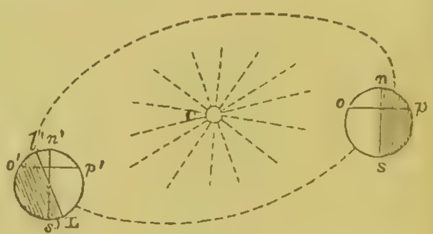


Fig. 1.

sent the sun, and the dotted line surrounding it the earth's orbit. If *s p n o* represent the earth in such a position that its axis *n s* is at right angles to the line extending to the sun, then, as the diagram shows, the circle of illumination would extend through both poles, and any point on the surface of the globe, for instance the point *o*, in its progress of rotation in the circle *o p*, would be, during an equal period of time, in the shadow and in the light, or, in other words, the day and night would be equal, and this would hold over the whole surface. If, however, the axis *s n* remain parallel to itself, while the whole globe advances to another point in the orbit, or to the position shown at *s′ p′ n′ o′*, then the circle of illumination L *l′* does not now reach through both poles, but is inclined to the axis, so that any point, such as *o′*, is not, in its progress round the axis *s n*, an equal period in light and darkness, and the days and nights are unequal. Thus, to the simple contrivance, that the axis of rotation should not be at right angles to the plane of the orbit, are all the varieties of summer and winter mainly due. As has been already mentioned, if the rays of light (which in free space

proceed in straight lines) in their course from the sun along the surface of the earth, encountered no substance capable of changing their direction, daylight would be instantly changed into darkness at sunset, and the glare of day would take the place of the soft growth of the morning. In the explanation of such phenomena, it is more convenient to suppose that the sun moves, and that the earth remains at rest; and as the results for such purposes are the same, this mode may be here adopted. In fig. 2, if A O represent the line

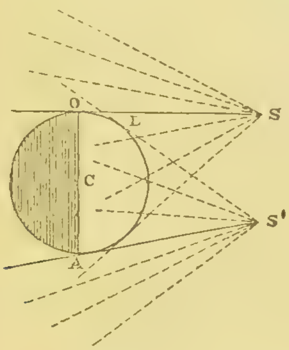


Fig 2.

of illumination on the earth, and S the sun giving out the rays of light, as shown by the dotted lines, then an observer at O would see the sun along the line O S, and apparently resting on the horizon. If the sun descends in the least degree farther as to S′, then no rays could reach the observer at O. The ray S′ L, which is the nearest to him, would graze along the surface at L, and pass away into space. All those in a lower position being totally intercepted, he would be left in darkness. If, however, any substance partially transparent, and yet capable of reflecting light, surrounded the earth, such rays as S′ L might be turned back again, and reach the observer at O, and thus cause a partial illumination of the shadow of night. Such a substance surrounds the earth as its atmosphere, and to it the phenomena of twilight are due. In fig. 3, let

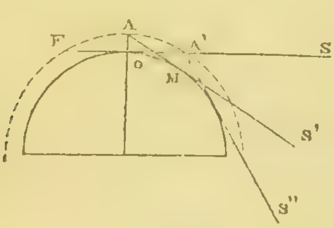


Fig. 3.

O S, as before, represent the horizon of an observer at O, then S would be the lowest position in which the sun could be in order that his rays might reach O; but if the dotted curve indicate the surface of the atmosphere of which A is one of the highest particles, then the sun, after having descended to S′, will still illuminate the air contained in A M, A, all of which is above the horizon of O, and will, therefore, be visible from it, and of course contribute to its illumination by reflection. Even when S has descended to S″, a portion of illuminated atmosphere at A′ will still be visible from O, but beyond this all will be darkness. It is obvious, that the duration of twilight will depend chiefly on the height to which the atmosphere extends above the surface of the earth. The duration of twilight and the height of the atmosphere are so connected together that the one can be deduced from the other, and, indeed, this is the chief means by which the height of the atmosphere has been estimated. The nature of such a calculation may be understood from fig. 4. If A represent the portion of

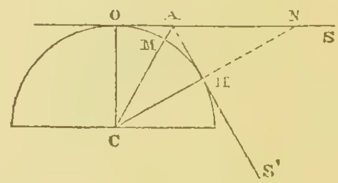


Fig. 4.

atmosphere seen from O to be last illuminated in the western horizon, just as twilight ends, the height M A is that which is to be determined. This will be got if C A be found, as C M is known to be about 4,000 miles—viz., the radius of the earth. Now, in the triangle C O A, C O is 4,000 miles, the angle at O is 90°, and the angle A C O equal to half the angle O C H (the lines O A and H A being tangents applied to the surface, and C A, drawn to their point of contact, necessarily dividing the angle at C into two equal parts because of the symmetry). If, therefore, we get the angle O C H, we get its half, viz., A C O. But, producing C H to N, the triangles N O C and N H A are similar, having each a right angle, and the common angle at N. Hence the angle at C is equal to the angle S A H. But S A H is the depression of the sun below the horizon at the instant twilight ends:—if, therefore, this can be got, the angle O C A, which is equal to its half, is found, and the side C A is computed by the formula,

$$C A = \frac{C O}{\cos O C A}.$$

The depression of the sun below the horizon at the termination of twilight cannot, of course, be directly measured; but it can be certainly inferred, from the well known circumstances of the earth's rotation. The simplest case will be to suppose the observer on the equator, and the sun also in the equinoctial; when it appears to descend vertically on the horizon, and occupies as much time under as above it. As the whole time of passing round the complete circumference of 360° is twenty-four hours, the rate of descent is 15° to the hour. Now, in such circumstances, it is observed that the duration of twilight is an hour and twelve minutes, which corresponds to

18°. Thus it is concluded that the sun must be 18° under the horizon of any place, before the twilight can end. The angle O C A is then 9°, and the computation runs thus—

$$\text{C A} = \frac{4{,}000}{\cos 9} \text{ or } \frac{4{,}000}{987} = 4{,}052{\cdot}6 \text{ miles},$$

then subtracting O C, the earth's radius, or 4,000 miles, we get

$$\text{M A} = 52{\cdot}6 \text{ miles}.$$

It is from such a calculation that the height of the earth's atmosphere is generally said to be forty-five or fifty miles. It is, however, obvious that very great accuracy cannot be attributed to such a method, as it has been taken for granted that the rays of light have been only once reflected from the air. But if we suppose, in fig. 3, that the light which strikes A′ could be thrown on to A to be then again reflected, and so on, it is evident that the sun might descend below S″ without causing the total cessation of twilight. Again, the rays S″ A′ have to penetrate the dense strata of air close to the earth before they reach A, where they are reflected by only an attenuated atmosphere; and they have again to pass down through the dense air close to the surface, along the line A O, before they reach the observer at O —all of which circumstances cause a doubt as to the results of this calculation.—Other methods have been applied to the same problem, particularly those resulting from the observation, not of the solar depression at the termination of twilight, but rather of the solar depression corresponding to the altitudes of what is called the crepuscular and anticrepuscular curves. By inspection of fig. 3, it is apparent that, at the moment of sunset, the whole portion of the atmosphere visible from O, viz., F A A′, is illuminated by direct sunshine; but as the sun descends below the horizon, a part of the air near F will be in shadow, and that, as S descends lower and lower, the shadow will rise higher and higher in the air, appearing with a curved outline, which ascends in the eastern sky. It is called the anticrepuscular curve till it reaches the zenith, when S is at such a position as S′, after which, as the sun sinks still lower, it also sinks towards the western horizon, and is named the crepuscular curve, finishing by totally disappearing as a faint glow close to the earth as twilight terminates. A pure and transparent atmosphere, such as that of the summits of high mountains, is most favourable for the observation of such curves. They have been made the subject of careful observation by Saussure and Lambert, and very recently by MM. Martins and Bravais, in their sojourn at the summit of the Faulhorn. In the *Annuaire Météorologique de France* for 1850 will be found the details of the mode in which they applied their observations to the determination of the height of the atmosphere. Their results are, that the height of the atmosphere is 115,000 metres, and that the crepuscular curve sets when the sun is 17° below the horizon. They also state that under very favourable circumstances, after the first crepuscular curve had set, they could see a whitish light illuminating the sky toward the north-west. It faded so gradually away towards the zenith, that it was impossible to define its boundary. It evidently corresponded to parts of the air for which the crepuscular curve had not set. The light which came from it had suffered a double reflection, as indicated above when referring to the uncertainty of the evidence of the atmospheric limits. Among the most remarkable circumstances connected with the twilight, are its very variable length at the same place at different seasons of the year, and its variable length for places in different latitudes. Both of these are easily explained by a more attentive examination of the circumstances. *First*, with regard to the variable length for the same place at different seasons of the year. It is well known that, at different seasons, the sun attains different altitudes at noon, and descends in a course inclined at different angles to the horizon. In fig. 5, if G S represent the absolute vertical

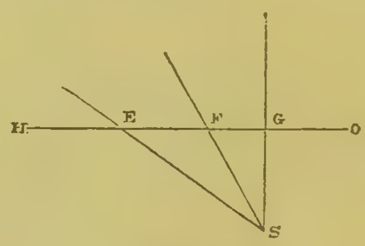


Fig. 5.

depth of 18° below the horizon, which requires to be attained by the sun before twilight can end, then it is obvious that this will be sooner reached by a passage vertically downward (as occur for places at which the sun is vertical at noon), than for cases such as F S or E S, where a much longer course requires to be run before a depth G S can be obtained. It is also obvious that for places at which for any season of the year the sun never descends, even when due north or south, 18° below the horizon, there can be no real night. This is the case with every place for which the least polar distance of the sun is only 18° greater than the latitude. The latitude of Glasgow is 55°..56′, hence, if we add to that 18°, we get 73°..56′. Now, the least polar distance of the sun is at midsummer 67°, so that on the 22d of June the sun only descends 11°..4′ below the horizon. Twilight is never extinguished during the night from the time the polar distance of the sun becomes less than 73°..56′ till it attains it again as the sun proceeds south with the advancing season. The same considerations will easily show that, for places still nearer the poles the twilight will be more and more perfect, till we reach a position where, during the summer, for days and weeks together, the sun never sets—all of which can be readily

understood by reference to the change of the lines of illumination at various positions of the globe, in such a diagram as fig. 1. Setting out from these principles, it is easy to compute the duration of twilight at any place and for any season of the year, by means of the formulæ of Spherical Trigonometry and the co-ordinates of the sun, as supplied by any astronomical ephemeris. Twilight, in all climates and at all seasons of the year, is distinguished and adorned by the rich colour then assumed by the atmosphere. In general, in low countries and in deep valleys, where the moisture of the air is liable to great variations, the tints of colour which succeed to sunrise and sunset are subject to great variety, and can scarcely be said to be reducible to any law or distinct order; but on mountainous elevations where the strata through which the crepuscular curves are seen is more uniform in composition, the tints observable succeed each other with far greater regularity. Thus, on the Faulhorn, M. Bravais observed the following changes of colour as the sun, in rising, approached the horizon. The zenith-distance greater than 90° signifies that the sun had not yet risen. 1st, The zenith-distance of the sun is 102°. In the east, a red or orange band, of which the altitude is 0°; the height of the crepuscular curve is 70°. The space comprised between these two arcs is of a bluish-white, clearer than the remainder, of the sky. 2d, The zenith-distance of the sun is 98°. The part of the sky from the horizon to 1°..15′ altitude, is red; above this a yellow tint prevails up to 3°..10′. The green begins to show above this, and extends to 5° in height; still higher a feeble blue shade appears, extending to a height of 25°, farther than which the twilight has not yet reached. 3d, Zenith-distance of the sun 96°. The elevation of the orange and yellow zones has not changed. The green tint prevails to a height of 7°. The crepuscular curve has reached an altitude of 70°. 4th, The zenith distance of the sun is 94°. The yellow and orange bands remain at the same height. The green zone extends to 12°; above which a purple tint commences to show itself, in favourable circumstances. This purple tint never forms till the sun is within 5° of rising, and never after it is within 3° of rising. It attains its maximum intensity at 25° in height. 5th, The zenith-distance of the sun is 92°. The eastern red commences to become yellow. The superior limit of the yellow zone is always 3°..15″. From this to 18° in height a green colour of considerable intensity prevails. In the western horizon the anticrepuscular curve has reached to within 3° of the horizon, and the colour is red from a height of 3° up to 15°. The science of Optics has not yet sufficiently advanced to be able to render a complete account of all these colours, and of the reasons why each should be limited to a certain altitude. There is no doubt, however, but that the principal cause of these tints is to be found in the fact that the differently coloured rays of which white light is composed, have different powers of penetrating the atmosphere; the most refrangible, such as the violet and blue, being most easily extinguished, and the red having even a greater power of penetration than the yellow. That this is true, is proved by the simplest experiment with a piece of smoked glass. A thin layer of smoke first extinguishes the blue rays, hence the transmitted light is yellowish-red—a greater thickness extinguishes most of the yellow, and the rays which penetrate appear orange. If still a deeper deposit is interposed, the red rays alone can penetrate. The simplest observation on the solar disc, as seen through the smoky atmosphere of a city, amply illustrates the same piece of theory. It is, then, to the unequal powers of penetration of the solar rays, as they pass through the densely vaporous strata of the lower atmosphere, that the glories of twilight are chiefly due. The blue of the mid-day sky is the colour which has been kept back by the air from the rays which at the same instant are garnishing the sky of a distant region with the splendid hues of sunset.

Twinkling of the Stars—Scintillation. The twinkling of the stars need not be described. It seldom occurs, however, unless in the case of a *fixed star*—that is of a *mere luminous point.* Orbs that have apparent discs, such as the large planets, twinkle very rarely, if, indeed, they ever do so. The explanation of the phenomenon was first given by Arago. It is this:—Rays of light coming from a point and traversing an atmosphere composed of strata unequally hot, dense, and humid, must travel with unequal velocities; and if these rays be collected into the focus of a lens, as they are by the lens of the eye, we shall see the results of their reaching us in *different phases*, by an alternately brightening and darkening of the image in that focus. It is sufficiently clear that such scintillation can only take place if the rays issue from a mere point. If they issue from a disc, *i.e.*, from many neighbouring points, the effects would conflict, and on the whole be null. This explains also why the very faintest stars are visible only through their twinkling. Their brightest phase is *a phase of double light*; and they may be seen in this phase, although in their medium state they would have always remained invisible.

Tychonic System. The astronomical system of Tycho Brahe. It possesses now an interest merely historical.

Typhoon. See WINDS.

Umbra. See PENUMBRA and ECLIPSE.

Undulatory Theory. The references to the theory now named, and the expositions of its capabilities already made in this Cyclopædia, are so numerous and full, that nothing remains as subject-matter for the present article except a general summary. The Undulatory Theory in its largest acceptation is as follows:—*The influences transmitted by a solar beam, or probably by any source of radiant light or heat or chemical power, are due to undulations in an ethereal medium, propagated from that source with great velocity,—such undulations or vibrations going through their wave-phases in different and, in all cases, infinitesimal although measurable periods of time.* The nature of the impression made by these vibrations depends on their rapidity. Vibrations of the ether, of a velocity included between 400 millions of millions and 800 millions of millions per second, contain the entire sphere within which the sensation of light or colour issues as a consequence. Slower vibrations produce the sensation of heat merely; and quicker vibrations yield only chemical power. Of course the three effects intermingle—the middle light-rays possessing all the three efficiencies. See SPECTRUM. The identification of these three apparently very different agencies, may not probably be reckoned complete as yet; nevertheless no hesitation need be felt in averring that the view now given is in every respect the most probable one. The practical assimilation of the rays of invisible heat, with the rays of heat that are also light-giving, is due to Melloni, Knoblauch, and many other inquirers; nor must mention be omitted of the labours of Professor J. D. Forbes, whose recent indifferent health, physical science has great reason to regret. Every inquiry of importance connected with the habitudes of such undulations and the general bearings of the theory, having its analogue in connection with the *Luminous Spectrum*, we shall in this place merely review the *Undulatory Theory of* LIGHT. This—perhaps the most remarkable speculation in modern physics—may be regarded under two aspects; *first*, in reference to its power as a *Geometrical* or abstract method of comprehending phenomena; and *secondly*, as a *Physical* Theory.

I. THE UNDULATORY THEORY VIEWED GEOMETRICALLY.—In order that the student apprehend the true geometrical character of this theory, he must hold present to his mind several distinct and very important points.

(1.) *General Principles of Wave Propagation.*—The general nature of propagation by undulatory movements has been explained under WAVES, and elsewhere in our pages. The propagation of a Wave, as we scarcely require to remark, is essentially different from the motions of the particles constituting the Wave. The waves of sound, for instance, or the pulsations of the atmosphere that produce sound, depend for the velocity of their propagation on the elasticity of that fluid, while the waves themselves are a succession of compressions and dilations, in which each particle moves to and fro within a varying but determinate range. In most liquid waves again, there is no *onward* motion of the liquid particles whatever—they merely rise and fall, or oscillate within a determinate space during a determinate time. The space through which a molecule rises and falls, or moves to and fro, is the *amplitude* of its vibration. On the other hand, an assemblage of vibrating molecules in all phases of a vibration, (no phase being repeated), is an *ethereal wave*, and the length of such an assemblage is generally represented by λ. It is clear that the velocity of propagation need not depend either on the amplitude or the quantity λ: nay, it may be conceived, that a great number of such waves—differing in their elements—may co-exist, or form a *sheaf* or concurring mass of waves, and still be propagated with the same velocity. In reference to a large class of optical phenomena and laws, no question need be started as to the nature of these light-waves. The simple conception of propagation by undulation, involves the necessity of the laws of REFLEXION, REFRACTION, INTERFERENCE, and several other important affections to which Radiant Light is subject.—The only important puzzle as to the general theory of propagation by waves has recently been triumphantly removed. It is physically impossible to conceive of waves diverging from, or converging to, a *mathematical* or *pure focal* point. But Professor Stokes has conclusively shown that waves spreading from an origin of disturbance of finite magnitude and any form, sensibly agree in all respects with waves spreading from a pure focal point, so soon as they have obtained a large distance from the focal space. So that the equations resulting from the conception of absolute focal points may be applied without sensible error in all those cases in which it is usual to apply them.

(2.) *The general Wave of Light is composite, or rather an aggregate of many Waves.*—The general light-wave cannot be deemed homogeneous. The phenomena of colour, as evolved by THIN PLATES, in DIFFRACTION, or the INTERFERENCE of common light, and above all in DISPERSION by the Prism when a *Spectrum* is formed, force upon Theory the new notion, that the light-wave is *composite*. As already indicated in separate parts of our volume, this composite or sheaf can be analyzed, and its different waves separated. The result of this analysis, in connection with the colours, whose sensations are excited by the various waves, is presented in the following table:—

Colours.	Length of an Undulation in parts of an inch in air.	Number of Undulations in an Inch.	Number of Undulations in a second.	
Extreme,	·0000266	37640	458	millions of millions.
Red,	·0000256	39180	477	"
Intermediate,	·0000246	40720	495	"
Orange,	·0000240	41610	506	"
Intermediate,	·0000235	42510	517	"
Yellow,	·0000227	44000	535	"
Intermediate,	·0000219	45600	555	"
Green,	·0000211	47460	577	"
Intermediate,	·0000203	49320	600	"
Blue,	·0000196	51110	622	"
Intermediate,	0000189	52910	644	"
Indigo,	·0000185	54070	658	"
Intermediate,	·0000181	55240	572	"
Violet,	.0000174	57490	699	"
Extreme,	·0000167	59750	727	"

The difficulty of a theory of *Dispersion*, in harmony with the Undulatory Hypothesis, is now entirely resolved. If the velocity of propagation depended solely on the elasticity of the ethereal medium, as originally supposed, all these composite waves would of course be propagated with the same velocity, and there could be no Dispersion. But M. Cauchy, taking a larger and more precise view of the conditions affecting the propagation of waves in a medium like that demanded by the Undulatory Theory, showed that the velocity of propagation ω, of any wave, is determined by the following formula:—

$$\omega^2 = a_1 + a_2\, k^2 + a_4\, k^4 +, \&c.,$$

where $k = \frac{2\,\pi}{\lambda}$. In consequence of this dependence of ω upon k, there must be Dispersion; and the *colour* of any portion of the spectrum, is thus indissolubly connected with its *refrangibility.* The modification introduced by M. Cauchy, as the ground of the foregoing conclusion, was not a specialty, but an enlargement or fuller view of the habitudes of elastic media. As we have explained under LIGHT, he simply dismisses the convenient geometrical fiction, that "*the sphere of the action, or of the vibration of the separate molecules, is infinitely small compared with the length of the wave.*"

(3.) *Nature or Direction of the Vibrations constituting the Light-wave.*—Are these vibrations *longitudinal*, like those of the waves of *sound*, or are they *transversal*, like those of a vibrating musical string, or the oscillations of a particle in a liquid wave? A question started by the phenomenon of POLARIZATION, and as we have shown under that article, satisfactorily resolved. Polarization, or the presentation of *sides*, by any ray or set of waves, is impossible, unless the vibrations constituting these waves are *transverse.* There can be *Reflexion*, *Refraction*, *Interference*, and *Dispersion* of *Sound;* but *Polarization* of *Sound* is a physical impossibility. The waves of light then, must consist of vibrations executed by the particles of the ethereal medium, *transversely to the direction of the propagation of these waves.* Another inquiry however, of much more delicacy, and concerning which even Cauchy has held different opinions, remained to be exhausted. A plane-polarized wave is distinguished from the wave of common light in this:—The vibration of the molecules which produce it, are, of course, transversal, like those of common light: but further, they take place *in one plane*, or may be represented as doing so. Now what is that plane? Is it indifferent, or is there any definite or fixed plane, to which we may refer it, by usual mathematical symbols? The only fixed plane indicated by the phenomenon of Polarization, is what is rightly termed the *plane of polarization;* a plane determined as follows:—In the case of a ray of light polarized by reflexion, that plane is the plane along which the polarized light is reflected; but a more general, although quite equivalent definition is this,—When a polarized ray is extinguished by a tourmaline plate, the plane *parallel to the axis of this plate*, is termed the *plane of polarization;* when, on the contrary, the ray seen through the tourmaline plate has its maximum of intensity, the plane of polarization is perpendicular to the axis of the plate. What relation then has the plane in which the ethereal molecules vibrate, with this determinate plane? At an early period of his researches, Cauchy considered the two planes parallel, or rather one and the same; and the lamented M'Cullagh of Dublin, as well as Mr. Green, held the same view. Cauchy, however, has returned to the opinion of Fresnel, viz., that the planes are *perpendicular* to each other; and this supposition promises to be a secure guide, as we thread the labyrinth of many still obscure phenomena, especially those of elliptic polarization by reflexion. It cannot indeed be now held as a mere assumption. Professor Stokes (see DIFFRACTION in Appendix) has devised and executed an experiment which must be termed a *crucial* one, clearly indicating the perpendicularity of the plane of vibration. The doctrine in question may thus be held as the new and most recent fact regarding the undulations that determine the phenomena of Light.

(4.) *Other special habitudes of the Light-wave, and of the Elastic Ether or Medium.*—Difficulties, however, and those of no light order, still remain and are brought under notice in a manner quite similar to the procedures that revealed "*Perturbations*" amid the celestial mechanism. The non-existence of an *absolute* angle of polarization, or rather its identity with an angle of *maximum* polarization, as well as the entire phenomena of the resolution by reflexion of a plane-polarized ray or of a ray of common light, into rays circularly or elliptically polarized—(the plane-polarized ray being merely that case of elliptically polarized light in which

the minor axis of the ellipse is, to the senses, indefinitely small), — these and other phenomena demand the introduction of other hypotheses in order that they may be geometrically resolved. It has become necessary, therefore, to fall back, to some extent, on molecular physics, and to inquire into the accidents that may occur, in consequence of certain disturbances in that elastic medium whose general affections have already been defined. These most recent and subtle modifications of the former rather confined theory, are due to M. Cauchy; although, in one very important point the truth was discerned by our excellent Green, and the doctrine, in other respects—although inadequately —carried out by another inquirer of truest genius, M'Cullagh. Cauchy soon pierced beyond these more general actions and modifications of the ethereal medium. He saw, stated, and calculated the effects of two grand truths. *First*, that—on the *disturbance* of any transverse wave, propagated through an elastic medium of any nature—there must be generated, along with the transverse wave, a *normal* wave, or a wave whose direction is perpendicular to the face of the wave. Mr. Green, along with M. Cauchy, attributes to this normal wave the effect of those alterations of phase in the two oppositely polarized waves constituting a common ray, which give rise to the class of phenomena designated as *elliptical polarization*. *Secondly*, the *intensities* of the reflected beams, on the other hand, are, as M. Jamin has shown, deducible from other formulæ by M. Cauchy, in which the effects of *extinction* by the media, are held in account.—Nothing, at present, is more earnestly to be desired by students of physical optics, than that M. Cauchy should devote his leisure to the production of a full and continuous view of Molecular Physics, as connected with the Undulatory Theory of Light. No other inquirer could do this so well; no living man has so thorough a right to assert that by his formulæ he has established the laws of phenomena discerned since these formulæ were produced, —formulæ rested on pure *à priori* grounds. Cauchy states that several theoretical consequences of the Undulatory Theory of light may still be verified: for instance, he avers that the normal wave of Green, which is also at the root of many of his own conclusions, must be distinctly and individually visible, so soon as we obtain telescopic and microscopic powers that will enable us to discern the diameter or true disc of SIRIUS!—In the meantime, a geometrical difficulty of another kind presses itself on notice. According to all conceptions hitherto entertained, the velocity of a wave on entering a Medium whose elasticities vary according to direction—say a crystal with more than one geometrical axis—ought to depend on the *direction of propagation*. But according to the theory of light, which has been confirmed by experiment—the theory, viz., of transverse perpendicular vibrations—the velocity of the ray within that medium must depend solely on the direction of the transversal characteristic of the movement propagated. The difficulty is a serious one, and ought to be grappled with. It has been suggested that the *elasticity* of the luminiferous medium may be the same in all directions and in all substances; but that the *inertia* of the mass set in motion may be different in different substances, and in doubly-refracting bodies, different also for different directions of the transversal characteristic of the movement propagated.

II. THE UNDULATORY THEORY VIEWED PHYSICALLY.—What is this *Ether*, and how are these geometrical undulations really produced? Are they more than abstract or *Mathematical Entities*, indicative perhaps, in its highest empirical form, of some grand ultimate Law? It cannot be denied that great physical difficulties exist, in the way of imagining the reality of an Ether, such as these geometrical exigencies demand. It must be a substance, constituted to resist almost entirely, longitudinal disturbance; for, notwithstanding the normal evanescent waves of Green and Cauchy, it is clear that transversal vibrations must proceed within it, of immense velocity, and be propagated with great ease, while longitudinal vibrations are scarcely propagated at all. How can an ether of this sort be fancied, in consistency with the comparatively free movements of the Planets? Encke's comet indeed indicates the reality of a resisting medium; but a medium of the kind of which we have been speaking, ought, if our existing physics are correct, to have made its substantiality manifest by other and indisputable tokens. Without departing, however, from the general foundations of this Theory, other views of its physical cause may be entertained: in illustration of the possibility of which we give the substance of a modification very recently proposed—that, viz., which Professor Rankine has denominated an *Oscillatory Theory*. The subjoined extract is from a paper read before the British Association at Hull, in 1853:—"The hypothesis now to be proposed as a groundwork for the undulatory theory of light, consists mainly in conceiving that the luminiferous medium is constituted of detached atoms or nuclei distributed throughout all space, and endowed with a peculiar species of polarity, in virtue of which three orthogonal axes in each atom tend to place themselves parallel respectively to the corresponding axes in every other atom; and that plane-polarized light consists in a small oscillatory movement of each atom round an axis transverse to the direction of propagation. Such a movement would be transmitted through such a medium with a velocity proportional,—directly, to the square root of the total rotative force exercised by the luminiferous

atoms in a given small space, upon those in a given adjacent small space lying in the direction of propagation, in consequence of a given amount of relative angular displacement round the axis of oscillation ;—and inversely, to the square root of the sum of the moments of inertia round the axes of oscillation of the atoms contained in a given space, loaded with such portions of molecular atmospheres surrounding them as they may carry along with them in their oscillations. Then denoting by h, the velocity of transmission in a given direction of plane-waves of oscillation round transverse axes parallel to a given line; C, a co-efficient of polarity or rotative force for the given directions of propagation and of axes; M, a co-efficient of moment of inertia for the given direction of axes; the above principle may be represented by this equation,

$$h^2 = \frac{C}{M}.$$

The co-efficient of polarity in question is proper only to an axis of oscillation transverse to the direction of propagation. To account for the stability of direction of the axes of the atoms, and also for the non-appearance, in ordinary cases, of phenomena capable of being ascribed to oscillations round axes parallel to the direction of propagation, it is necessary to suppose the corresponding co-efficient for the latter species of oscillations to be much greater than the co-efficient for transverse axes of oscillation.—It is evident, that how powerful soever the polarity may be, which is here ascribed to the atoms of the luminiferous medium, it is a kind of force which must be absolutely destitute of direct influence on resistance to change of volume or change of figure in the parts of that medium, or of any body of which that medium may form part; and that, consequently, the difficulty which in the hypothesis of vibrations arises from the necessity of ascribing to the luminiferous medium properties like those of an elastic solid, has no existence in the hypothesis of oscillations now proposed. The luminiferous atoms may now be supposed to be diffused throughout all space, and as molecular nuclei, throughout all bodies; the distribution and motion of their centres being regulated by forces wholly independent of that species of polarity which is the means of transmitting a state of oscillation round those centres.

"(3.) *Of the Diffraction of Plane-polarized Light, and the relation of Axes of Oscillation to Planes of Polarization.*—In the diffraction of an oscillatory movement round transverse axes past the edge of an obstacle, a law holds good exactly analogous to that demonstrated by Professor Stokes for a transverse vibratory movement, substituting only the axis of oscillation for the direction of vibration; that is to say,—*The direction of the axes of oscillation in the diffracted wave is the projection of that of the axes of oscillation in the incident wave on a plane tangent to the front of the diffracted wave.*—Consequently, oscillations in the incident wave, round axes oblique to the diffracting edge, give rise to oscillations in the diffracted wave round axes *more nearly parallel* to the diffracting edge. But the experiments of Professor Stokes have proved, that light polarized in a plane oblique to the diffracting edge, becomes, after diffraction, polarized in a plane *more nearly perpendicular* to the diffracting edge.—Therefore the axes of oscillation in plane-polarized light are perpendicular to the plane of polarization.—Therefore the velocity of transmission of oscillations round transverse axes through the luminiferous medium in a crystalline body is a function simply of the direction of the axes of oscillation.—Now if the variations of the velocity of transmission arose from variations of the co-efficient of transverse polarity (denoted by C), they would depend on the direction of propagation as well as upon that of the axes of oscillation, so that the plane of polarization would be that which contains these two directions. Since the velocity of transmission depends on the direction of the axes of oscillation only, it follows that its variations in a given crystalline medium arise wholly from variations of the moment of inertia of the luminiferous atoms, together with their loads of extraneous matter.—Consequently the co-efficient of polarity C for transverse axes of oscillation is the same for all directions in a given substance.—To account for the known laws of the intensity and phase of reflected and refracted light consistently with the hypothesis of oscillations, it is necessary to suppose also that this co-efficient is the same for all substances; so that the variations of the velocities of light and indices of refraction for different media depend solely on those of the moments of inertia of the loaded luminiferous atoms.

"(4.) *Of the Wave-surface in Crystalline Bodies.*—Let the axes of co-ordinates be those of molecular symmetry in a crystalline medium. Let M_1, M_2, M_3 be co-efficients proportional to the moments of inertia of the luminiferous atoms with their loads of extraneous matter, round axes parallel to x, y, z respectively. Let r be a radius vector of the diverging wave-surface in the direction (α, β, γ). Then the equation of that surface for polar co-ordinates is

$$\frac{1}{r^4} - \frac{1}{r^2} \cdot \frac{1}{C} \Big\{ (M_2 + M_3) \cos^2\alpha + (M_3 + M_1) \cos^2\beta + (M_1 + M_2) \cos^2\gamma \Big\} + \frac{1}{C^2} \Big\{ M_2 M_3 \cos^2\alpha + M_3 M_2 \cos^2\beta + M_1 M_2 \cos^2\gamma \Big\} = 0;$$

and for rectangular co-ordinates,

$$\frac{1}{c^2}(x^2+y^2+z^2)\cdot(M_2 M_3 x^2 + M_3 M_1 y^2$$

$$+ M_1 M_2 z^2) - \frac{1}{c}\Big\{(M_2+M_3)x^2 +$$

$$(M_3+M_1)y^2 + (M_1+M_2)x^2\Big\} + 1 = 0.$$

The above equations are exactly those of Fresnel's wave-surface, with the following semi-axes:—

$$\text{Directions}\left\{\begin{array}{lll} & & \text{Semi-axes.} \\ x, & \sqrt{\frac{C}{M_2}}, & \sqrt{\frac{C}{M_3}}; \\ y, & \sqrt{\frac{C}{M_3}}, & \sqrt{\frac{C}{M_1}}; \\ z, & \sqrt{\frac{C}{M_1}}, & \sqrt{\frac{C}{M_2}}; \end{array}\right.$$

the squares of the semi-axes of the wave-surface along each axis of co-ordinates being inversely proportional to the moments of inertia of the loaded luminiferous atoms in a given space round the other two axes of co-ordinates. The plane of polarization at each point of the wave-surface is perpendicular to the direction of greatest declivity.—The equation of the index surface, whose radius in any direction is inversely proportional to the normal velocity of the wave, is formed from that of the wave-surface by substituting respectively,

$$C, \quad \frac{1}{M_1}, \quad \frac{1}{M_2}, \quad \frac{1}{M_3},$$

for

$$\frac{1}{C} \quad M_1, \quad M_2, \quad M_3.$$

These equations are obtained on the supposition that the co-efficient of polarity for axes of oscillation parallel to the direction of propagation (which we may call A) is either very large or very small compared with that for transverse axes. By treating the ratio of these quantities as finite, there is obtained an equation of the sixth order, representing a wave-surface of three sheets, differing somewhat from that of the propagation of vibrations in an elastic crystalline solid; inasmuch as the former has always three circular sections, while the latter has none, unless it is symmetrical all round one axis at least. By increasing the ratio $\frac{A}{C}$ without limit, this equation is made to approximate indefinitely to the product of the equation of Fresnel's wave-surface by the following,

$$\frac{M_1}{A}x^2 + \frac{M_2}{A}y^2 + \frac{M_3}{A}z^2 = 1;$$

which represents a very large ellipsoidal wave of oscillations round axes parallel to the direction of propagation.

UND

"(5.) *Of Reflexion and Refraction.*—According to the proposed hypothesis of oscillations, the laws of the phase and intensity of light reflected and refracted at the bounding surface of two transparent substances are to be determined by conditions analogous to those employed in the hypothesis of vibrations by M. Cauchy and Mr. Green. They are the consequences of the principle, that if we have two sets of formulæ expressing the nature and magnitude of the oscillations in the two substances respectively, then either of those formulæ, being applied to a particle at the bounding surface, ought to give the same results.—According to this principle, the following six quantities for a particle at the bounding surface must be the same at every instant, when computed by either of the two sets of formulæ:—The three angular displacements round the three axes of co-ordinates.—The three rotative forces round the same three axes.—There is, generally speaking, a change of phase when light undergoes refraction or reflexion. It is known that we may express this change of phase by subdividing each reflected or refracted disturbance into two, of suitable intensities and signs; one synchronous in phase with the corresponding incident disturbance, and the other retarded by a quarter of an undulation. There are thus twelve quantities to be found, viz., the amplitudes of the six components of the reflected disturbance, and those of the six components of the refracted disturbance. To determine these quantities there are twelve conditions, viz., the equality at every instant, according to the formulæ for either medium, of the total angular displacements, and of the total rotative forces, round each of the three axes of co-ordinates, for the set of waves composed of the incident wave and those synchronous with it, and for the set of waves retarded by one quarter of an undulation.—The results of these conditions have been investigated in detail for singly refracting substances.—The indices of refraction of such substances are proportional to the square roots of the moments of inertia of the loaded luminiferous atoms in a given space. Thus, if the co-efficients M', M'' are proportional to these moments in two given substances respectively, then the index of refraction of the second substance relatively to the first is

$$\mu = \sqrt{\frac{M''}{M'}}.$$

—In the case of light incident on a plane surface between two such media, the axes of co-ordinates may be assumed respectively perpendicular to the reflecting surface, perpendicular to the plane of reflexion, and along the intersection of those two planes; and oscillations round axes normal and parallel to the plane of reflexion may be considered separately.—When the axes of oscillation are normal to the plane of reflexion, that is to say, when the light is polar-

ized in that plane, the formulæ for the intensities of the reflected and refracted light agree exactly with those of Fresnel. When the reflexion takes place in the rarer medium, the reflected light is retarded by half an undulation; when in the denser, there is no change of phase unless the reflexion is total, when there is a certain acceleration of phase depending on the angle of incidence. In the last case, the disturbance in the second medium is an *evanescent wave*, analogous to those introduced into the vibratory theory by M. Cauchy and Mr. Green; that is to say, a wave in which the amplitude of oscillation diminishes in proportion to an exponential function of the distance from the bounding surface (called by M. Cauchy the *modulus*), and which travels along that surface with a velocity less than the velocity of an ordinary wave; the square of the negative exponent of the modulus being proportional to the difference of the squares of those velocities, divided by the square of the velocity of an ordinary wave.—This is an evanescent wave of oscillation round transverse axes.—How large soever the co-efficient of polarity for oscillations round longitudinal axes may be, an evanescent wave of such oscillations may travel along the bounding surface of a medium with any velocity, however slow, provided the negative exponent of the modulus is made large enough. Consequently, in framing the formulæ to represent oscillations round axes parallel to the plane of incidence, we must introduce in each medium two such evanescent waves of suitable exponents and indeterminate amplitudes; one travelling along the surface with the incident wave, and the other a quarter of an undulation behind it. The maximum amplitudes of oscillation in these evanescent waves constitute four unknown quantities; the amplitudes in the two ordinary reflected waves and the two ordinary refracted waves, differing by one quarter of an undulation, constitute four more unknown quantities, making eight in all: four conditions having been fulfilled by the waves polarized in the plane of incidence, there remain to be fulfilled eight conditions, viz., the identity, as calculated by the formulæ for the first and second substance respectively, of the following eight functions at the bounding surface; the angular displacement, and the rotative forces, round each of the two axes in the plane of incidence, for the incident wave and the set of waves synchronous with it, and for the set of waves retarded by one quarter of an undulation. These conditions are sufficient to determine the unknown quantities, and to complete the solution of the problem. The following is a general statement of the results of the solution when the second medium is the denser. They agree with the results of the experiments of M. Jamin, and are in every respect analogous to those deduced from the hypothesis of vibrations by M. Cauchy, Mr. Green, and Mr.

Haughton.—Light polarized in a plane perpendicular to the plane of incidence, suffers by reflexion at a perpendicular incidence no alteration of phase.—At a grazing incidence (or when the angle of incidence differs insensibly from 90°), the phase, like that of light polarized in the plane of incidence, is retarded by half an undulation.—The variation of phase with the angle of incidence is, in fact, continuous; but it is, generally speaking, not appreciable by observation, except in the immediate neighbourhood of an angle called by M. Jamin the *principal incidence*, where the retardation of phase is a quarter of an undulation.—This angle differs by a very small amount, appreciable only in certain substances, from the *polarizing angle*, at which the intensity of light polarized in a plane at right angles to the plane of incidence is a minimum.—The "Law of Brewster," that *the tangent of the polarizing angle is equal to the index of refraction*, is, theoretically, only approximately true; but the error is quite inappreciable.—When the second medium is the less dense, the phase of the reflected light is half an undulation in advance of its value when the second medium is the denser.—In either case, light polarized in planes perpendicular to the plane of incidence is less retarded, that is to say, is accelerated in phase, as compared with light polarized in that plane according to the following table:—

	Angle of Incidence.	Relative Acceleration.
	°	
Perpendicular incidence, .	0	$\frac{1}{2}$ undulation.
Principal incidence, . .	...	$\frac{1}{4}$ undulation.
Grazing incidence, . .	90	0

In the case of total reflexion, light polarized in planes perpendicular to the plane of incidence has its phase more accelerated than light polarized in that plane, by an amount to which the formulæ of Fresnel give a close approximation.—The proposed hypothesis has not yet been applied to reflexion from doubly refracting crystals; but there can be little doubt that it will be found to represent the phenomena correctly.

"(6.) *Of Circular and Elliptic Polarization.*—Light polarized in a plane oblique to the angle of incidence is, generally speaking, elliptically polarized after reflexion, the plane-polarized components of the disturbance being in different phases.—According to the hypothesis of oscillations, circularly and elliptically polarized light, being compounded of oscillations in different phases round two transverse axes, consist in a sort of *nutation* of the longitudinal axis of each luminiferous atom. The direction of this nutation, and the form of the circle or ellipse described by the ends of the longitudinal axes, serve to define the character of the light. The ellipse of nutation has its axes in the same proportion with, but perpendicular in position to, those of the elliptic orbit supposed to be described by each atom according to the hypothesis of vibra-

tions.—The molecular mechanism by which certain media transmit right and left-handed circularly or elliptically polarized light with different velocities, is still problematical according to either hypothesis. The laws of the phenomena, however, may be represented by means of the assumption, that in the substances in question the extraneous load on the luminiferous atoms is a function of the direction of rotation.

"(7.) *Of Dispersion.*—If we assume the extent of sensible direct action of the polarity of the luminiferous atoms to be appreciable as compared with the length of a wave, the velocity of propagation (precisely as with the vibratory hypothesis) is found to consist of a constant quantity, diminished by the sum of a series in terms of the reciprocal of the square of the length of a wave.—It may be doubted, however, whether this supposition is of itself adequate to explain the phenomena of dispersion; and whether it may not be necessary to assume, also, that the load upon the luminiferous atoms is a function of the time of oscillation, as well as of the nature of the substance and the position of the axes of oscillation.—In conclusion, it may be affirmed, that, as a mathematical system, the proposed theory of oscillations round axes represents the laws of all the phenomena which have hitherto been reduced to theoretical principles, as well, at least, as the existing theory of vibrations; while as a physical hypothesis, it is free from the principal objections to which the hypothesis of vibrations is liable."

Universal Instrument. See Appendix.

Urania. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Uranus. The planet immediately beyond Saturn in the order of distances from the Sun, discovered accidentally by Sir William Herschel on 13th March, 1781. It turned out afterwards that the planet had been seen, and its place recorded no fewer than fifteen times between 1690 and 1771; but in every instance it was mistaken for a fixed star. The mean distance of Uranus from the Sun is 19·18229 times the distance of the Earth: it revolves in its orbit in 84 years, 5 days, 19 hours, 41 minutes, and 19 seconds: it rotates on its axis in 9½ hours: its diameter is 345,000 miles, and its density 0·18 that of the Earth. The remarkable part played by the apparent irregularities of Uranus in promoting the discovery of NEPTUNE (*q. v.*), has already been fully explained. There is much that is interesting in reference to the satellites of this planet. Sir William Herschel left on record that he had discovered SIX. Two of these only have with certainty been re-discovered, and Mr. Lassel has seen other two, with periods so little reconcilable with any of Herschel's six, that the probability is, they are additional ones. In this case Uranus must have eight satellites, for there is no great likelihood that Sir William Herschel committed a mistake on such a point. Four satellites have therefore still to be looked for.—These small bodies appear to have retrograde motions, and their orbits are inclined nearly 80° to the ecliptic. Of the physical aspects of Uranus, nothing whatever is known.

Ursa Major, and Minor. Two of the most remarkable northern constellations. The latter contains the pole star. In the former there are seven well known stars, by two of which—the pointers—the pole star is readily found. They are disposed in the form of a quadrangle joined at one of its corners to a triangle, and are all of them very large. A line through the pointers passes through the *pole star*. This star is the largest in Ursa Minor;—seven stars in the latter are arranged very like those of Ursa Major.

V

Vacuum. A perfect vacuum or space void of all matter seems not to exist, upon the hypothesis of the luminiferous ether and the retarding matter in the case of Encke's Comet. We can produce no perfect vacuum. In the air-pump receiver the approximation is very rough, and in the Torricellian vacuum (over the mercury at the top of a barometer), there is mercurial vapour.

Valve. An arrangement by which air or any fluid may be alternately admitted into and expelled from a vessel. We cannot enter here into the arrangements by which the mechanist secures the more and more perfect performance of these functions.

Vanishing Point and Line. See PERSPECTIVE.

Vapour is any substance in the gaseous condition, at the maximum of density consistent with that condition. This is the strict and proper meaning of the word "Vapour." It is sometimes used in an extended sense, identical with that of "gas," in speaking of substances whose ordinary condition is the liquid or solid; but this extended sense tends to ambiguity, and will not be used in the present article. It is certain that most substances are *volatile*, that is to say, that they can and do exist in the state of vapour, at all attainable temperatures. Many vapours, whose existence cannot be proved by mechanical or chemical processes, are obvious to the sense of smell; for example, those of iron, copper, lead, and tin. Whether *all* substances are volatile at all temperatures is yet uncertain. If there be cases of exception, it is to be understood that the laws stated in the sequel of this article do not apply to them.

§ 1. *Pressure and Density of Vapour.*—For each volatile substance, at each temperature, there is

a certain pressure which is at once the least pressure under which the substance can exist in the liquid or solid state, and the greatest pressure which it can sustain in the gaseous state at the given temperature. This pressure is called the *pressure of saturation*, or the *pressure of vapour* of the given substance at the given temperature; this pressure is a function of the temperature, and the density of the vapour is a function of the pressure and the temperature. The relation between the pressure of vapour and the temperature, for various substances, has been the subject of many series of experiments, of which the latest and best are those of M. Regnault on steam (*Memoires de l'Academie des Sciences*, 1847), and on various other vapours (*Comptes Rendus*, 1854.) The best sources of information as to the pressures of vapours are the tables computed by M. Regnault from those experiments; but such pressures may also be computed in most cases with great accuracy by the aid of a formula, which, with the constants applicable to vapours, as deduced from M. Regnault's experiments, is given in HEAT, MECHANICAL ACTION OF, § 19, and more fully in the *Edinburgh Philosophical Journal*, July, 1849, and the *Philosophical Magazine*, Dec., 1854. The general result of such formulæ and tables is, that the pressure of vapour increases with the temperature at a rate which itself increases rapidly with the temperature. If any vapour were a perfect gas, its *density* D_1, at any temperature T_1, might easily be computed, when its density D_0, at some other temperature T_0, had been ascertained by experiment, by means of the formula

$$(1.) \ldots \frac{D_1 (T_1 + 461^\circ.2 \text{ Fahr.})}{P_1} = \frac{D_0 (T_0 + 461^\circ.2 \text{ Fahr.})}{P_0},$$

in which P_1 and P_0 are the pressures of the vapour at the temperatures T_1 and T_0 respectively; but no vapour is an absolutely perfect gas; and the density of every vapour increases more rapidly with increase of pressure than that which would be given by the above formula. That formula, however, is sufficiently near the truth for practical purposes when the density of the vapour is below certain limits, as is the case with the vapours of most substances at the temperatures which usually occur in the atmosphere. The experimental determination of the densities of vapours, to a certain rough degree of approximation, sufficient to enable the formula (1.) to be applied, is easy, and is assisted by a knowledge of their chemical composition, in consequence of the well established laws, *first*, that perfect gases combine by volumes in simple numerical ratios only; and, *secondly*, that the volume of a given weight of a compound perfect gas always bears simple numerical ratios to the volumes which its constituents would occupy separately. An example of the application of these laws is given in STEAM. In stating the results of such computations (which are of frequent occurrence in works on chemistry), T_0 is generally assumed $= 32°$ as a standard temperature, and the quotient $\frac{D_0}{P_0}$ is computed, D_0 being the approximate weight of a cubic foot of the vapour, and P_0 the pressure in *atmospheres* of 14.7 lbs. to the inch. What is conventionally called the *specific gravity* of a vapour is the ratio of the quotient $\frac{D_0}{P_0}$ for the vapour in question to the corresponding quantity for atmospheric air. The specific gravities of vapours and gases are proportional either to their atomic weights, or to some simple multiple of their atomic weights. The following are the values of $\frac{D_0}{P_0}$ in lb. per cubic foot per atmosphere, for a few gases and vapours:—

Air,	0·080728	Ether,	0·2093
Oxygen,	0·089256	Bisulphuret of carbon,	0·2137
Hydrogen,	0·005592	Mercury,	0·563
Carbonic acid,	0·12344		
Steam,	0·05022		

The direct experimental determination of the densities of vapours, to a degree of accuracy sufficient to show the *exact* amount of their deviation from the perfectly gaseous condition, has not yet been accomplished. A method of computing the probable value of such densities theoretically, from the heat which disappears in evaporating a given quantity of the substance, is explained and illustrated in HEAT, MECHANICAL ACTION OF, § 20.

§ 2. *Atmospheres of Vapour—Spheroidal State.*—From what has been stated, it appears that every solid or liquid substance in a state of molecular equilibrium, wherever it is not enveloped by another solid or liquid substance, is enveloped by an atmosphere of its own vapour, of a density and pressure depending on the temperature (provided the substance be volatile at that temperature.) It has been suggested as a hypothesis, that the density of a very thin layer of this atmosphere, immediately adjoining the surface of such liquid or solid, may, owing to the attraction of the liquid or solid, be much greater than the density at considerable distances, and that the elasticity of an atmosphere of vapour so constituted may be the cause of that resistance to being brought into absolute contact, which is displayed by the surfaces of solid and liquid bodies in general (*e. g.*, when raindrops roll on the surface of a river), and which is so great at high temperatures as to produce what is called the "*spheroidal state*" of masses of liquid, in which they remain suspended over hot solid surfaces with a visible interval between. The only substance on the earth's surface which is sufficiently abundant to pervade the whole of the earth's atmosphere at all times with vapour, to an amount appreciable by mechanical and chemical processes, is water.

§ 3. *Mixtures of Vapours and Gases.*—It has already been explained (HEAT, MECHANICAL ACTION OF, § 9), that the pressure exerted against the interior of a vessel by a given quantity of a perfect gas enclosed in it, is the sum of the pressures which any number of parts into which such quantity may be divided would exert separately, if each were enclosed in a vessel of the same bulk alone, at the same temperature; and that, although this law is not exactly true for any actual gas, it is very nearly true for many. Thus, if 0·080728 lb. of air, at 32°, being enclosed in a vessel of one cubic foot of capacity, exert a pressure of one atmosphere, or 14·7 lbs., on each square inch of the interior of the vessel, then will each additional 0·080728 lb. of air which is enclosed, at 32°, in the same vessel, produce very nearly an additional atmosphere of pressure. It has now further to be explained, that *the same law is applicable to mixtures of gases of different kinds.* For example, 0·12344 lb. of carbonic acid gas, at 32°, being enclosed in a vessel of one cubic foot in capacity, exerts a pressure of one atmosphere; consequently, if 0·080728 lb. of air, and 0·12344 lb. of carbonic acid, mixed, be enclosed at the temperature of 32° in a vessel of one cubic foot of capacity, the mixture will exert a pressure of two atmospheres. As a second example—let 0·080728 lb. of air, at 212°, be enclosed in a vessel of one cubic foot, it will exert a pressure of $\frac{212° + 461°\cdot2}{32° + 461°\cdot2} = 1\cdot365$ atmosphere. Let 0·0379 lb. of steam, at 212°, be enclosed in a vessel of one cubic foot; it will exert a pressure of one atmosphere. Consequently, if 0·080728 lb. of air, and 0·0379 lb. of steam, be mixed and enclosed together, at 212°, in a vessel of one cubic foot, the mixture will exert a pressure of 2·365 atmospheres. It is a common, but erroneous practice, in elementary books on Physics, to describe this law as constituting a *difference* between mixed and homogeneous gases; whereas it is obvious, that for mixed and homogeneous gases the law of pressure is exactly the same, viz., that the pressure of the whole of a gaseous mass is the sum of the pressure of all its parts. This is one of the laws of mixtures of gases and vapours.—A *second* law is, that *the presence of a foreign gaseous substance in contact with the surface of a solid or liquid, does not affect the density of the vapour of that solid or liquid,* unless (as M. Regnault has recently shown) there be a tendency to chemical combination between the two substances, in which case the density of the vapour is slightly increased. For example: let there be a mass of liquid water in a receiver, at the temperature of 212°; and above the surface of the liquid water, let there be a space of one cubic foot;—it is necessary to molecular equilibrium at the given temperature of 212°, that that space of one cubic foot should contain 0·0379 lb. of steam, whether the space be void of all other substances, or filled with any quantity of air, or of any other gaseous substance which does not exert an appreciable chemical attraction on the water. To illustrate the law further, let the temperature of the water be 50°; then it is necessary to molecular equilibrium that the space of one cubic foot above the water should contain 0·00058 lb. of watery vapour, whether and to what amount soever air, or any other gaseous substance not chemically attracting the water, be contained in the same space.—This, and the preceding law of mixtures of gases and vapours (discovered by Dalton and Gay-Lussac), enable the following question to be solved:—*Problem.* Given the total pressure P, of a mixture of a gas and of a given vapour, in a space saturated with the vapour at the temperature T; required the pressure and density of the gas separately.—*Solution.* Find, from a table of experiments, or from a formula, the pressure of saturation of the vapour for the given temperature T; let it be denoted by p; then the pressure of the gas is $P - p$; and its density is less than the density which it would have had under the pressure P, if no vapour had been present, in the ratio $\frac{P - p}{P}$.—*Example.* A space contains mixed air and steam, being saturated with steam at 50°, and the total pressure is 14·7 lbs. on the square inch; what is the pressure of the air separately, and what weight of air is contained in each cubic foot of the space?—*Answer.* Either from M. Regnault's experiments, or from the formula already cited, it appears that the pressure of the steam is 0·175 lb. per square inch; consequently, the pressure of the air separately is 14·7 — 0·175 = 14·525 lbs. per square inch. Also, the weight of air in a cubic foot, at 14·7 lbs. per square inch and 50°, had there been no steam present, would have been $0\cdot080728 \times \frac{493°\cdot2}{50° + 461°\cdot2} = 0\cdot077885$ lb.; consequently the weight of air actually present along with the steam, in a cubic foot, is $0\cdot077885 \times \frac{14\cdot525}{14\cdot7} = 0\cdot076976$ lb. A second problem is, to find the density of the mixture of gas and vapour; which is solved by adding to the density of the gas already found, the density of the vapour as computed by the methods formerly referred to. Thus, in the case last given, it appears, by computing from the latent heat of evaporation, that the weight of steam in a cubic foot is 0·00058 lb.; consequently, the weight of a cubic foot of the mixture of air and steam is 0·076976 + 0·00058 = 0·077556 lb. With respect to the amount of the deviations from the laws given in this section, which occur when the ingredients of the gaseous mixture have a chemical affinity for each other, the reader is referred to the later researches of M. Regnault already mentioned.

§ 4. *Evaporation and Condensation.*—When the density of the vaporous atmosphere of a solid or liquid is diminished, by the enlargement of the space in which the substance is contained, or by the removal of part of the vapour, whether by mechanical displacement or by condensation in an adjoining space, the solid or liquid evaporates until the vapour is restored to the density corresponding to the existing temperature. The same thing takes place when the molecular equilibrium is disturbed by communicating heat to the solid or liquid.—When the density of the vaporous atmosphere is increased, by the contraction of the space in which the substance is contained, or the addition of vapour from another source, part of the vapour condenses until equilibrium is restored as before. The same thing takes place when the equilibrium is disturbed by abstracting heat from the vapour.—Evaporation is accompanied by the disappearance of heat, called the *Latent Heat of Evaporation*, and condensation by the re-appearance of heat, according to laws stated in HEAT, MECHANICAL ACTION OF, § § 19 and 21. When the space above the solid or liquid is void of foreign substances, the restoration of equilibrium is sensibly instantaneous; when that space contains foreign gaseous substances, it is more or less *retarded*, although the conditions of equilibrium are not changed. It is the retardation of the diffusion of watery vapour by the presence of air which prevents every part of the atmosphere from being always saturated.

§ 5. *Ebullition.*—When the communication of heat to a liquid mass and the removal of the vapour are carried on continuously, so that the pressure throughout the mass of liquid is not greater than that of saturation for its temperature, evaporation takes place, from the exposed surface of the liquid, and also from its interior; it gives out bubbles, and is said to boil. The ascertaining by experiment of the temperatures of ebullition, or *boiling points*, of a liquid under various pressures, is the most accurate method of determining the relation between the temperature and pressure of saturation of its vapour.—Conversely, when that relation is known, the boiling point of the fluid may be made the means of measuring the pressure on it. On this principle is founded the method invented by Wollaston, and since perfected by Dr. J. D. Forbes, of deducing the atmospheric pressure, from the boiling point of water in an open vessel.—When the term *boiling point* of a fluid is used without qualification, it means the boiling point under the average atmospheric pressure of 14·7 lbs. on the square inch.—The presence in a liquid of a less volatile substance dissolved in it, resists ebullition, and raises the temperature at which the *liquid boils*, under a given pressure; but unless the dissolved substance enters into the composition of the *vapour*, the relation between the temperature and pressure of saturation of the latter remains unchanged. A resistance to ebullition is also offered by a vessel of a material which attracts the liquid; and the boiling takes place by starts. To avoid the errors which causes of this kind produce in the measurement of boiling points, it is advisable to place the thermometer not in the liquid, but in the vapour.

Vapour, Nebulous or **Vesicular,** is a condition of fluids more properly called *Cloud, Mist*, or *Fog*, inasmuch as it is not a vaporous condition, but a particular form of the liquid condition, in which the fluid floats in the air in the form of innumerable small globules, which are with good reason believed to be vesicles or hollow bubbles. The hollowness of the globules of cloud has of late been confirmed by some optical researches of M. Bravais on the Fog-bow. The condition of cloud is one into which fluids pass from the state of vapour, on being condensed by mingling with cold air. It is probable that the globules of cloud are filled and enveloped with a vaporous atmosphere. By heat they are made to evaporate and disappear; by cold to coalesce into liquid drops, which fall to the ground, or adhere to neighbouring solid bodies.

Variable. A quantity considered in any investigation may be regarded as either constant or varying during the course of it. Thus a rectangle may be considered as described by a constant line which moves parallel to itself along a given line. Any plane curve may be considered as described in the same way by a variable straight line.

Variation. See LUNAR THEORY.

Variations, Calculus of. Allusion has already been made to the principles of this remarkable method in Analysis. Its foundation is a method of *differentiation*, but quite of a peculiar kind. For instance, it is the primary object of the ordinary calculus, to determine the form that must be assumed by the function $\varphi\, x$, if x should receive an increment or become $x + h$. In the calculus of Variations the object is to ascertain and lay down the laws of the changes supervening on a slight alteration of the *form of the function*, or should $\varphi\, x$ become $\varphi^1\, x$. Had space permitted, we should have given an outline of the present grasp of this calculus. Problems of *Isoperimeters*, formerly insoluble, it commands with utmost ease. Its power over mechanical problems, was long ago demonstrated by Lagrange. At present there is no department of high physics that can be touched without its aid. The student is recommended to the treatise by Mr. Airy, the more extensive one of Professor Jellett, and the great German work of Strauch.

Variation of the Moon. See LUNAR THEORY.

Vault. Unfortunately we can now add nothing more to the elementary theory given of the ARCH under a former article. The student is referred to an excellent and perspicuous elementary work—Hann's *Theory of Bridges*, and

above all others to Professor Moseley's *Engineering and Architecture.*

Vector, Radius. When a body describes any path round a fixed point, the radius vector at any moment is the line drawn from the fixed point to the position of the body at that time.

Velocity. The speed with which a body is moving. If the body move uniformly, this may evidently be measured by the quotient of the space divided by the time. Thus the velocity 30 miles an hour, would be represented by 30 if the units be miles and hours. Where the movement is not uniform, the velocity is measured by the space described in an infinitely small time divided by the time. Thus $\frac{ds}{dt}$ is the measure of velocity. For *angular* velocity, see ROTATION.

Velocities Virtual, Principle of. Under STATICS, the notion of this principle has been unfolded and its nature described, as the formal link between the general principles of Equilibrium and Abstract Analysis. Its merit indeed is, that it contains and expresses in an analytic form, the entire foundations and peculiarities of statical problems. Many demonstrations have been given of the principle of virtual velocities; some of them resting on very abstract mathematics, and — exactly as they have done so—with indifferent success. It is not an abstract mathematical proposition at all. Just as the principle of D'Alembert is a permissible generalization of the law of the equality of action and re-action, so is this principle of Varignon, or rather of Lagrange, a permissible generalization of the simplest and best known laws of equilibrium. In neither case, however, is the generalization reached by rash or arbitrary inference. Certain definite notions or laws or modes of reasoning on these subjects having been accepted—whether on the authority of universal experience or of intuition, or what is nearer the truth, a mixture of the two—a few elementary facts or formal expressions of facts, become capable of being generalized into those two laws. We are touching however on metaphysical rather than on physical difficulties. The scientific import of the principle of virtual velocities, is that belonging to a *link*, as aforesaid; as D'Alembert's principle, in its turn, resolves Dynamical into Statical problems.—See a very interesting article by Professor De Morgan in *Knight's Cyclopædia.*

Ventilation has been defined "the art of supplying enclosed spaces with adequate quantities of pure atmospheric air." Man does not live by bread alone. Besides food to eat and water to drink, he must have air to breathe. From the moment of his birth to that of his death, twenty times every minute, he must inhale his aerial draught. Of his material wants the demand for air is by far the most inexorable. Food he may go without for many hours; water he requires only occasionally; but let him be deprived of air for a few seconds only, and death is inevitable.—The gaseous compound provided by nature for the use of man in respiration—common or atmospheric air—consists of two ingredients, oxygen and azote, combined in the proportion of one measure of the former to four of the latter. Of no other mixture can he partake with perfect safety. A man of average size inhales about three thousand gallons of air in a day. To meet this air, physiologists tell us that the heart sends two ounces of blood, seventy-five times a minute, or twenty-four hogsheads in twenty-four hours. Not a few important consequences ensue from bringing together these enormous quantities of air and blood. A fire without flame is kindled, and a large amount of heat is produced. The blood is purified, the air is vitiated; a fifth part of the oxygen of the inspired air disappears, and is replaced by a deadly poison, carbonic acid gas. The mixture *ex-pired* is wholly unfit for the support of animal life. When man is in the open air, the evolution of the noxious products of respiration is attended with little harm. Being warmer than the surrounding air, and therefore, bulk for bulk, lighter, they immediately, like oil in water, rise to a higher level, where they are speedily diffused, leaving their place to be filled by the rushing in of the colder, denser fluid around.—But we do not live always in the open air. Atmospheric vicissitudes, were there nothing else, compel us to have recourse to "ceiled houses." In these, it will readily be perceived, the process of respiration is carried on in circumstances altogether different. In few cases is there provided either outlet or inlet for air. In the earlier ages, when the openings for light were at the same time openings for air, something like ventilation necessarily took place, but the invention of glass windows put an end to that. It need cause no surprise that such a state of matters should frequently be attended with injurious consequences. With the causes of contamination in full activity, and the natural movements of the air, whereby the defilement might be neutralized, carefully and studiously prevented, what can possibly follow but disease and sometimes death? Numberless plans have been adopted for clearing buildings or apartments, of foul air. From the days of Desaguliers and Hales, few years have elapsed without the appearance of an expedient for accomplishing complete and perfect ventilation. It is well, however, to keep in mind that the leading principles involved in the problem are few. In every system of domestic ventilation there must be an outlet for the vitiated air, and an inlet for fresh air. Having obtained these, the next point to be considered is, the means to be used to cause a current between the two. In the spontaneous ventilation of nature, we have seen that the heat of the expired air in ordinary circumstances is enough for this purpose,

and there can be no doubt that but for artificial interruptions the case in rooms would be exactly the same. The cause of motion would be the difference of temperature between the air expelled from the chest of the individual breathing, and that immediately outside the apartment. An operation, in short, would be instituted identical in character with the *draught* of the common chimney. Few apartments, however, are constructed with a view to admit of this simple process being brought into play. The ceilings are almost invariably flat and impervious. If there be outlet or inlet at all, these, generally speaking, are furnished by very sorry make-shifts,—such as imperfectly fitted casements, warped doors, keyholes, and chinks in the walls of the building. But what of the chimney? it may be asked; is not the open fire-place and flue quite a sufficient outlet for the vitiated air? There can be no doubt that as at present constructed they are not. The brisk draught caused by a blazing fire in an open grate does much, certainly, to change the air near the floor, but this is by no means all that is wanted. The hotter strata near the ceiling, contain the baneful products of breathing and burning, together with various animal exhalations; and these, it is obvious, can be but little affected by the chimney current. Several contrivances have been brought forward with the view of aiding the open fire-place in ventilating the apartment. Tredgold recommended an inverted syphon, the one leg to open into the chimney,—the mouth of the tube, with a register, being placed near the ceiling. Mr. Toynbee inserted into the chimney, a little way from the ceiling of the room, a square iron tube, fitted at the end opening into the room with a plate of perforated zinc, and a flap of oiled silk to act as a valve, allowing the foul air to pass up and prevent smoke coming down. Another instrument, intended to answer the same purpose, is the balanced valve of Dr. Arnott. Where the outlets and inlets are properly adjusted, this contrivance often proves of great service in securing a draught from the upper part of the apartment. In most places fitted up with the view of allowing spontaneous ventilation full play, it is customary to have the abduction tubes at the roof, and the induction tubes near the floor. This arrangement, however good theoretically, is in practice attended with inconvenience. Cold currents on the lower portions of the body are unavoidable. A plan altogether different from these has been tried lately, and apparently with the best results. The method adverted to is founded on the circumstance that *one* tube, provided it be sufficiently capacious, may serve at one and the same instant for abduction and induction,—the centre being occupied by a column of warm outgoing air, while towards the circumference a stream of cool air is rushing inwards. Although a partial knowledge of the fact of counter currents taking place in a single opening was possessed by the earlier writers on ventilation, Mr. M'Kinnell, of Glasgow, was the first, it is believed, to discover, and draw attention to, this invariable arrangement of aerial currents in the circumstances described. Openings of sufficient capacity, however, to admit of the unimpeded movements of these currents, would, in the climate of Britain, be intolerable. But, on investigation, it was found that the same effects could be obtained in smaller space by relieving the ascending and descending currents from mutual contact. Mr. M'Kinnell's patent ventilator is constructed on these principles. It consists mainly of two tubes arranged concentrically, the inner discharging the vitiated air, while the fresh supply flows down the outer tube. It is almost automatic in its action, requiring little or no attention in ordinary circumstances. It removes the air as it is vitiated, and supplies its place with pure air, in the exact amount required, in currents so gentle as scarcely to be perceptible. The contrivance also possesses this great advantage, that it can be introduced, and acts as effectively, between the ceiling and floor of the lower storeys of buildings as in apartments having immediate access to the roof. It may be well to mention that the schools of the Highland Society, and a number of other public buildings in Glasgow, have been ventilated on this principle with the best possible results. Several scientific gentlemen of eminence have examined the apparatus in operation, and have spoken favourably regarding its merits, as an instrument at once simple and effective. Such being the fact, it is to be hoped that the capabilities of the invention to attain the important end in view will be fairly tested; and it will be gratifying, if the long vexed question as to the best mode of ventilation should be set at rest by this appeal to what seems to be one of those laws which cannot be violated or overlooked with impunity,—the laws of Nature.

Venus. The second, and to the eye, the most beautiful star in the heavens. Being nearer the sun than the earth, her apparent path is mainly an oscillation to and fro,—with the sun as its centre,—from which central point the planet departs, only about 46°;—an angle termed her maximum elongation. According to the side of the sun on which she is, Venus is our morning or evening star. If to the east of that luminary, she sets after him, and then is the *Hesper* of the ancients; if on the west, she rises first, and then is the welcome *Lucifer*. The light of Venus is a dazzling white, and so brilliant as to cast a distinct shadow even under our northern impure skies. The planet's mean distance from the sun is 68,897,500 miles, so that, when at inferior conjunction she is only 27 millions of miles from the Earth. The diameter of Venus is 7,807 miles—less than that of the earth by a very small amount; her density is fully one-eighth greater than the density of our globe, or more accurately 1·12; the period of her orbital revolution 224 days, 16 hours, 49 minutes, and 7

 VEN

seconds; and the length of her day is 23 hours, 21 minutes, 22 seconds. From the comparative proximity of this planet, one might reasonably expect that much information concerning its surface has been accumulated. Unfortunately this is not the case. When Venus is nearest us, only a very small part of her illuminated portion is turned towards the earth, and she is then very near the sun. But further, the planet appears to have a very dense atmosphere charged with clouds; and the consequence is that her true surface has been rarely if ever seen. We may safely dismiss the details and measurements of her mountains given by Schroeter, as pure romance; nevertheless it is not so easy to discard the numerous intimations of the existence of mountains that were noticed by Sir William Herschel. Indeed, there is an antecedent probability in favour of such a fact, inasmuch as the upheaving cause that has disrupted and rendered so uneven the surfaces of other better known bodies, is in all likelihood a *cosmical* one. If the determination usually given of the inclination of the axis of Venus to the plane of its orbit, viz., 74°, be correct, the relations of the surface of the planet to heat and the seasons, must be very different from ours; the breadth of its torrid zone must be equal to nearly five-sixths of its entire circumference. In this respect Venus stands out in remarkable contrast with Jupiter, for this gorgeous planet may be said to have no seasons. The orbit of Venus lying within the orbit of the earth, that planet necessarily has phases. It is not, however, at superior conjunction or when she is full, that she sends out the greatest quantity of light. At that position in her orbit she is farthest from the earth, and her light is diminished by distance. Her position of greatest brilliancy is an intermediate point, viz., when her elongation is 40°, although then she is in phase. Venus, because of her proximity and her mass, must largely affect the earth's orbit by inducing perturbations. One of these, of long period, has been determined only in recent times. The general relations of the two planets in their orbits are the following:—*Thirteen times* the period of Venus is very nearly equal to *eight* times that of the earth; and this gives rise to an approximate coincidence of every fifth conjunction in the same parts of each orbit. The coincidence is not exact, so that besides the effects arising from the relation just stated, there is necessarily accumulating, effects springing out of this non-exactness. These pass though a course also, occupying very many years, and constitute a *long inequality*. The detection and full discussion of this inequality is due to Mr. Airy. It amounts to only a few seconds at its maximum, and its period is 240 years. We have already referred under LUNAR THEORY to a similar action of Venus on our Moon.

Venus, Transits of. If the orbits of the two planets between us and the sun lay in the plane of the ecliptic it is easy to see that, at one period of every revolution they would seem to an observer on the earth to *transit* or pass across the sun's disc, exactly as each of the satellites of Jupiter transits at every revolution the disc of that planet. The *inclinations* of the two orbits, however, interfere with the regularity of this occurrence; and we have such transits accordingly only when—as in the case of eclipses of the sun and moon—the nodes of these orbits are in suitable positions. These phenomena are of no importance in regard of any light they shed on the physical character, or relations of any of the bodies concerned; but they constitute our best means of ascertaining the distance, or the *parallax* of the sun. A simple illustration will make

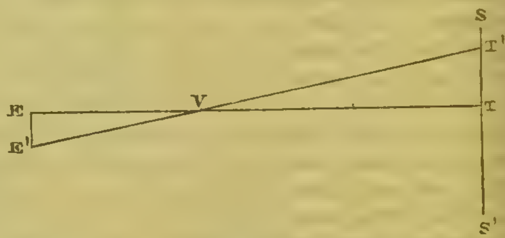


Fig. 1.

the process clear. Let $S\,S'$ be the vertical diameter of the sun, $E\,E'$ the vertical diameter of the earth, and V an intervening body. An observer at E, will see that body projected on the sun at T, while one at E' sees its projection at T'. If then that distance $T\,T'$ can be measured, as well as the distance $E\,E'$, it is plain that the ratio of the distances $E\,V$ and $V\,T$ are determined, for because of the similarity of the triangles, the following proposition holds

$$E\,E' : T\,T' = E\,V : V\,T.$$

Should $E\,V$ be also known, the distance $V\,T$, and therefore $E\,T$ would thus be determined. It scarcely requires to be remarked that the nearer the body V is to the earth, the intercepted space $T\,T'$ will appear larger, and that errors of measurement will therefore less affect the accuracy of the result; on which account as well as several others, the transits of Venus are of much more importance than those of Mercury in their application to determine the distance or parallax of the sun. The practical question remains, however, by what means shall we measure $T\,T'$? Let the occurrence be transferred to the *disc* of the sun. If that disc is represented by the subjoined circle, the paths of the two apparent transits will evidently be $T\,T''$ and $T'\,T'''$; and as these are of different lengths, the periods occupied by the transits will be different. Now, as the distance between the two chords is necessarily a function of that difference, an accurate determination

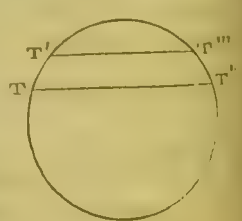


Fig. 2.

of the one will necessarily conduct to a correct value of the former. The problem is therefore reduced to this—required the periods occupied by Venus in passing across the disc of the sun, as that phenomenon is seen by observers at different places of the earth suitably chosen and at considerable distances from each other. The calculation is not so brief certainly as represented in our illustration, but as the entire nature of corrections, &c., that are required, is explained in ECLIPSES and OCCULTATIONS, they need not be again discussed. These transits are of very rare occurrence, and the intervals of time that separate them might well appear capricious; they form a recurrence, however—the following being the cycle in years, 8, 122. 8, 105. The transits never happen unless in June and November. On the last occasion, when Captain Cook had it in charge to observe from Otaheite, viz., in 1769, the sun's parallax was determined to be 8″·5776. *One hundred and five years* added to 1769, brings us to the year 1874, when the next transit will occur on December 8th; and *eight* years farther on, viz., on December 6th, 1882, another opportunity of repeating the measurements will be within reach of Astronomers. There will not be another such transit until the year 2004.

Vernier. When the unit of any line is divided into the largest number of small equal parts that can be introduced conveniently to the mechanician, many purposes induce us to seek for a still minuter subdivision. The method by which Vernier procured this was the following:—Let one of the units be divided by mechanical means into m parts; and let a little sliding scale, upon which a unit is engraved be divided into $m+1$ parts. Then suppose that in the measurement of any magnitude which reaches up to the point P, the last unit division on the original scale of measurement A, is at H. Then let the subdivision go on as marked in the figure, P lying between the nth and the $n+1$th. Move the Vernier B which is attached to the instrument by a very fine screw motion, until a division of it be just at P. Count down how many spaces on the vernier we have to go before there is a point for which its division exactly coincides with that of the original scale, and suppose that p divisions are so necessary. Then if the space p k be

called x, we have $x + (p - 1.)\frac{a}{m} = \frac{p\ a}{m+1}$.

$$\text{Hence } \frac{x}{a} = \frac{p}{m+1} - \frac{p-1}{m} =$$

$$= \frac{m\,p - m\,p + 1 - p + m}{m\,(m+1)} =$$

$$\frac{m + 1 - p}{m\,(m+1)} \therefore \frac{x}{a} = \frac{1}{m} - \frac{p}{m(m+1)}$$

$$\text{Hence } x = \frac{a}{m} - \frac{p}{m+1} \times \frac{a}{m}.$$

$$x = \frac{a}{m}\left(1 - \frac{p}{m+1}\right)$$

$$x + \text{H K} = \frac{a}{m}\left(n + 1 - \frac{p}{m+1}\right) = \text{H P}$$

Hence the value H P is accurately measured. —To discern how much smaller a quantity may be thus measured, than by the ordinary processes of measuring by $m$ths and $m+1$ths; we remark that the only two approximations to the value of H P, without the vernier would be $\frac{a}{m}n$ and $\frac{a}{m}(n+1)$. Now if $p = 1$, the additional quantity measured is $\frac{1}{m+1}$ of the division of a. Thus if the divisions are in 10ths, the vernier enables us to read with the same accuracy to 110ths.—This is the only important method now used. Another, called after Nonius has become quite obsolete. Microscopes are usually attached to an instrument provided with a vernier, so as to enable us to see accurately when P, one of the vernier division lines, coincides with one of the division lines of the proper scale.

Vertical. The direction in which a plummet hangs—that is the direction of the radius of the earth. This not quite the direction (*v.* DEVIATION) near a large mountain, in consequence of the attraction of the mountain itself.

Vesta. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Vibration. See ACOUSTICS, UNDULATORY THEORY, &c.

Victoria. One of the Asteroids. For Elements, &c., see ASTEROIDS.

Virgo. A well known constellation in the Zodiac.

Vision. The phenomena of vision have been largely discussed under other articles. See EYE, PARALLAX BINOCULAR, STEREOSCOPE, &c.

Vis Viva; Living Forces, Conservation of. During the Augustan period of Leibnitz and the Bernouillis, a question was hotly debated as to the right *measure of force*,—viz., whether it ought to be the mass multiplied by the *square of the velocity*, or by the *simple velocity*. D'Alembert, in his very beautiful treatise on Dynamics, represents the question as a purely verbal one. It was not so; although no practical problem could turn up, about whose treatment the scientific combatants would have disagreed. Hence the controversy must have been caused by the omission, from general propositions, of considerations that always entered with due weight into the treatment of special propositions; or in the general proposition, an imperfect definition of some of these considerations must have been admitted.—

The *vis viva* of any system of bodies in motion, is *the sum of all the masses, each multiplied by the square of its velocity.* If the system consist of a finite number of separate molecules, the theorem is expressed by the equation:—

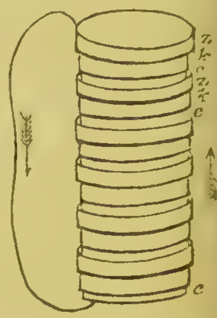
$$\Sigma\, m \,.\, v^2$$

If we treat of a continuous mass of a body, for instance, the expression is:—

$$\Sigma \int v^2\, d\, m.$$

The *Conservation* of the *Vires Vivæ* is this,—no system of bodies in motion, can acquire or lose any quantity of *vis viva*, in consequence of the mutual action of its different parts. This loss or acquisition must accrue through the agency of external forces. In technical language, when external forces are not operative, we have $\Sigma\, m\, v^2$ necessarily constant. This proposition is so valuable, that it may be taken as one main foundation of the philosophy of *Machines.* Still farther, in all cases that occur in nature, the amount of the *vis viva* acquired in passing from one position to another, depends on the co-ordinates which settle the initial and final positions.

Voltaic Pile. Shortly after the discovery, by Galvani, of the remarkable effects of associated metals and liquids, in developing an agency capable of powerfully affecting the nerves of living or recently killed animals, Prof. Volta of Pavia, deeply interested in the new field thus opened up to research, was led to notice particularly the importance of the contact of the dissimilar metals in developing this phenomenon. By some fortunate combination of thought, he conceived the happy idea not only of increasing the amount of energy by enlarging the surfaces of the metallic plates in contact, but also of greatly modifying it by placing one combination or element above another, and conducting the current thus produced through wires connected with the upper and lower plates. This arrangement was called the Pile, its ends were named the poles from their opposite qualities. The figure represents a portion of a pile, such as that first constructed by Volta; z is a zinc plate, k a similar plate of copper, and c of cloth of the same size. The cloth is moistened with an acidulated solution, such as one part of sulphuric acid, and twelve parts of water. An element consists of zinc, copper, cloth, and these elements are placed above each other, in the order indicated. The current of electricity is supposed to travel as shown by the arrows.—For the improvements on this wonderful apparatus see BATTERY and ELECTRO-DYNAMICS.



Vortex Wheel, or **Turbine.** See WATER-WHEELS.

W

Waterspout. The meteorological phenomenon known by the name of waterspout has been long the subject of terror and amazement to the sailor, and of inquiry and conjecture to the philosopher. The apparition itself is thus described by Malte Brun:—"Underneath a dense cloud the sea becomes agitated with violent commotion, the waves dart rapidly round the centre of the agitated mass of water, on arriving at which they are dispersed into vapour, and rise whirling round in a spiral direction towards the cloud. This conical ascending column is met by another descending which joins with it. The lower is often from five to eighty fathoms in diameter at its base. Both columns, however, diminish towards the middle so as to be not more than three or four feet in diameter. The entire column presents itself in the form of a hollow cylinder, or tube of glass, empty within. It glides over the sea without any noise being heard." As to the real nature of the column which constitutes the chief part of this phenomenon, and as to the causes which give rise to it, very many conjectures have been made by Benjamin Franklin, Malte Brun, Oersted, Peltier, and others, some of them plausible but incomplete, and altogether deficient in proof; others untenable as being contrary to the established principles of physics. Recently the subject has been made matter of theoretical and experimental research by Dr. Taylor of Glasgow, and the results presented to the British Association in a paper read at the Glasgow meeting in 1855. As the explanation seems to be complete and satisfactory, the object of this article will be best attained by the following extract from the paper referred to: —"Although some theories of the waterspout, more or less plausible, have been proposed, yet I believe it may be correctly stated that no attempt has been made from any of them to account for all the phenomena, far less to give anything like a rigid analysis or experimental proof. The theory which seems most eligible is that which supposes the cause of the appearance of the spout to be a whirlwind, produced most probably as is seen on the small scale in dust eddies, or dimples in a stream of water. But the difficulty comes to be felt in the details. What is the tube or spout? What force projects it downward and again draws it up? If it be the superincumbent pressure forcing down a portion of air and cloud into the rarefied space in the interior of the rotating column, how does this account for the strange appearance frequently

seen, of four or even more snake-like tubes descending close together? Is it possible to suppose each to have a separate whirl round it? This cannot be, as the opposite currents when they join would destroy each other. See fig. 1. Again, the columns or spouts are frequently seen to have a regular coiling motion like a living snake, and also to dilate and collapse like the drinking throat of an animal. I adopt the theory that the phenomenon is one of rotation, and subject the matter to calculation, thus:—1st, Is it possible that the spout can be a real vacuum as some have supposed? If so, the rotatory motion must be great enough to generate at its exterior limits sufficient centrifugal pressure to sustain the whole force of the atmosphere. Suppose then that we have a line of particles rotating in a horizontal plane round its one end as a centre as at C, then w being the angular velocity $r\,w$ will be the absolute velocity of any

Fig. 1.

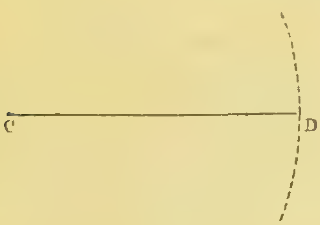


Fig. 2.

particle whose distance from C is r, and its centrifugal force is $\frac{r^2\,w^2}{r}$ or $r\,w^2$. Then the outward force at D being due to the accumulation of the efforts of the whole line of particles we have $F = \int w^2\,r\,d\,r = \frac{w^2\,r^2}{2} + C$, and if we take the diameter of the tube or non-rotating part to be inconsiderable, C will be $= o$ and $F = \frac{w^2\,r^2}{2}$ where r now signifies the outer limit of the revolving line of particles, but $v = r\,w$, so $\frac{v^2}{2} = F$, and this centrifugal force acting on the exterior particle is to be, in the case of a vacuum being left at C, equal to the whole accumulative force of gravity, which if H be the height of a uniform atmosphere, will be H g where g is the force of gravity, so $\frac{V^2}{2} = H\,g$ or $V^2 = 2\,H\,g$ or $V = \sqrt{2\,g\,H}$, which is also the velocity which a body could acquire by falling freely from the height H. So we see that the exterior boundary of the revolving plate of air would require to have a velocity altogether incredible and utterly incompatible with the comparatively harmless nature of waterspouts—twenty-five miles per minute, while the most violent hurricane has only a speed of from one to two miles per minute. Ships have passed through waterspouts with only the loss of a sail or two. But, indeed, it is evident that, unless the whirl extended to the upper limits of the atmosphere, it is utterly impossible that a vacuum could subsist, even were the rotatory velocity of the surrounding air great enough. If the phenomenon be one produced by rotation there can be only a slight degree of rarefaction in these spouts, far less can they be empty tubes as is often supposed.—Again, suppose rotation only gentle in degree, we can see that the tendency will be for the air which lies immediately above and below the axis of motion to be thrust like two cones into its interior, and the problem now for solution is as follows:—Taking the air above, and supposing it specifically lighter, and therefore it will have a tendency to remain up, unless pushed down, and to return again when the pushing force ceases. If such a column be depressed into the interior of a mass of denser air (supposing it to have as great elasticity) it will be pushed up with a force equal to the difference between its own weight and the weight of an equivalent column of the exterior air. If h be the height of these columns, and d the density of the lighter or internal one, that of the external being unity, we have the pressures to each other as h to hd and the difference between them will be $h - hd$ or $h\,(1 - d)$ and this is the pressure which the centrifugal force of the revolving air must sustain, as it is that with which the depressed column tends to rise. That the centrifugal force may be equal to this, we must have $v = \sqrt{2\,g\,h\,(1 - d)}$. Suppose a not unlikely case, that the height of the descended column of the spout is 400 feet, and that the density of the stratum of air and cloud composing it is ·99, while the external air at the average height is 1, then the velocity of rotation necessary to retain it depressed, or to depress it, is got by the form $v = \sqrt{64 \times 400 \times \cdot 01} = 16$ feet per second, which is the extreme velocity requisite at the part of the revolving column farthest from the spout.

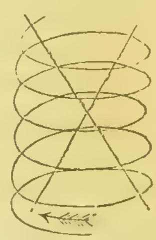


Fig. 3.

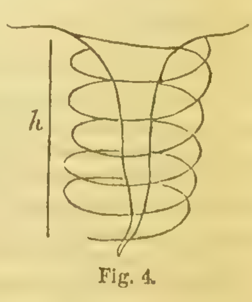


Fig. 4.

Seeing that this view of the matter gives a very moderate speed as sufficient for the depression

of cloudy shreds down into the interior of the revolving air, I resolved to submit the supposition to experiment. I take a glass box (fig. 5) supported on a wooden tray and suspended by a long twisted string. The glass plates composing the sides of the box are about eighteen inches square, and are not closely joined at the corners, but leave long narrow slits, as represented. The cover is of glass and has a circular hole in it of two inches or more in diameter. A piece of burning nitrated paper is dropped into the interior, and produces a white smoke, which, when it cools, is somewhat heavier than the air. A thin, loose, stratum of cotton wool is now placed on the upper opening to prevent external agitation from disturbing the atmosphere within, and at the same time to allow free admission for the air. The whole is now set into rotation, and almost immediately the dark column of the external atmosphere begins to descend in a beautiful conical form, like a black finger, and exactly resembling the waterspout. It, before long, passes completely to the bottom, if the rotation be quick enough, after which, as the speed slackens, the column is again drawn up (of course in reality pushed or floated), and widens out, leaving an irregular border, exactly as after the termination of the real waterspout. The action evidently is the passage of the internal heavy vapour through the slits left at the corners by centrifugal force, and the descent of the external atmosphere from above to take possession of the partial vacuum. The ascent of a similar cone would, of course, take place from an opening in the bottom, as happens with the air and spray from the sea in the real waterspout; but this part of the operation presents difficulties in practice. A gentle rotation is sufficient for the above experiment.—While making these experiments, I was pleased to find that frequently two, three, or even four or more cones descended and appeared, as the whole revolved, to coil and twist round each other. I likewise saw a complete explanation of the so-called swallowing action of the waterspout. The shreds forming the descending columns are frequently not round, but flat at one end and round at another, and sometimes resemble a slightly twisted ribbon. So, when a flat part, seen at a distance great enough to prevent its rotation being detected, presents itself alternately edgeways and then with its flat side to the eye, it seems as if it had dilated and subsequently collapsed, giving the appearance of drinking.—In conclusion, I would state that the waterspout consists of shreds of cloudy air pushed down into the interior of the portion of the atmosphere below, which has been rarefied by the action of a rotation, probably originally caused by contending currents. When the rotation is very regular and circumstances favourable, the descending column may possess symmetry and a circular section, but at other times there may be many columns, which then, however, have not each a separate rotation about it, but go round with the general mass; the rotation in the central space, where they exist, being of course slow. The ascent of the spray and air from the surface of the sea, or of light bodies from the land, is produced by the same cause."

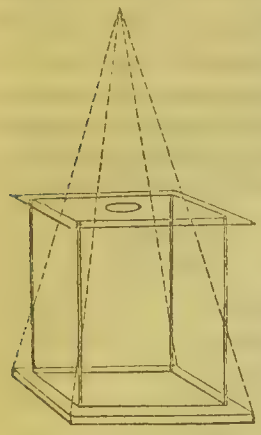


Fig. 5.

Water-Wheels are rotating engines whereby water, set in motion by the earth's gravitation, is made to perform work. Their first invention is of unknown antiquity.

§ 1. *General Theory.*—The power of a water-wheel may be stated in two ways: as *total power* and as *available power*. The *total power* is the whole energy lost in a given time by the water which is supplied to the wheel, and is a matter for theoretical computation, when the elevations and velocities of the water before and after acting on the wheel are given, and also the quantity of water supplied to the wheel in a given time; the *available power* is the work which the wheel is capable of performing in a given time when applied to overcome the resistance of mechanism, and falls short of the total power by reason of waste of energy, whose amount can be determined by special experiment only. The *total*, or *maximum theoretical efficiency* of a water-wheel, is the ratio of the whole energy lost by the water in acting on the wheel, to the whole energy possessed by it before so acting. The *available efficiency* is the ratio of the work performed by the wheel to the whole energy possessed by the water before acting on it.—Water may act on a wheel either by its *weight*, or by its *velocity*; that is, either by its *potential*, or by its *actual energy*. See MACHINES. Water acting by its weight may either press directly on the wheel, or may transmit pressure to the wheel indirectly through an intervening mass of water. In either case the *potential energy* of the water is found by multiplying the weight of water supplied to the wheel in an unit of time, into the elevation of the upper surface of the water just before it begins to press on the wheel directly or indirectly, above the upper surface of the water just after it has ceased to press on the wheel. The *actual energy* of the water is found by multiplying the weight of water supplied to the wheel in an unit of time, by the height from which it must fall in order to acquire the velocity which it has just before beginning to press on the wheel; which height is found by dividing the half-square of that velocity by the accelerating force of gravity. The total power of the wheel is found by adding together the potential and actual energy as already de-

WAT

fined, and subtracting the actual energy due to the velocity which the water has just after ceasing to press on the wheel. A main object of skill in the designing and construction of water-wheels is to insure that the latter velocity shall be neither less nor more than sufficient to prevent the accumulation of water about the wheel. To express symbolically what has been stated, let Q denote the weight of water, in *pounds*, supplied to the wheel in a second; H the difference of elevation, in *feet*, of the surface of the water before and after its action on the wheel; V_1 the velocity of the water, in *feet per second*, just before it begins to press on the wheel, or *supply-velocity*; V_2 the velocity of the water just after it has ceased to act on the wheel, or *discharge-velocity*.

Then the potential energy of the water is

QH foot-pounds per second;

the actual energy,

$$Q\frac{V_1^2}{64 \cdot 4} \text{ foot-pounds per second;}$$

the total energy,

$$Q\left(H + \frac{V_1^2}{64 \cdot 4}\right) \text{ foot-pounds per second;}$$

the energy of the water when discharged,

$$Q\frac{V_2^2}{64 \cdot 4} \text{ foot-pounds per second;}$$

the total power of the wheel,

$$Q\left(H + \frac{V_1^2 - V_2^2}{64 \cdot 4}\right) \text{ foot-pounds per second;}$$

the maximum theoretical efficiency,

$$\frac{H + \dfrac{V_1^2 - V_2^2}{64 \cdot 4}}{H + \dfrac{V_1^2}{64 \cdot 4}} \quad . \quad . \quad . \quad (1.)$$

The special theory of water-wheels consists mainly in determining the relation which the discharge-velocity V_2 bears, for each different construction of wheel, to the supply-velocity V_1 and height of descent H. The quantity

$$H + \frac{V_1^2}{64 \cdot 4}$$

may be called the *theoretical fall* or *head*. The available efficiency of a water-wheel falls short of the maximum theoretical efficiency principally from the following causes.—1. The resistance of the channel and orifices by which the water is supplied, which causes the actual height from which the water must descend in order to acquire the supply-velocity V_1 to be greater than

$$\frac{V_1^2}{64 \cdot 4}$$

The effect of such resistance is expressed by putting for the *actual fall*,

$$H + (1 + f)\frac{V_1^2}{64 \cdot 4}$$

f being the co-efficient of resistance of the channel and orifices of supply, determined according to the principles of hydraulics.—2. The escape of part of the water before it has completed its action on the wheel;—3. The agitation and mutual friction of the particles of water acting on the wheel;—and, 4. The friction of the wheel. The effects of the last three causes are expressed by multiplying the total power and the theoretical efficiency of the wheel by an empirically-determined fractional co-efficient; so that the *available power* is denoted by

$$(1 - k)\,Q\left(H + \frac{V_1^2 - V_2^2}{64 \cdot 4}\right)$$

and the *available efficiency* by

$$(1 - k) \cdot \frac{H + \dfrac{V_1^2 - V_2^2}{64 \cdot 4}}{H + (1 + f)\dfrac{V_1^2}{64 \cdot 4}} \quad . \quad . \quad . \quad (2.)$$

The quantity

$$(1 - k)\left(H + \frac{V_1^2 - V_2^2}{64 \cdot 4}\right)$$

may be called the *effective fall*.

§ 2. *Classification, Parts, and Adjuncts of Water-Wheels.*—Water-wheels may be classified as follows:—*Overshot-wheels* and *High Breast-wheels*, being vertical wheels, on which the water acts chiefly by its weight, or by potential energy. *Undershot-wheels* and *Low Breast-wheels*, being vertical wheels, on which the water acts chiefly by its velocity, or by actual energy; and *Turbines*, being horizontal wheels, on which the water acts partly by potential and partly by actual energy, being supplied and discharged in all directions round the vertical axis. The following are the essential parts common to all classes of water-wheels:—1. The *axis* or shaft, and its gudgeons or pivots. 2. The radiating parts on which the water acts; which in Overshot and Breast-wheels are *Buckets* or cells; in Undershot-wheels, *Floats;* in some Turbines *vanes*, and in others hollow arms. The necessary *adjuncts* of a water-wheel are,—1. The *Reservoir* or *Pentstock*, from which the water is supplied. 2. The *Sluice* or *Valve* for regulating the expenditure of water, which is sometimes acted upon by a *Governor* or revolving pendulum. 3. The *Tail-Race*, or channel for carrying away the water discharged.

§ 3. *Overshot and High Breast-Wheels.*—The water is supplied to this class of wheels at or near the summit, and acts wholly, or almost wholly, by its weight. The periphery of an Overshot-wheel consists of the *crowns* or *shrouding*, being two thin vertical rings, connected with the shaft by *arms* and *braces*, and having the space between them divided into cells by curved or angular trough-shaped partitions called *buckets*. The water pours from the pentstock through the regulating sluice, into the openings at the outer edges of the circle of buckets, filling them in succession. Formerly the buckets used to be closed at their inner sides, but now they are

made with openings for the escape and re-entrance of air. While the buckets are descending, part of the water overflows and escapes, and this is a cause of waste of energy: as each bucket arrives at the lowest point of its revolution, it discharges all its water into the tail-race, and ascends empty. A high breast-wheel differs from an overshot-wheel chiefly in having the water poured into the buckets at a somewhat lower elevation as compared with the summit of the wheel, and in being provided with a casing or trough, called a *breast*, of the form of an arc of a circle, extending from the regulating sluice to the commencement of the tail-race, and nearly fitting the periphery of the wheel, which revolves within it. The effect of the breast is to prevent the overflow of water from the lips of the buckets until they are over the tail-race. The usual velocity of the periphery of overshot and high breast-wheels is from three to six feet per second; and their available efficiency, when well designed and constructed, is from 0·7 to 0·8. The diameter of an overshot or high breast-wheel must evidently be equal to or greater than the height of the fall of water; and they are, consequently, sometimes of enormous size. A few exist exceeding seventy feet in diameter. Wheels of this class are the best where there are large supplies of water, and falls that are not too low. They have been much improved of late years by various engineers, and especially by Mr. Fairbairn.

§ 4. *Undershot and Low Breast-Wheels.*—Wheels of this class are driven chiefly by the impulse of water, discharged from an opening at the bottom of the reservoir with the velocity produced by the fall, against *floats* or boards. Every such wheel has a certain *velocity of maximum efficiency*, being the velocity of the wheel which gives the least possible velocity to the discharged water, and bearing a ratio to the supply-velocity of the water which depends on the form of the floats, but does not in any case differ much from $\frac{1}{2}$. In undershot-wheels of the old construction, the floats are flat boards in the direction of radii of the wheel; and the maximum theoretical efficiency is $\frac{1}{2}$. The available efficiency is much less, seldom exceeding $\frac{1}{3}$. An undershot-wheel, provided with a *breast* or casing extending as before described from the sluice to the commencement of the tail-race, becomes a low breast-wheel, in which the water acts partly by weight, though chiefly by impulse. This class of wheels was much improved by Poncelet, who curved the floats with a concavity backwards, adjusting their position and figure so that the water should be supplied to them without shock, and should drop from them into the tail-race without any horizontal velocity. The maximum theoretical efficiency of such wheels is as great as that of overshot wheels; but their available efficiency has not been found to exceed 0·6. They are well adapted to low falls with large supplies of water.

§ 5. *Turbines.*—A turbine is a horizontal water-wheel with a vertical axis, receiving and discharging water in all directions round that axis. It is driven partly by the weight of water and partly by the impulse, and has a certain velocity of maximum effect, bearing a ratio to the fall of water which depends on the construction of the turbine, and which is, in general, about half the velocity at which the turbine would revolve if unloaded. The maximum theoretical efficiency of every well designed turbine differs from unity only by the amount due to the velocity requisite to discharge the water: the available efficiency is variously stated, but may be held to range from 0·66 to 0·8. Turbines are easily adapted both to high and to low falls, and are specially well suited for high falls with small supplies of water; they have also the advantage of being capable of working *drowned*, or wholly immersed in the water of the tail-race. Turbines are of three classes.—*Class* 1. The *Re-action Turbine*, or Barker's Mill, modified and improved by various inventors, and especially by Messrs. Whitelaw and Stirrat. In the best form of this turbine, the water is conducted from the pentstock by a large pipe into the centre of a hollow rotating disc, provided with two or three hollow arms, which discharge the water through orifices directed backwards, in streams tangential to the circle of revolution of the orifices. The great improvement of Messrs. Whitelaw and Stirrat, consisted in placing valves for adjusting the expenditure of water to the work to be performed at the *orifices of discharge*. The best form for the arms has been elaborately investigated, but to little purpose, the essential qualities being—abundance of room within for the motion of the water from the central pipe to the orifices, and little resisting surface without. The velocity of maximum theoretical efficiency for the revolution of the orifices is nearly that with which a jet would issue from these orifices if at rest.—*Class* 2. The *Internally Supplied Vane-Turbine*, perfected by M. Fourneyron, is represented in plan by figure 2. A large pipe A supplies water from the pentstock to a fixed or cylindrical water-chamber B B, having

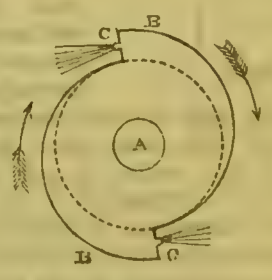


Fig. 1.

Plan of Reaction Turbine.

A Supply Pipe. B B Arms of disc. C C Orifices.

orifices all round its periphery, to which the water is so directed by curved guide-blades, that it strikes in an almost tangential direction against the vanes of the wheel C C, which encircles the water-chamber. Those blades are curved backward in such a form, that when the turbine revolves at its proper velocity the water is discharged without rotatory motion. The rules for

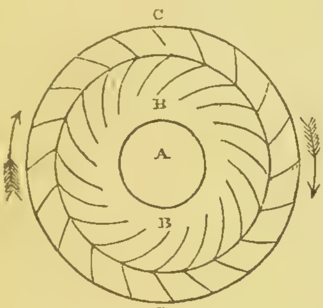


Fig. 2.

properly determining the forms and proportions of this turbine are given in detail in M. Fourneyron's work on the subject.—*Class* 3. The *Externally Supplied Vane-Turbine*, or *Vortex Water-Wheel* of Mr. James Thomson, is represented in plan by figure 3. A is a cylindrical water-chamber, supplied by a large pipe or pipes from the pentstock. B B B B curved guide-blades, so placed as to cause the water, in moving from the circumference to the centre of the chamber, to form a vortex or whirlpool. These blades are moveable about pivots at C C C C, so as to adjust the openings between them for the passage of water. D the wheel, with vanes round its circumference, and a large orifice, or pair of orifices in its centre, for the discharge of the water into the tail-race. The vanes, at their outer ends, are in the directions of radii of the wheel, so as to be acted on perpendicularly by the whirlpool; but towards their inner ends they are curved backwards in such direction that the water, when delivered from them into the central discharge-orifices, has no rotatory motion. The best velocity for the periphery of this wheel is that due to one-half of the fall; and for a given fall, it is much less than the best velocity for other classes of turbines. This is a considerable practical advantage. Another advantage is the ease with which, by means of the moveable guide-blades, the expenditure of water is adjusted to the work to be performed. See Fairbairn *On Water-Wheels;* Rankine *On Prime Movers.*

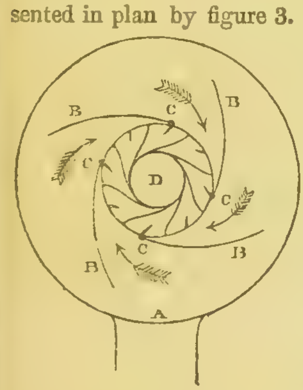


Fig. 3.

Waves. Weber divides waves into two kinds, standing waves and progressive waves. The first kind are seen in the motions of the particles of a stretched string which has been drawn aside in the middle and allowed afterwards to move by its own elasticity and tension, as in fig. 1, where the particle A moves with a gradually increasing velocity till it reaches the position of repose indicated by the continuous line, during which motion it is urged by the decreasing tension. Here it would remain were it not for the momentum it has acquired. In consequence of this, however, it still proceeds, and now against the force of tension. Its velocity is thus gradually destroyed, and it comes to rest in a position A′, again to commence a new vibration towards A, and so on, till the air and other resisting causes gradually exhaust its energy. This energy exists, at one part of the motion, it will be seen, wholly in the tension of the string, and at another wholly in momentum, or *vis viva* of the particles, and at intermediate parts of both. The one is called *potential* energy, and the other *actual* energy. If the potential energy is proportional always to the distance from the neutral or quiescent position, it is easy to prove that large and small vibrations will be performed in the same time, and the reasoning is precisely the same as that employed in the case of the cycloidal pendulum. This is very nearly the fact for stretched cords of such substances as musical strings are made of, where Hooke's law "ut tensio sic vis," holds good. Hence the valuable property, that the note of such a musical string is the same whether it is softly or strongly struck. It will be noticed that in this kind of vibration or curve motion, all the particles of the string similarly move or come to rest at the same instant, or, in other words, that they are all in the same phase of movement simultaneously; hence the name standing waves. It is otherwise with the movements of the atmospheric particles which are set in motion by such a string, and which constitute the sound derived from it. The air being a compressible and elastic substance, the moving string drives one particle closer against another than its normal position; this arouses a repulsive action between these two, which sets the farther one in forward motion against the one still more in advance, till the diminishing distance between them is able to overcome its inertia by the resulting repulsion, when a corresponding movement of its mass begins, and so on, in a manner more easy to conceive than shortly to describe. A condensation of aerial particles thus propagates itself with a velocity dependent on the elastic forces alone, and therefore the same for all varieties of impulse. The string after communicating this impulse recedes, thus leaving a vacuity between it and the con-

Fig. 1.

tiguous aerial particles. Back into this they are pressed by the elastic forces, and follow the string, in its opposite excursion, thus propagating, now a rarefaction which follows the preceding condensation, exactly at the same rate and in a manner similar to that which has been described. If the string still continue to vibrate, another condensation and rarefaction will follow, which will spread themselves onward from particle to particle, and thus a series of similar movements affecting *successively* the same particles, will follow each other. In this case, therefore, for a certain distance from the string, the particles are approaching each other, while at the same instant, farther on, the particles are receding from each other, or rarefaction is occurring. During this process, it is evident that the motions of any individual particle must be opposite at different times, and also that each particle must successively undergo the same motions. Thus in the annexed fig. (fig. 2), the upright lines, A, B, C, &c., represent contiguous strata of aerial particles in a state of undulation up to P, while those beyond are in repose. From A to D the particles are moving forward; from D to G they are moving backward. From G to K they are moving backward, and from K to O forward. The particles moving in the same direction at any instant are said to be in the same *phase*.

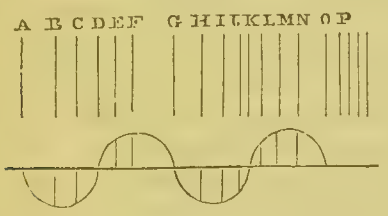


Fig. 2.

The particles in any one phase, though they move in the same direction, yet have different velocities. In the lower part of fig. 2, the velocities are represented by lines drawn downwards from the straight line, and are proportional to the lengths of those lines. The curves, instead of being, as in this case, parts of circles, might be any whatever in different kinds of undulation. Thus, it will be seen, that while A is at rest, B is in rapid motion, C less so, and D is at rest, E moving in an opposite direction, F moving more rapidly in this direction, G is at rest. Each of these in their turn going through all the movements of the others successively. From any one point in a certain state of motion or density to another point in the same state, is called the *length* of a wave. In this way waves of sound travel onward with admirable regularity, and with uniform speed, losing strength in so far as the *amplitudes* of the motion of the individual molecules is concerned, but continuing of the same *lengths* and *periods*. Individual waves cross each other and separate without mutual destruction, each particle taking on the movements due to the combined influences, as pointed out by the laws of mechanics, proving by the agreement of theory and observation, that it is by undulation of the atmosphere that sound is propagated, and that the aerial particles in their movements are regulated by the same laws which govern the most ponderous and solid masses. Wave motion of a similar kind, that is, alternate condensation and rarefaction, conveys sound and all other impulses through liquids and solids whether it be the gentle impulse of a vibrating bell, the stroke of a hammer on a bar of iron in passing from particle to particle of its substance, or the impulse of the earthquake. It has been, by application of the mechanics of undulating motion to a hypothetical substance called ether, that results so marvellous have been achieved in physical optics, and the theory of undulation is destined to play a part no less important in the science of heat, and it may be, of electricity. It would be altogether impossible, within the assigned limits of the present volume, to enter on a subject so vast as to have occupied a considerable portion of the lives of Lagrange, Euler, the Bernouillis, Poisson, Fresnel, Young, and many other mathematicians. Newton was the first who attempted successfully to explain undulation by the principles of mechanics, and to deduce the laws which regulate its propagation, and though his methods are deficient to some extent in generality, and even in applicability, compared with those which have been devised since the modern improvements in analysis, they yet have the advantage of being far more perspicuous, enjoying in this respect the full superiority of geometrical representation as compared with the results of symbolical analysis. In what follows, therefore, on the undulation of liquids, we shall take the method set forth by Newton in the eighth section of the second book of the *Principia*. Waves, in a liquid where the compressibility of the liquid is inconsiderable, as in the case of water, are propagated by alternate movements, not of condensation and rarefaction of contiguous particles, but by elevation and depression of the superficial parts of the liquid, so as to call into play the force of gravity in producing a pressure primarily in a vertical direction, but which, by a property of liquids (equality of pressure in all directions), is converted into pressure producing lateral displacement in a manner that may be illustrated as follows:—If a stone be thrown into a pool of still water the particles against which it impinges

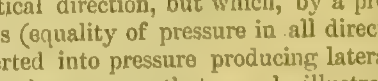


Fig. 3.

are pushed down, and as water is nearly incompressible the parts at B and C must necessarily be pushed up to make way for A. The parts B and C are now in the position of loads laid on the surface at those points which each press down-

ward and force up contiguous portions on each side, which again in a similar manner descend and propagate the motion to others, and so on. Here there is no elastic force called into play, but the weight and momentum of the particles acting according to the laws of equilibrium of fluids, develop the alternating movements of the individual particles, and as a consequence of these, cause the waves to spread regularly outward from the centre of disturbance. It is evident that, in the wave motions of liquids, it is the wave *form* alone that progresses; the individual particles of the liquid merely moving to and fro through short distances, and at the end of every wave returning to the same position. Any one who has observed the march of a line of foot soldiers, must have noticed the wave motions which run rapidly along the line. This arises from the want of perfect coincidence between the movements of the different portions of the column. Those in the front move the foot a little earlier than those farther to the rear, and this retardation proceeds regularly back, so that after a certain distance, the descent of the foot is occurring at the same instant as the lifting of those in front, and in the interval all degrees of intermediate movements have occurred. As a consequence of this, a wave is seen along the heads of the men, some rising while others are sinking. In such a case it is clear that the individual men rise and fall merely (neglecting in the meantime their real progress), perhaps only once in a second, whereas the wave may proceed 1,000 yards in the same time. A little consideration of this simple illustration will point out one of the most important peculiarities of wave motion, viz., that the rapidity of movement of the wave, or its velocity along the line, does not depend on the extent of the movements of each man, but upon the time he takes to complete a step, and above all, on what alone determines the length of the wave, the degree of retardation of movement from man to man. Thus if one-fortieth of the time of a step is lost by each man, then the twentieth man will be at his greatest height while the fortieth is only beginning to rise. The length of a wave will extend over forty men, and over this distance will the wave run during the time of each step. To proceed with Newton's investigation of the laws of progression of liquid waves. He points out the analogy of the *kind* of motion observed in a pendulum, and in the individual particles of an undulating liquid. Both proceed from rest through more and more rapid motion to rest again, and so on. Both are impelled by pressures resulting from gravity, which vary in a manner in each case nearly identical. In fig, 4, let A C represent the level of water in a bent tube, and let some force be applied, so that the level be raised in one leg to E, and depressed in the other by a similar quantity to G, it will then oscillate, and it will be observed that all the oscillations, small and great, till it comes to rest, will be performed in the same time. The reason of this will be manifest from the consideration, that the impelling force is the load of water above the level, viz., at the beginning of the oscillation E A and C G, or twice E A; and the distance to be passed over is also twice E A for each oscillation. But as the undulations decay, and become smaller, the load of water or the driving forces become smaller, and the time taken would be greater were it not for the circumstance, that the spaces to be run over are less *in the same degree.* For it is an axiom in mechanics, that with the same load, if the spaces and driving forces be proportional, the times will be equal. Hence is derived the law of isochronism of the undulations.

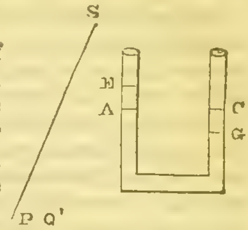


Fig. 4.

Next, to discover the law which connects the time and the velocity of a wave with its length. If a pendulum, S P, fig. 4, be taken of half the length of the undulating column of water, and it be separated from its position of repose in the vertical line, by a space equal to E A, the displacement of the water, it follows by the well-known laws of the pendulum, that the driving force will be to the whole weight of the pendulum as P Q to P S, that is as E A to half the column of liquid, or as Q E A to the whole column; that is, the driving forces in the two cases are the same parts of the load to be driven, and the spaces to be passed over are equal, so the times of describing these spaces must be equal also. If then we know the times of undulation of a pendulum of the same length as half the wave, we also know the time of a wave, and hence the velocity with which it travels, as it travels its own length in the time of two oscillations. Now, the time of a pendulum is given by the formula $t = 3\cdot14\sqrt{\frac{L}{32}}$ where L is the length in feet, and t is the number of seconds. According to the foregoing theory, if we substitute in this formula, half the length of the undulating column for L, we will have the time of oscillation of the water in the tube. Experiment verifies this. We see that from the formula $t = 3\cdot 4\sqrt{\frac{L}{64}}$ which the above becomes when we put L for the length of the wave that the time of an oscillation is proportional to the square root of the wave length, which is exactly the result arrived at by the most approved modern researches. Newton remarks that it is not true that the particles in real waves of water move back and forward exactly in the manner of the liquid in the syphon, but that they describe curved paths reentering into themselves, and therefore that this mode of investigation can only be approximately

correct; but the same remark applies to every mode which has hitherto been used for the theoretical examination of wave motion. Calculations made by means of a formula derived from Newton's method, only differ very little from the results of observation on actual sea waves. In the analytical treatment of the question of waves, the conditions of the motion of the aqueous particles are expressed in equations derived from the circumstances, 1st, that any mass of water, though altered in form, must still occupy the same volume; 2d, that movement is periodic or oscillatory; and, 3d, that the particles are subject to the action of gravity, and that they transmit to each other equal pressure in every direction. There are thus got three equations, which by elimination determine the horizontal and vertical displacements of the particles in terms of the time, of the position of the particle when at rest, and of the wave lengths or the distance between two successive particles in the same phase. The methods which must be followed in the discussion of those equations would be unintelligible, without the introduction of symbolical representation to an extent inadmissible in the present volume. For the details, the reader is referred to the treatise on Waves in the *Encyclopædia Metropolitana*. Some of the results only can be given. When the length of the wave is not greater than the depth of the water, the velocity depends (sensibly) only on its length, and is proportional to the square root of its length. When the length of the wave is not less than a thousand times the depth of the water, the velocity of the wave depends only on the depth, and is proportional to the square root of the depth. When the depth is great in comparison with the length of the wave (as in the case of ordinary waves in the open sea), the motion of the water at any great depth below the surface is wholly insignificant in comparison with that at the surface. As the depth below the surface proceeds in arithmetical progression, the motion diminishes in geometrical progression; and, at a depth equal to the length of the wave, the motion is diminished to $\frac{1}{535}$th part at the surface. In waves, shallow in comparison to the whole depth, the greatest horizontal motion is equal to the greatest vertical motion. In ordinary cases of small waves, the motions of the individual particles is in circular orbits round their positions of equilibrium as centres. In other cases, especially where the length of the wave is great in comparison to the depth of the water, the particles move in elliptical orbits, with different degrees of flatness at different depths. These results of theory are confirmed by observation. Weber has made many experiments on waves on a small scale in glass troughs, and observed the motions of the particles at different depths, by means of small particles of floating dust. He showed that, in the case of many regular waves, the particles all revolve in circles, with a uniform velocity, and he gives diagrams showing the manner in which the revolution of the particles, each in succession being in a different part of its orbit at the same instant, causes an approach of particles, and therefore from the incompressibility of the fluid, a rise of the surface at one part, while a recession of particles causes a depression. Fig. 5 is an elementary portion of such a diagram for a single particle. In cases

Fig. 5

of less regularity the particles revolve in ellipses or similar figures. Mr. Russell, in his reports to the British Association, has detailed several important experiments on the velocities and depths of waves, and he has for the first time investigated the peculiarities of the wave generated by the movement of a solid through a liquid, which he calls the oscillating wave, or wave of translation.—Captain Scoresby on a recent voyage to America observed waves 30 feet high, and 550 feet long. The horizontal pressure of waves encountering an object has been made the subject of experiment by means of Stephenson's Marine Dynamometer. It was found that the Atlantic waves frequently exerted a pressure of three tons on each square foot, while in the German Ocean the force did not exceed one and a-half tons. Waves run to vast distances along the surface of the sea, but do not agitate the water to any great depth. Thus, the same wave runs continuously from Cape Horn to the Island of Ascension, a distance of 3,000 miles, and the waves from tropical storms frequently visit the British coasts, while at a depth of a few hundred feet the ocean remains in perpetual repose. For further developments of the theory of Undulation, the reader is referred to SOUND, UNDULATORY THEORY, &c., &c.

Watt's Indicator. For the principles of all such instruments, see DYNAMOMETER.

Weather. The name generally given to the condition of any locality at any given time, as to the elements of climate. In those variable countries in which we live, fine or bad weather usually signifies, fair seasons or rainy seasons. In many regions of the globe, there are stated and regular periods of rain and serene sky; so that the prediction of the weather becomes an easy task and excites little attention: with us, however, who live under a climate so changeable, and to whom, as to all the world, fair seasons are of high importance, men catch eagerly at signs of what is to happen, and have embodied their experience in proverbs. It cannot be doubted that there are signs, having a distinct physical ground, that intimate the future with more or less precision. Of the popular signs, however,

we may safely dismiss as altogether groundless, the common proverbs connected with the Moon. The change of the moon, the periods when she enters on her new or full state, or her state of quadrature, have nothing whatever to do with the subject, and would never have attained even a popular authority, unless in regions like ours where a guess is as likely to be right as to be wrong, and where the correct guess or prognostication is always the surest to be remembered. There is only one possible influence of the moon in this direction—that, viz., derived from her *light*. There seems some ground for the assertion, that full moon has a certain efficacy in dispelling clouds: possibly because she must then transmit a certain amount of radiant heat—which heat is absorbed by our atmosphere. The beneficence attributed to the harvest moon may thus be not wholly a fable. If it is not, we should expect comparatively good weather when full moon occurs, with a high northern declination of our nocturnal luminary.—All other popular indications or prognostics are connected with the clouds; these have been well explained by Sir Humphrey Davy in his *Salmonia*. One of the speakers in the dialogue, inquiring why the clouds in the west, being red, with a tinge of purple, should portend fair weather, is answered, that the air, when dry, refracts more of the red and heat-making rays than when moist; and as dry air is not perfectly transparent, those rays are reflected in the horizon. It is added that a coppery or yellow sunset foretells rain; but that, as an indication of approaching wet weather, nothing is more certain than a halo round the moon, since it is produced by precipitated water: the larger the circle is, the nearer are the clouds; consequently, the more ready to descend in rain. In explaining why a rainbow in the morning betokens rain, and one in the evening fair weather, it is stated that the bow can only be seen when the clouds depositing the rain are opposite to the sun; thus in the morning the bow is in the west, and in the evening in the east; and as the rains in this country are usually brought by westerly winds, a bow in the west indicates that the rain is coming towards the spectator; whereas a bow in the east indicates that the rain is passing away from him.—The indications of fine weather from swallows flying high, is explained by stating that the insects on which these birds feed delight to fly in a warm stratum of air; but warm air being lighter than that which is moist, occupies a higher part of the atmosphere, and therefore the birds then find their prey in the upper regions. On the contrary, when the warm air is near the surface of the earth, the insects and birds are there also, and then, as the cold air from above descends into it, a deposition of water takes place. The opinion that sea-birds come to land in order to avoid an approaching storm, is stated to be erroneous; and the cause assigned is, that as the fish upon which the birds prey go deep into the water during storms, the birds come to land merely on account of the greater certainty of finding food there than out at sea. It may be observed here, that the kind of cloud which is designated cirro-stratus, is almost always followed by a depression of temperature in the atmosphere, and by wind or rain.—In a section of RAIN, we have described the relations of the rise and fall of the mercurial column, with that meteor.

Weight of Observations. This subject has already been discussed, in so far as our space will permit, under ERROR, PROBABILITIES, and SQUARES THE LEAST. The student is referred to the works on Probabilities by De Morgan and Galloway.

Weights and Measures. In so far as an article with this title may be presumed to comprise an account of the weights and measures in use in civilized countries, we renounce all intention of writing it here. We can advert only, and that with every brevity, to the principles involved in the adoption of *units*, and in the mode of multiplying and subdividing those units. —(1.) There are no natural units, except as to quantities of time and angular magnitude. The sidereal day is a fixed unit of time, and the right angle a fixed unit of angular magnitude. All other units are conventional,—only it were well that civilized nations could agree and adopt some *one* convention. Whence however, one standard of *length*—whence one unit of *weight?* Several attempts have been made to derive a natural standard or unit of length from some fixed dimension in Nature. For instance, the French government defined a *metre* as the ten-millionth part of a quarter of the meridian,—which quantity they trusted that their great survey would ascertain with an ultimate exactness. English scientific men on the other hand preferred as an unit, the length of the pendulum vibrating seconds in a given latitude. But as the result of the last commission appointed by the British government, we have the recommendation that all attempts to obtain a natural standard be abandoned, that a return be made to the old plan of standards manufactured in metal, and that the unit be taken from our best existing representation of the old standards. It would seem strange, to recommend a retrogression to the authority of standards which have no *absolute* authority, and of which we have no perfectly accurate copies; but the balance of convenience is decidedly with the conclusion of the commissioners,—the uncertainty being capable of being made less in this way than in any other. The commissioners proceeded farther, and accomplished the fabrication of a standard, by use of precautions and an amount of care and skill superior to which, nothing could be demanded by the most delicate and important problem in engineering. As to standards of weight, these were formerly referred to standards of length—the unit being a cubic inch of distilled

water. The commissioners recommend however that capacity be not henceforth measured by length but by weight; and that the unit henceforth shall be a pound avoirdupois carefully preserved in the form of a piece of metal or other durable substance.—(2.) Scientific abstractions having thus been abandoned as unfit for practical uses in the determination of units, it remains to inquire whether aid from theory might not help us towards a regular and uniform system of divisions and subdivisions? Now there is perhaps no change, so simple in itself which promises to yield so great an amount of practical advantage to the great body of the people, as the adoption of a purely *decimal system* in the arrangement of the various denominations of weights, measures, and money. This is a change which has been long contemplated and frequently and strongly urged by a few mathematicians and others whose attention has been more especially directed to the subject; but the question is one in which it is found extremely difficult to interest any considerable section of the public, and in the absence of popular feeling it is to be regretted that even the most obviously useful and necessary reforms are too apt to be postponed to questions of a far less important but more exciting character. —The various scales, according to which the different denominations of British weights, measures, and money are now related to each other, present the most inconceivable anomalies, and are indeed quite barbarously complex and confusing. In avoidupois weight the scale in use is 16, 16, 28, 4, and 20; in troy weight, 24, 20, and 12; in apothecaries' weight, 20, 3, 8, and 12; in long measure, 12, 3, $5\frac{1}{2}$, 40, 8, and 3; in superficial measure, 144, 9, $3\frac{1}{4}$, 40, and 4; in liquid and dry measures, 4, 2, 4, 2, 4, 8, and 10; and in money, 4, 12, and 20. It is impossible to estimate with any degree of accuracy the amount of labour annually thrown away by the nation at large, while persisting in performing the manifold computations necessary to its gigantic commerce and industry, by means of a series of tables so needlessly complicated and imperfect as those now in use. But the waste of time and loss of money must be something quite enormous, while every day it becomes greater and greater. Were the different denominations of weights, measures, and money brought into harmony with the fundamental principle of our common arithmetic, by the adoption of a purely decimal arrangement, it may safely be affirmed that the labour of commercial and professional calculations would be reduced much below one-half of what is now expended in this direction, while the risk of errors would be diminished in a still greater ratio.—In an educational point of view, this question is also very important, more especially as regards the instruction of the children of the poorer classes. Were a decimal system introduced, the various denominations of weights, measures, and money, increasing and diminishing by a uniform scale of tens and tenths, the labour of imparting and of acquiring a knowledge of all the arithmetic necessary for ordinary commercial purposes would evidently be greatly abridged. Sir John Bowring states that, in China, where a uniform decimal system is in use, a boy at school becomes a better practical arithmetician in a month than a boy in an English school can become in a year. There may possibly be a little over-colouring here; but when it is considered that for ordinary commercial computations nothing would be required beyond the power of applying readily the four simple rules, it is not to be disputed that, in so far as arithmetic is concerned, a lad of ordinary intelligence might do more by attending an evening class, during a single winter session to qualify himself for a place in a shop or office than he could now do even by a years' constant attendance at a regular day school.—Readers, who have not hitherto considered the subject, are recommended to refer to the French Tables of Weights, Measures, and Money, which are constructed on a uniform decimal scale. Full information regarding the progress of the question in this country will be found in the following publications: — *Report of the Royal Commission, appointed in* 1838, *on a Decimal System of Weights and Measures*; *Report of the Royal Commission, appointed in* 1843, *on Decimal Coinage*; *Report of Committee of House of Commons, appointed in* 1853, *on Decimal Coinage*; Sir Charles Pasley's volume on a *Decimal System of Weights, Measures, and Money*; and the publications of the "Decimal Association," established in London in 1854, to promote a decimal system of money, &c., &c.

Wheel and Axle. One of the mechanical powers. Its form and mode of operation are known to every one. It is evident enough that it is simply a lever arrangement in which the arms (the radii or spokes) are changed continually as the machine moves, the arms remaining, however, of constant length. The ratio of the power to the weight is therefore inversely as the radius of the wheel to that of the axle.

Whirlpool. A place in a river or in the sea, where, in consequence of obstructions that cause the meeting of various currents, the water takes on a revolving motion. When different currents meet in this way there is no resultant mechanically possible except a whirlpool. In various portions of the ocean, most powerful results of this sort are evolved, — witness the Maelstrom, Scylla, and our own Corryvreckan. They are all reducible to the same principle. See WINDS.

Whirlwind. See WINDS.

Winds. The Theory of the Winds, although far from perfect, has recently been greatly advanced by important discoveries in three directions. We shall briefly state these in their order.

I. THEORY OF THE WINDS IN GENERAL.—If the surface of our globe were *equally warm* at every part, and continued so, the Atmosphere

would remain ever at rest, *i.e.*, winds could not exist. But *if any one spot or region of the terrestrial superficies be heated more than another*, the equilibrium of its superincumbent aerial mass must be disturbed, and motion will necessarily ensue. The *rationale* of the process is very simple. The column of air resting on the heated region, becomes also heated by conduction, convection, and to some extent by radiation. It expands therefore, or augments in length, and must *flow over* at its highest part. The barometer must therefore fall at the earth's surface, and, in obedience to the fundamental laws of the Statics of Fluids, a rush of air will ensue towards the heated region from all quarters near the surface. Two currents or winds result therefore: one *from* the heated region in the upper parts of the atmosphere, and another *towards* it, at the surface and low elevations. The entire habitudes of these currents—their intensities at all heights, and of course the elevation of the neutral stratum, or the stratum where there is no current—have been analyzed by the late Professor Daniel, in his most instructive work on *Meteorology*, to which again we earnestly refer the student.—Apply this elementary conception to the actual earth, and how extreme the complexity of its winds! Let a variation of surface-temperature occur through any cause—even the shifting of a cloud—and peculiar aerial currents must ensue! Nevertheless there are general laws. For instance, there must in the main be a tendency to upper currents from equator to both poles, and of compensating under currents from both poles to the equator. Within the tropics, the annual course of the sun's declination must also produce regular aerial changes: and—still more minutely—so must the alternation of day and night, especially in countries on the sea-board.—But there is a second cosmical element of paramont moment in reference to all movements of the atmosphere. That element is *the diurnal rotation of the earth.* Let us see how this must affect the two great winds of which we have just spoken, viz., the under current from the poles to the equator, and the upper current from the equator to the poles. A particle of air at the pole, has no motion of rotation; and if one could suppose it transferred at once to the earth's surface at the equator, it would rest *stably*, over a surface rotating from west to east with a great velocity. The *stability* of such a particle would have precisely the effect of an east wind blowing with the velocity of the earth's rotation. And it is easy to see that through all the regions—say between North pole and equator—this polar current must take on an easterly inclination; that is to say, it will produce winds between pure north and pure east—the pure north wind existing only at the pole—the pure east only at the equator,—while north-easts must prevail in the intermediate or temperate zones. Because of precisely the same principle, only reversed in its action, the upper current from equator to pole, must become a purely west wind at the pole, having changed gradually to that direction from its initial direction of pure south—passing in the temperate zones through the intermediate inclination of south-west.—The equatorial east winds are of course the *Trade winds:* the *Monsoons* owe important modifications to the cause now specified: and it is well known that in the temperate zones, our chief winds are those from the north-east and south-west. The alternation of these opposite currents, is owing to the fact, that, through various causes, the equatorial and polar currents often change places,—the one descending to the earth's surface, and the other ascending.—The *irregular* winds prevailing in the temperate zones—say over Europe—involve indeed no other causes than differences of temperature. Suppose, for instance, that a general south-west occupies the upper regions, but that the western part of Europe is very hot, while the eastern districts remain very cold with a clouded sky. This difference of temperature will immediately give rise to an east wind, and when this wind meets that from the south-west, there will be a south-east wind, that may be transformed into a true south wind. M. Dové has indeed deduced in a most simple and ingenious way, the evolution of all winds from the reigning south-west and south-east aerial currents. He concludes, further, that in general the winds must succeed each other in a certain determinate order—an order which he designates as the *Law of the Rotation of the Wind-Vane.* The law is this:—in the regular succession of phenomena, the wind, in our latitudes, should turn from the east to the south and to the west,—thence through the north to the east again. In the southern hemisphere the course is reversed. The reader should consult Dové's original memoirs in his *Meteorologische Untersuchungen*, or the discussion of the same subject in the Manual by Kaemtz, as translated by Mr. Walker.—We close this part of our subject with an account of the main winds originating in the efficient causes just unfolded,—an account copied by kind permission of the Editor from the Notes to Johnston's invaluable *Physical Atlas.*

(1.) *Trade Winds.*—The trade winds are so certain in their course, and navigation is so simple where they blow steadily, that for this reason the early Spanish navigators gave to the trade wind region of the Atlantic between Europe and America, the name of the "Ladies' Gulf." Where these winds are well established, the weather is constantly fine, and the sky in general clear. If they suddenly cease, the sky lowers, and, in certain localities, rain and storm succeed, with a violence and a duration proportioned to the vicinity of the place to the equator; and it is remarked that their re-establishment is always by a violent reaction, or by an excessive fall of rain. The trade winds blow, with occasional interruptions and modifications, more or less

regularly round the globe; but in general their influence is not felt within 100 miles of the shore, where they are affected by the vicinity of land: in many places, as on the coast of Africa in summer and autumn, they are southerly, and assume the character of monsoon winds. On the north of the equator they blow from the E. to N.E., N.N.E., and N.; and on the south of the equator, S.S.E., E.S.E., and E.—The polar limits of the N.E. and S.E. trade winds extend generally on each side of the equator in the Atlantic and Pacific Oceans, to the parallel of latitude 30° N. and S. But these limits vary greatly in different parts of the ocean, and at different seasons, because, being subject to the influence of temperature, they remove two or three degrees towards the north or south, according as the sun has north or south declination. Thus in spring they are nearest the equator, extending sometimes at this season not further from it than the parallel of 15° N. lat. The zone of the N.E. trades in the Atlantic Ocean extends, on an average, from about lat. 7° to lat. 29° N., a section of the globe composed of nearly one-third part of land and two-thirds water. The zone of the S.E. trades in the Atlantic is much broader than that of the N.E., and the S.E. trades blow with greater freshness: they sometimes extend to lat. 10° or 15° N., whereas the N.E. trades seldom blow south of the equator. In the Pacific Ocean the trade winds first strike the sea in the parallel of about 30° N.; thence they blow N.E., and reach the belt of equatorial calms, which here merges into the monsoon region, in the vicinity of the Caroline Islands. Between the N.E. and S.E. trades a space intervenes, extending at different seasons from about 150 to 500 miles in breadth. Here the two winds meet with opposing forces so nearly balanced as to neutralize each other, and produce a calm. But as the air of the trade winds has in each hemisphere traversed obliquely a large space of ocean, it is necessarily loaded with moisture, and hence this belt of calms, known to seamen by the name of the "Variable," is also the zone of constant rain, of baffling winds, and of electrical explosions. Following the course of the sun, the belt of calms shifts its position during the year over nearly 17° of latitude, according to the season. In summer it stretches farthest north, where it remains several months, and then returns south, so as to attain its extreme southern latitude some time in March or April. In July and August it extends to between lat. 7° and 12° N., and in March and April between lat. 5° S. and 2° N. This system of calms, which, in connection with the monsoons of the Atlantic and Pacific Oceans, is called "equatorial doldrums," has a powerful effect in retarding the voyage of vessels under canvas. Besides the great equatorial belt of calms, recent investigations have confirmed the opinion that there exist, near the tropics of Cancer and Capricorn, belts of calms across the great oceans. On the equatorial side of these belts, the wind at the surface of the sea blows permanently towards the equator; while on the polar side, the prevailing direction of the wind on the surface of the ocean is towards the poles. On the polar side of the Capricorn belt the winds prevail from the N.W., and on the equatorial side from the S.E.; on the polar side of the Cancer belt the prevailing winds are from the S.W., and on the equatorial side from the N.E. Like the belt of equatorial calms, the calm belts of the tropics are variable both in breadth and in position, according to the season. The extreme vibration of the calm belt of Cancer is between the parallels of 17° and 38° N. This belt is known to American seamen as the "Horse Latitudes."

(2.) *Monsoon Winds.*—The promontory of India intruding itself into the region of the trade winds, interrupts the continuous westerly current of air, which is replaced by alternating currents from the N.E. and S.W. These change their direction as the sun passes the latitude of the place. On the Malabar coast, as the sun approaches from the southward, clouds and variable winds attend him; and his transit northward is in a week or ten days followed by that furious burst of thunder and tempest which herald the rainy season. His southward transit is less distinctly marked; it is the sign of approaching fair weather, and is also attended by thunder and storm. Monsoon winds prevail to a limited extent in several portions of the globe, as in the Gulf of Mexico, where the *norte*, or north winds, blow from September to March; and on the coast of Mexico and Central America, where recent observations have ascertained the existence of regular monsoon winds; on the coast of Brazil, where in autumn (September to March) they blow from the N.E., and in spring (March to September) from the S.E. On the other side of the continent, the monsoons of the Chili coast blow from the N. from May to September, and from the S. from October to May; on the coast of Africa, where, between the parallel of 13° N. and the equator, the trade winds are during the summer and autumnal months turned back by the heated plains of the interior, and blow as a regular southwardly monsoon during six months. The region of the ocean exposed to these winds is of a cuniform shape, having its base resting upon Africa, and its apex stretching over to within 10° or 15° of the mouth of the Amazon. These monsoons blow towards the coast of Africa from June to November, inclusive, and bring the rains which divide the seasons in these parts; in the Mediterranean, where the north winds (*Elesian winds* of the Greek) attain their greatest force in summer. But these winds are most thoroughly established, and blow with the greatest regularity, in the Indian Ocean, especially on the north of the equator, where they extend from the coast of Africa to the east coast of the Bay of Bengal, and even to the China Sea; although in the northern portion of

it they blow with less regularity, south of the equator, the monsoons extend to lat. 7° or 8° S. in the Indian Ocean. In general the monsoons blow towards the continent during summer, and in an opposite direction in winter. Thus, the period of the commencement of the S.W. monsoon, which prevails in the northern part of the Indian Ocean from April to October, corresponds to the season in which the sun, having attained a great north declination, the opposite, or N.E. monsoon prevails. The monsoons blow in one direction during half the year, or rather from the middle of April to the middle of September, and in an opposite direction from the middle of October to the middle of March. In the north of the Indian Ocean the S.W. monsoon commences in the middle of April, and terminates in the middle of September. The N.E. monsoon succeeds, and continues from the middle of October to the middle of March or beginning of April. The S.W. monsoon brings rain and foul weather. The wind blows with greater force during this than during the N.E. monsoon, when the sky is generally clear. The zone of the S.W. and N.E. monsoons is comprised between the equator and tropic of Cancer. It extends from the east coast of Africa to the coasts of India, China, and the Philippine Islands. Its influence is sometimes felt in the Pacific to the vicinity of the Mariannes, *i. e.*, to 145° E. longitude. In the north it is occasionally observed as far as the islands of Japan. Regular monsoons are established in the channel of Mozambique, from the parallel of the Bay of Iofala to the equator. On the east of Madagascar, the N.E. monsoon blows from November to April, and the S.E. from April to November, the latter being the fine season. The limits of the monsoons are not uniform in all places, and they do not always change exactly at the same period of the year. In the Bay of Bengal the winds are more variable in force and direction than in the Indian Ocean, where storms occur very frequently at the change of the monsoons. On the south of the equator, the S.E. monsoon commences in the middle of April, and terminates in the middle of September; it is replaced by the N.W. monsoon, variable to the W.S.W., which commences in the middle of October, and ends in the middle of March. This latter monsoon is the period of squalls and foul weather.—The S.E. and N.W. monsoons blow within a zone comprised between the equator and the parallel of 8° or 9° of S. latitude; but on the coast of Australia, and in the west of the Pacific, this zone extends to the parallel of 20° or 30° S. —The N.W. monsoon rarely blows with force and regularity, except in the months of December and January, when it sometimes occupies a zone comprised between the parallels of 10° or 12° S. and those of 2° and 3° N. This monsoon is subject to great irregularity. The S.E. monsoon which prevails during the fine season on the south of the equator, may be considered as an extension of the S.E. trade winds, which then extend to the equator, the sun being at that time near the tropic of Cancer. The part of the ocean where S.E and N.W. monsoons blow with the greatest force and regularity, is the Sea of Java, and thence toward the east to Timor, among the Moluccas, and toward New Guinea, in the Arapua Sea. These monsoons are experienced on the north coast of Australia, between Melville Island and Cape York, as well as in Torres Strait, where the N.W. monsoon begins at the end of October, and continues till March. —The change of the monsoons occurs between the latter half of March and September, and the first half of April and October. This change takes place gradually, and is almost always accompanied by storms and tempests. When the monsoon is about to cease, the clouds in the upper atmosphere are observed to take a direction opposite to that in which it has been blowing, although several weeks sometimes intervene before the change is apparent at the surface of the sea. Monsoon winds penetrate far into the interior of continents, but then their direction is modified by the form and contour of coasts and islands, chains of mountains, and other causes. The direction of the monsoons determine the wet and dry seasons in India; the rainy season of the west coast corresponding with the S.W., and that of the east coast with the S.E. monsoon.

(3.) *Land and Sea Breezes.*—These alternating winds, which prevail on the coasts of continents, and islands of the Indian ocean, on the African coast and other places, are occasioned by the diurnal heating and cooling of the soil, the temperature of the sea remaining nearly uniform. They follow the course of the sun, on which they appear entirely to depend, occurring sooner or later according to locality. When most powerfully felt, the air at noon is found to have attained a temperature of 120°, while that of the sea rarely rises above 80°. The air, thus heated and expanded, ascends, and draws from the sea fresh supplies to fill its room; the current thus generated constitutes the sea breeze. During night, the temperature of the earth often sinks to 50° or 60°, cooling the conterminous air, and condensing, in the form of dew, the moisture floating around. The sea is now from 15° to 20° warmer than the earth, the greatest difference between the two existing at sunrise; the air then rushes in, and draws off a current from the shore, constituting the land breeze. The sea breeze commences gently at first, and gradually increases, attaining its greatest force at the period of *maximum* heat of the day. It declines with the decreasing heat of the evening, and at sunset there is an interval of calm. During night, when the land is colder than the sea, the land breeze prevails. It attains its greatest force at the period of the *minimum* temperature of the night. A knowledge of the winds is of great use in local navigation.

(4.) *Prevailing North-Easterly and South-*

Westerly Winds.— Beyond the limits of the trade winds in the temperate zone of both hemispheres, are the region of the prevailing south-westerly and north-westerly winds. These appear to be produced by the fusion of the return currents from W. to E., occasioned by the trade winds, with the currents flowing from the poles towards the equator. The winds which result from these must necessarily take a mean direction, depending on the relative force of the opposing currents of air; this direction, although very variable, is chiefly westerly. On the north of the equator, between the parallel of 30° and 60° north, it is variable to the W. and S.W. The prevalence of south-westerly winds in the temperate zone of the North Atlantic is formed by the difference of time occupied in the voyage of wind-propelled vessels from the north of Europe to North America, and that of the return voyage from North America to Europe. From Liverpool to New York the average is forty days, while from New York to Liverpool is twenty-three days. Between Europe and America the S.W. winds prevail in the ratio of two to one. The mean direction of the prevailing winds in this zone, deduced from numerous observations, is, for England, S. 68° W.; France, S. 88° W.; Germany, S. 76° W.; Denmark, S. 62° W.; Sweden, S. 50° W.; Russia, N. 87° W.; North America, S. 86° W. Russia is the only country in which the mean direction of the wind is a little to the N. of west. In the Atlantic Ocean the most prevalent direction of the winds is between S. 45° W., and S. 10° W. When the sun is in the northern hemisphere, they prevail from S.W. to W.S.W.; but when he is in the southern hemisphere, they blow from W.N.W. to N.W. This latter period is the season of squalls and foul weather between North America and Europe. — On the south of the equator, between the parallels of 30° and 50° S. it is observed that the winds blow periodically from S.W. to N.W., that they vary from W. to N.W. when the sun has south declination; whilst during the rest of the year they are in general from W. to S.W., and are then accompanied by storms and foul weather. In general they are very variable and inconstant. Between Cape Horn and the Cape of Good Hope, a north wind of several days' duration is succeeded by dull and rainy weather, but when the wind passes to the S. of W. the weather is clear and fine.—In the frigid zone of the Atlantic, comprised between the parallel of 60° N. and the pole, enclosed by Europe and North America, and containing the islands of Iceland and Spitzbergen, no regular succession of winds has been observed. The land, from its vicinity, and the snow and ice by which it is environed, exercises a varying influence on the currents of air, according to the season. North winds are, however, the most regular and dominant. All the winds of this zone are accompanied by rain and snow, except during portions of June, July, and August, when the weather is tolerably mild during southerly winds, although accompanied by snow, rain, and fogs. The coldest winds are those from the N. and N.E., but in June and July they frequently blow from the S.S.W., and sometimes with violence. During the months of April and May, south winds bring snow; during the rest of the year there are thick fogs and bad weather. At Spitzbergen, it has been observed that, during the earlier part of the year, the winds blow from the south, and that they are northerly during the remainder. S.E. and N.E. winds bring the greatest amount of snow. At Novaia Zemlia, from September to May, the winds blow from the north almost without interruption, while from May to August they are westerly. On the coast of Greenland the winds are not periodical. From May to July the weather is fine with changeable winds, chiefly from the S.S.W. The winds are variable till September. Rains are unfrequent; storms rare, and of short duration. The coldest winds are from the N.E. In the arctic regions, according to Parry's interesting series of observations, extending uninterruptedly from July, 1819, to September, 1820, between the parallels of 74° and 75° N., the winds are very variable and moderate at all seasons of the year. The number of days on which they blow from the different quarters are as follows:—N., 111½; N.N.W., 56; N.W., 32; W.N.W., ½; W., 32; W.S.W., 1; S.W., 19; S.S.W., 2½; S.S.E., 8½; S.E., 12½; E.S.E., 8½; E., 19½; E.N.E., —; N.E., 6; N.N.E., 10; calm, 11; variable, 21. Captain M'Clure found the prevailing winds N.E. along the American shore of the polar sea. During his two winters' detention at Barring Island, he found S.S.W. winds invariably bring the greatest cold. In Hudson Bay, it has been remarked that, from October to May, the prevailing winds are from N. to N.W., and from June to October, S.E. to E. Strong northerly blasts prevail in spring and autumn.

II. Theory of Hurricanes or Cyclones. —The winds or sudden and peculiar storms usually known as Tropical *Hurricanes*, *Typhoons*, &c., belong to a peculiar order—originating in disturbing causes acting suddenly, but taking their character from the cosmical conditions of the earth. The phenomena of these are fully described in the extract given below. We shall premise a very few remarks as to their constitution and its physical causes. Two views have been taken of the character of these storms. The idea originally started by Brandes, that they are centripetal, or that they consist of currents directed towards a point of disturbance, which point has also a motion of translation, is still vigorously maintained in the United States by Professor Espy, and has been ably advocated in this country at several meetings of the British Association by Mr. Russell of Kilwhiss. But a *test*, or *crucial* question, was long ago applied by M.

Dové to the great storm of December, 1821, and the reply of the phenomena did not at all coincide with the centripetal theory. The fact that these storms are *rotatory*, or *moving whirlwinds*, owing their force, not to their motion of translation, but to their *whirlwind force*, seems established beyond doubt by a vast accumulation of observations by Redfield, Reid, Back, Dobson, and other distinguished physicists.—But besides, the rotatory theory has its roots deep in physical and efficient agencies. Dové long ago showed that a body of air could not be projected suddenly northwards or southwards from an equatorial region without taking on a rotatory movement, as well as a motion of translation corresponding in all features with the progress of the Cyclones. This paper—too little known in this country—is in the third volume of Taylor's *Scientific Memoirs*. And the subject has been recently treated on the ground of wider views by Dr. John Taylor of Glasgow. At the meeting of the British Association in Belfast, he clearly proved, alike by *à priori* reasoning and ingenious and applicable experiment, that no centripetal flow of aerial currents can take place, towards any point on the earth's surface without the generation of a rotatory storm, having all the characters of hurricanes and typhoons. The *fact of the earth's rotation* constrains this result; nor—as Dr. Taylor infers universally—can there exist a purely rectilinear current of air, except on the equator itself.—Referring to the original memoirs named, we subjoin Mr. Johnston's account of the phenomena of the cyclones.

Cyclones or Hurricanes.—The cyclones of the Antilles, and those of the Indian Ocean and the China Sea, appear to be subject to fixed laws for each hemisphere, whether as regards their movements of translation, or their gyratory movements. These laws have been carefully deduced from observation, and verified by the experience of vessels exposed to storms at all seasons, and in all the known hurricane regions. It is now fully ascertained, that in both hemispheres the air in the cyclone rotates in a direction *contrary* to that of the sun. Thus in the northern hemisphere the course of the sun is from the E. by the S., W., and N.; and the movement of the air in a hurricane is in the opposite direction, or from the N. by W., S., and E. In the southern hemisphere the course of the sun being from E. by N.W., and S., that of the cyclone is from the N. by E.S , and W. From the observations alluded to, the following general laws have been derived :—

1. Cyclones originate in the space between the equator and the tropics near the equatorial limit of the trade wind, during winter, when the winds are irregular, or at the change of the monsoons. There is no instance on record of a hurricane having been encountered on the equator, nor of any one having crossed the line, although two have been known to rage at the same time on the same meridian, but on opposite sides of the equator, and 10° or 12° apart. In both hemispheres, during the early part of their course, storms move in a direction from E. to W., and, in an opposite direction, or from W. to E., during the latter portion. In all cases they move obliquely in a direction from the equator toward the poles. In the N. hemisphere, the area of their commencement is comprised between lat. 10° and 20° N., and long. 50° and 68° W., on the borders of the narrow zone of calms and variable winds, which, as before explained, is always north of the equator, and corresponds with the zone of constant precipitation of rain and of electrical explosions.

2. They obey a double movement, one a gyratory movement, the other a movement of translation. North of the equator this gyratory movement is from right to left by the N., or in a direction *contrary* to the hands of a watch. On the S. of the equator, the movement is from left to right by N., or in a direction *coincident* with the hands of a watch. The general movement of *translation* is in the form of a parabolic curve, of which the summit is toward the W., and the branches extended to the E. The summit of this curve is tangent to the meridian, about lat. 30° in the N. hemisphere, and about 26° in the S. hemisphere, coinciding with the polar limits of the trade winds.

3. The point of departure of the storm is at the E. extremity of the curve, nearest the equator, in corresponding latitude with the declination of the sun. Thence the first half of the storm is directed westward, towards the summit of the curve, receding from the equator. It then follows this summit in a tangent to the meridian, and turns eastward, the second half of its course being at the greatest distance from the equator. The cyclones or typhoons of the China Sea are occasionally exceptional, since they follow, in certain cases, an opposite direction, *i. e.*, they approach the equator instead of receding from it.

4. The rate at which cyclones travel varies greatly, not only in different parts of the world, but even in the same localities, and at the same season. The cause of this difference of motion is not yet ascertained. In the West Indian and North American cyclones, the highest rate is stated by Redfield to be 43 miles per hour, and the lowest 9·5 miles per hour; mean, 26 miles. In the South Indian Ocean the rate is estimated by Mr. Thom at from 10 miles to little more than 2 miles per hour, and by Colonel Reid at from 7 to 12½ miles per day. Mr. Piddington's researches show, that, about the "storm path," the rate of progression is only from 2½ to 1½ miles per hour. In the Bay of Bengal the rate varies from 2 to 39 miles per hour. In the China Sea the observed rate is from 7 to 24 miles per hour, and in the Pacific Ocean probably from 10 to 12 miles per hour. Some cyclones move so very slowly that they may be almost

"considered stationary, like waterspouts or desert whirlwinds."

5. Cyclones vary in size from 50 to 500, or even 1,000 miles in diameter, a medium size being most common. They occasionally dilate and contract in their progress, and while contracting they often augment fearfully in violence. In the West Indies, near the islands, they are sometimes as small as 100 or 150 miles in diameter; but on reaching the Atlantic they dilate to 600 or 1,000 miles; the wind blowing a severe gale over the whole area; of true hurricane violence towards the centre; and the entire vortex so whirling, travels over thousands of miles of track. In the South Indian Ocean, Mr. Thom thinks that hurricanes, when first discovered, are from 400 to 600 miles in diameter. Mr. Piddington shows that they may be as small as 150 miles, and he agrees with Colonel Reid in supposing them to extend to 600 miles. In the Arabian Sea, cyclones are supposed to be under 240 miles in diameter. In the Bay of Bengal the usual size is from 300 to 350 miles, but they sometimes contract to 150 miles, at the same time augmenting in force. The typhoons of the China Sea appear to vary in size from 60 or 80 miles to 3 or 4 degrees in diameter.

East winds are characteristic of a commencing hurricane; while, in general, west winds occur only in the latter portion of the storm, as decreasing winds; hence in the northern hemisphere the most dangerous part of a hurricane is the advancing border of the right-hand semicircle, while in the southern hemisphere it is the advancing quadrant of the left-hand semicircle. The only chance of safety in navigating the hurricane regions lies in a careful observance of the indications of the barometer and simpiesometer. There is no authentic case on record of the occurrence of a cyclone in or near the tropics without some depression of the barometer, nor does any case of remarkable depression of the instrument ever occur without being followed by a storm. The amount of this depression sometimes amounts to 2½ inches, the lowest indication being at the centre of the storm.—The effect of the rotatory movement of a cyclone is to accumulate the air around its outer margin, with a pressure increasing as it recedes from the centre; consequently the barometer is lower at the middle of a storm area, and highest at its extremity. The barometer and simpiesometer oscillate before and during a hurricane, rising and falling rapidly, owing to the inequality of the pressure of the atmosphere which causes the storm, so that great barometric oscillations almost always announce the approach of a tempest. During a typhoon in the China Sea in October, 1840, in which the transport *Golcondah*, with 300 troops on board, foundered, the simpiesometer in another ship which avoided it, was observed to oscillate for 24 hours before the cyclone. The barometer is observed to rise before the strength of the cyclone is over, and this beautiful indication, as Mr. Piddington observes, is often to the seaman a rainbow of hope in the depth of his distress.—Two classes of cyclones have been observed, one in which the fall of the barometer is more or less gradual, and another in which the fall is sudden and excessive, and the tempest furious in proportion. This rapid fall appears to begin at from 3 to 6 hours before the passage of the centre. The fall of the barometer varies according to the intensity of the storm; in an extreme case it fell more than 2½ inches, or from 29·9 to 26·30. —Previous to the commencement of a cyclone, the wind is observed to be moderate or calm, the air close, sultry, and oppressive. The wind then rises and falls with a moaning sound, and, a few hours after its commencement, it is succeeded by a lull, which lasts for an hour or more, after which the wind blows from the same quarter with increased violence. The hurricane is usually accompanied by an excessive accumulation of aqueous vapour, developing electrical explosions in incessant flashes of lightning. In cyclones of considerable extent, the swell of the ocean is often felt as a double sea, one preceding the track or the wave of progression, which is driven before the storm, and the other the cyclonal wave, or that which is occasioned by the wind on different parts of the storm circle.

Seasons of Cyclones.—Little is yet known of the exact times at which hurricanes occur in different parts of the world, except in the West India regions, where they appear to be influenced by season, both as to their frequency and their direction. It has been observed that they occur most frequently near the close of the rainy seasons, when the sun is vertical to the plane of their origin. In the North Atlantic they occur from the end of June to the middle of December, but the greatest number occur in August. In the Indian Ocean the chief period of their occurrence is from November to June; they are most frequent in January and March, and least so in June and November. The cyclone season of the Bay of Bengal is in October and November, when the N.E. monsoon blows with greatest violence. Storms also occur during the S.W. monsoon in May and June. In the China Sea the typhoon cyclones occur nearly at the same season as the West India hurricanes, from June to November; the maximum being in September, and the minimum in June. In the Arabian Gulf, cyclones occur during both monsoons.

III. Physical Influences of the Different Winds.—It is evident that the various winds must be distinguished by various attributes as to heat, humidity, and the heights of the barometer by which they are accompanied. The south-west wind of these northern latitudes, being the equatorial current, must be comparatively warm and moist: while its opposite, the north-east, descending from polar regions, must be comparatively cold and dry. General facts of

this description have long been discerned, but it was reserved for M. Dové to establish the existence of a regular wind-rose or wind-card-compass, indicative of a regular dependence alike of the temperature, humidity, and atmospheric pressure of a place, on the direction of the wind. As to *temperature*, for instance, that element is lowest with a north-east wind; it rises gradually as the wind veers to the south-west, where it attains its maximum again approaching to its minimum as the wind passes through west and north to north-west. The mean tension of vapour follows the same law, while the course of the changes of atmospheric pressure is rigorously the inverse. The importance of this remarkable discovery can scarcely fail to be estimated;—it is the key to almost all the irregular variations of the meteorological elements. The *periodical* variations of these elements depend of course on the periodic (daily and annual) changes in the relations of the earth and sun. Something is due also to the influence of atmospheric waves or tides; and at length these apparently *irregular* changes are thrown back on the phenomena of the *winds*. It is to the laws of *these* then, that we must look for the ultimate resolution of meteorological problems. Their succession cannot yet be predicted; but to know that with this succession all other atmospheric phenomena are bound up, is of itself to have achieved a most important advance. The student is again referred to the excellent manual by Kaemtz; to memoirs by Quetelet in the *Annales de l'Observatoire de Bruxelles;* and to a recent tractate by Professor Manuel Johnson of Oxford.

Winter. Those three months of the year during which the sun is lowest. Astronomically, midwinter is, when the sun enters the tropic of Capricorn. But owing to the time occupied by the globe in cooling and heating, the true meteorological winter in these northern climates ought to include the months December, January, and February.

X

Xiphias. A name for the *constellation* Dorado.

Y

Year. See BISSEXTILE, CALENDAR, SEASONS.

Z

Zenith. The point in the heavens, vertically above us, that is, the prolongation of the earth's radius to meet the celestial sphere.

Zenith Sector. An astronomical instrument of a special character, but of great importance in former times, and still frequently employed. It is a telescope fixed as nearly as possible in the zenith, with micrometrical motions and apparatus fitted to determine accurately the deviation of certain stars from the zenith point. It is of signal use in detecting certain small apparent motions, and checking or verifying certain physico-astronomical theories.

Zodiac. A belt whose centre is the ecliptic or path of the sun in the heavens. The twelve most prominent constellations through which the sun moves during the year, lie along this belt, and are called the *signs* of the zodiac.

Zodiacal Light. A remarkable appearance in the sky, seen regularly after sunset or before sunrise in tropical countries, although only at suitable seasons in zones of the latitude of ours. It is a triangle of light corresponding in intensity to the brightness of the Milky Way, and stretching up through the sky nearly in the direction of the sun's equator. Being thus inclined about eight or nine degrees to the ecliptic, it is plainly only about March and April at one season, and about October at the opposite season, that such a cone can stretch—after sunset or before sunrise—sufficiently high in our skies, to be free from the usual mists of the horizon, and from the effects of twilight and dawn. Nevertheless, even with us, under favourable circumstances, this meteor shines so brightly that the crescent moon in the heart of it does not obscure its lustre. The Zodiacal Light—although seen before—was really *discovered*, *i. e.*, brought out as a substantive and constant fact by Dominic Cassini. It was long accounted the exterior part of the solar atmosphere; nor did the vast amount of its extension suggest to observers, as it did comparatively recently to Laplace, the dynamic impossibility of such a solution. No solar atmosphere could reach farther from our luminary than one-third way towards the orbit of Mercury: the centrifugal force (to speak popularly) of its extreme particles, would, at such a distance, dissipate them through space. But the Zodiacal Light appears to stretch beyond the orbit of Venus. A solar atmosphere, too, would lie rigorously in the plane of the solar equator: not only does the position of the Zodiacal Light not conform exactly to that plane, but its plane shifts, and there is every reason to believe *that the line of its* NODES *is variable*. No hypothesis, indeed, appears adequate to satisfy known dynamic conditions, but the one at present generally received. It is now recognized, that the larger planets are not the sole constituents of our

solar system, but that in all probability, masses of planetoids or meteorites—too small to be discerned individually—circulate around our luminary in streams. The zone of the asteroids is an indubitable example. We might refer also, as an analogous instance, to Saturn's Rings. The periodic showers of meteors receive, in such an hypothesis, their best explanation; and in the conception of such a ring of meteors nearer the sun, we find a key to all established phenomena connected with the Zodiacal Light. A new theory has recently been broached in America, founded on observation of this singular meteor in equatorial regions—the theory, viz., that instead of encircling the sun, the ring in question encircles the earth.—We fear we are not yet in a condition to accept of any theory, as part and parcel of positive astronomical science. Much remains to be accomplished by observation. Neither the termination nor the position and shiftings of the axis of the Zodiacal Light, have been rigorously ascertained. Its *absolute* brilliancy is also unknown, nor is it to be expected that an accurate determination of it can be obtained unless in tropic countries. The degree of that brilliancy certainly varies from the edge of the cone towards its axis, and, as we have said, the axis of greatest brightness appears to change. So soon as such facts are rightly defined, we shall expect an induction conclusive as to the geometrical form of the mass which transmits the Light. Still further however, we desire information as to the colours of the cone, and several other attributes. —It does not appear that the inquiry is at all beyond our reach; but the existence of these and similar *desideranda*, compels one to recur to the desire, that instead of erecting new Observatories at home, our British Government would take advantage of the securities offered by the spread of its Empire, and scatter such establishments through various latitudes. The phenomenon now under consideration, has a significance far wider than what concerns its own special peculiarities. The progress of Physical Science has connected it and similar portions of our planetary arrangements with the probable cause of the heat of the Sun, and with questions intimately related to the history and destinies of our system. How strangely the Future usually opens!—We fancy that we have reached some bourne, and unite in felicitations that one line of inquiry, at least, has terminated. But no term is attainable by a finite creature partaking of that Reason which is an attribute of the Infinite. Some apparently trifling and neglected fact, suddenly assumes significance; and before we have traced its indications to their conclusions, large and unexpected spheres of inquiry and speculation have been opened; the researches that seemed ended and complete, turn out a mere step in our ascent towards some loftier and spacious plain.

APPENDIX.

Atmospheres of Planets. From the close analogy of the other planets with the earth, and from the fact, almost proved, of their having with it a common origin, there is strong *à priori* reason to believe that they, too, are enclosed in gaseous envelopes, or atmospheres. According to the hypothesis of our planetary system having originated from the condensation of a previously diffused mass of gaseous matter possessing a slow but definite motion of rotation in the direction of what has now become the plane of the planetary orbits and of the sun's equator, the atmospheres would merely be such portions of each of the separate masses which went to the formation of the individual planets, as remained in the gaseous form after the other parts had assumed the liquid or solid state. The amount of such gaseous portions would mainly depend on the temperature and pressure in each individual case. Thus it is easy to suppose changes of the temperature of the earth, such that its atmosphere would be increased, by the whole of the water on its surface assuming the gaseous form, or, on the other hand, by the oxygen and nitrogen of the air becoming liquified, or even solidified, so as to render the earth destitute of an atmosphere. It is thus easy to conceive portions of the originally diffused mass of the solar system separated in the process of segregation, which, at the temperatures now prevailing, would be altogether solid, as appears to be the case with the moon, or altogether gaseous as is exemplified by comets, as suggested by Dr. Taylor of Glasgow, in a paper on the theory of comets, in which he points out the origin and motion of these bodies, on the supposition that they are atmospheres without worlds, in the same way as the moon is a world without an atmosphere. Independently, however, of conjecture, actual observation shows that several of the other planets have atmospheres. The telescope exhibits such changes of appearance on the surface of the planets, more particularly Mars, Jupiter, and Saturn, as can only be easily explained on the supposition of an atmosphere varying in its transparency at different times and places similarly to our own. The astronomer, Schroeter, came to the conclusion, that the small planet Ceres had an atmosphere 668 miles in height, while that of Pallas was not less than 465 miles. The existence of an atmosphere surrounding a world seems to us to be essential to the existence of life on its surface; and to those who believe it probable that such a world as Venus, with the same extent of surface, and the same period of rotation as the earth, would not be left without a living inhabitant, while the earth, its immediate neighbour, has been in every part, and in so many of its geologic ages, crowded with animated forms, it is easy, even in the absence of positive proof, to assume the existence of a covering of atmosphere on the surface of such a world; but even then, there still remains a difficulty in the way of peopling the different planets. This difficulty consists in the great diversity of distances from the sun, and, consequently, in the amounts of heat which they receive from this source of radiation. The great world Neptune, for instance, being at thirty times the earth's distance from the sun, would only receive $\frac{1}{900}$th of the amount of heat by radiation, on a surface of the same extent; while the planets Venus and Mercury, being nearer than the earth, would be heated to a much greater degree. The temperature of planetary space, the obliquity of the axes of the different planets, by giving rise to variations of seasons, along with diversity of conducting power for heat, as also varieties in the thermal capacity of the materials forming their outer crusts, might, to a considerable extent, modify the climates directly due to the solar power; but there is reason to believe, that the atmosphere surrounding each globe would, more than any other cause, contribute to regulate the temperature of its surface. The reader is referred to Radiant Heat, for a description of the means by which it has been discovered that media differ greatly in their power of transmitting rays of heat from sources of high or low temperature. Transparency for heat is called diathermancy; and it is found that atmospheric air is nearly completely diathermanous to heat from a source so high in temperature as the sun, so that the sunbeams traverse the atmosphere of the earth nearly in undiminished intensity; whereas the heat which must emanate from the surface of the moon along with the light, is almost completely intercepted. Mr. Hopkins of Cambridge has recently shown the bearings of that power of air, and probably other gaseous mixtures, in absorbing heat from low temperature sources, on the question

of the climate of the different planets. His reasoning is as follows:—The solar heat arrives at the lower strata of the earth's atmosphere nearly in undiminished intensity, and is spent heating the surface of the earth itself; but as this heat then becomes that of a low temperature source it cannot again be radiated off into space, because the air is athermanous to this species of heat ray. The only means by which this heat can again escape into space, is by the lower strata of air becoming heated by contact with the earth, and rising by expansion, or by partial radiations through small distances. In this way the heat must find its way again to the upper limits of the atmosphere, and be thence radiated into space; but for this convection upwards of heat by currents, &c., it is evident that the lower strata of the air must be hotter than those above them, and this to a greater degree in proportion as more heat is to be carried upwards; and also, that throughout the whole height of the atmosphere, this decreasing temperature must prevail; so that, if the atmosphere be high enough, any degree of heat might be maintained in the lower strata in contact with the surface. The temperature of the upper limit of the atmosphere must be determined by this condition, that the quantity of heat which is radiated from it into space, must equal that which reaches it from the surface of the earth. This temperature is, therefore, independent of the height of the atmosphere; whereas the temperature of the surface is greater, the greater the height of the atmosphere, to enable the heat to be transmitted upwards. The same effect, viz., an imprisonment, so to speak, of the heat rays received from all the other radiating bodies in space besides the sun, would be caused by the atmosphere; so that the surface of a planet, though receiving none of the solar radiation, might maintain on its surface a temperature higher than the general temperature of space, and higher in any degree, the depth of atmosphere being correspondingly increased. By the general temperature of space is meant the temperature which would be indicated by a thermometer, composed of some heat-absorbing material, held in space remote from the sun and every planet, and sheltered from direct solar rays, but exposed to the radiation from all other sources of heat in the universe. This might be greater than the temperature indicated by a thermometer held in the upper strata of atmosphere, as the atmospheric particles radiate heat, but do not absorb it from external space; so they would be colder than the thermometer, and would cause depression of temperature. A thermometer carried deeper into the atmosphere, by reason of meeting with greater density of atmospheric particles, would be more cooled, or would sink further, and so on, as it is made to descend deeper in the atmosphere, till, on reaching a certain point, the increasing temperature of the air, by reason of nearness to the surface of the planet, would compensate for this cause of chilling; and beyond this in descent the thermometer would begin to rise; so that there is a level in the atmosphere where a minimum of temperature would be experienced. An atmosphere of less depth than to give this minimum, would cause a diminution of temperature to the surface of the planet. This latter part of the argument used by Mr. Hopkins seems to be the weakest, as the question whether or not the thermometer would attain its minimum in the upper strata of air (admittedly the coldest), or would require to be sunk to greater depth, where it would meet with less cold, though more dense air, would depend merely on the size and nature of the thermometer; so that it does not seem so well established that the diathermanous qualities of the atmosphere of a planet might lower the temperature of its surface below that of surrounding space. Mr. Hopkins, by deducting the effects of the solar radiation, concludes that the earth's surface would have a temperature of about 39 c, if exposed only to the radiation of space, and that no planet, with a similar atmosphere, can suffer a greater degree of cold than this. And he concludes that, if the height of the atmosphere were increased by about 40,000 feet, the temperature of the earth, even in the total absence of the sun, could not sink below what it is at present. He further computes that the planet Mars, with an atmosphere about 15,000 feet higher than that of the earth, would have a temperature of about 60° at his equator—that is, have a climate similar in temperature to our own; and also, that the planet Venus, with an atmosphere similar to that of the earth, would have a maximum temperature (at the poles, by reason of the great obliquity of the axis,) of 95°; but if the atmosphere were reduced in depth by about 25,000 feet, the maximum temperature would be reduced to nearly that of the equator of the earth. These estimates of the effects in elevating temperature, produced by the atmosphere, are based upon the observations made during the balloon ascents from the Observatory at Kew. In confirmation of this view, it has been observed that places sunk below the general level of a country have a higher mean temperature, as has been observed in regard of the plains of the Dead and Caspian Seas, where a greater depth of atmosphere must be the cause of the greater heat of climate. A similar imprisonment of heat by the diathermanous qualities of the covering medium can be noticed in the case of hot-beds and greenhouses, &c., where the heat of the solar rays readily penetrates the glass, and heats the interior; but, being then converted into heat from comparatively low temperature sources, it cannot again escape through the glass with the same facility by radiation, but can only escape slowly by conduction or by currents, so that the temperature in the interior of such an enclosure rises much higher than that without.

Imaginary Expressions. Indicated even roots of negative quantities, such as

$$\sqrt{-9},\ \sqrt[4]{-a^2},\ \sqrt[6]{-b^2}, \text{\&c.}$$

They are called imaginary, because it is impossible to conceive of quantities which they represent, according to the ordinary methods of interpreting algebraic symbols. We know that any even power of a quantity, whether positive or negative, is always positive, and it is impossible to conceive of such a quantity that being taken an even number of times as a factor, will give a negative result.—Imaginary expressions arise from correct algebraic combinations, and although in an arithmetical point of view their exact value cannot be determined, they are, nevertheless, subject to all the rules of analysis, as much as other expressions. From the greater generality of algebraic operations, many expressions result which can with difficulty be brought within the range of ordinary interpretation. These expressions are, however, correct expressions of analytical facts, and it only requires a more enlarged view to render their meaning perfectly comprehensible. It will probably be found, on a proper analysis, that the subject of imaginary expressions presents no more difficulties than that of negative quantities, which is now so thoroughly settled as to leave nothing to be desired.—We shall first explain the form to which every imaginary quantity may be reduced, and then give an account of the signification to be attached to imaginary expressions generally. Every imaginary quantity can be reduced to the form

$$a + b\sqrt{-1},$$

in which a and b are real, and $\sqrt{-1}$ imaginary.—To show this, let us assume the well-known formula, for the simplification of radicals,

$$\sqrt{a+\sqrt{b}} = \sqrt{\frac{a+c}{2}} + \sqrt{\frac{a-c}{2}} \ldots (1);$$

in which $c = \sqrt{a^2 - b}$. Making, in formula (1), $b = -b^2$, which gives, $c = \sqrt{a^2+b^2}$, and reducing, it becomes

$$\sqrt{a+b\sqrt{-1}} = \sqrt{\frac{a+\sqrt{a^2+b^2}}{2}} + \sqrt{\frac{a-\sqrt{a^2+b^2}}{2}} \ldots\ldots (2).$$

In formula (2), the first radical in the second member is positive and real, and may be denoted by c; the quantity under the second radical sign, in the second member, is negative, since $a < \sqrt{a^2+b^2}$; denoting it by $-d^2$, and reducing, we have

$$\sqrt{a+b\sqrt{-1}} = c + d\sqrt{-1} \ldots (3);$$

which shows that the square root of an expression of the form, $a + b\sqrt{-1}$, is of the same form as the expression itself. Hence, the fourth root of the expression, $a + b\sqrt{-1}$, is of the same form, and so on for the 6th, 8th, or any other even root. This shows that every imaginary expression may ultimately be reduced to the form, $a + b\sqrt{-1}$, which was to be proved. Hence, in the treatment of imaginary expressions, we need only confine our attention to imaginary expressions of the second degree, and in these we shall only consider the parts involving $\sqrt{-1}$, as a factor. Every such quantity can be placed under the form $b\sqrt{-1}$, by the rule for removing a factor from under the radical sign. For multiplying imaginary expressions together, we make use of the following formulas:

$$(\sqrt{-1})^{4n+1} = \sqrt{-1} \ldots (1);$$
$$(\sqrt{-1})^{4n+2} = -1 \ldots\ldots (2);$$
$$(\sqrt{-1})^{4n+3} = -\sqrt{-1} \ldots (3);$$
$$(\sqrt{-1})^{4n} = 1 \ldots\ldots\ldots\ldots (4);$$

in which n may be 0, or any positive whole number.—To multiply any number of imaginary expressions together, reduce them to the form $b\sqrt{-1}$; multiply the co-efficients of $\sqrt{-1}$ together for one factor of the product: to find the other factor, look in the above formulas for that one in which the exponent of $\sqrt{-1}$ equals the number of factors to be multiplied together; the second member of it will be the second factor of the required product.—Thus, let it be required to find the 17$^{\text{th}}$ power of $\sqrt{-a^2}$. Reducing to the required form, we have $a\sqrt{-1}$; the 17$^{\text{th}}$ power of the co-efficient is a^{17}; making $n = 4$, we see that formula 1 is applicable; hence, the second factor is $\sqrt{-1}$, and the required power is $a^{17} \times \sqrt{-1}$. This rule will cover all operations, which differ from the corresponding operations for real quantities.—With respect to the logical value of the symbol $\sqrt{-1}$, it may be remarked that there are two separate views that may be taken of the expression. In the first place, we may regard it as a symbol of *operation*, in which case it indicates an operation absolutely impossible; for no quantity whatever, taken twice as a factor, can produce -1. In this sense, the quantity indicated by the expression, is truly *imaginary* or *impossible*. The expression may, however, be regarded as a *symbol of interpretation;* that is, it may be an expression resulting from the correct application of the principles of analysis. In this point of view, it admits of complete and satisfactory interpretation. The method of interpretation, which we are about to give, is due to M. Monrey, a distinguished modern analyst. Before proceeding to give an account of his method of interpretation, some preliminary explanations are necessary. We have seen that every imaginary expression can be reduced to the form $a + b\sqrt{-1}$. The expres-

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sion, $\sqrt{a^2 + b^2}$, is called the *modulus* of the expression. It is a property of these expressions, that, if two of them be multiplied together, the resulting product will be of the same form as each factor, and its modulus will be equal to the product of the moduli of the two factors. Thus, $(2 + 3\sqrt{-1})(3 - 7\sqrt{-1}) = 27 - 5\sqrt{-1}$. The modulus of the first factor is $\sqrt{13}$, that of the second is $\sqrt{58}$, and that of the product $\sqrt{754} = \sqrt{13} \times \sqrt{58}$. In general, if any number of factors of the given form be taken, their product will be of the same form, and its modulus will be equal to the product of the moduli of all the factors.—We may now proceed to an examination of M. Monrey's explanation of imaginary results.—If we take the expression $a + b\sqrt{-1}$, and denote its modulus by M, we shall have, for the expression

$$\text{M}\left(\frac{a}{\text{M}} + \frac{b}{\text{M}}\sqrt{-1}\right).$$

By inspection, we see that if $\frac{a}{\text{M}}$ is taken for the cosine of an angle ϕ, $\frac{b}{\text{M}}$ will represent the sine of the same angle, and by substitution, the expression becomes

$$\text{A}(\cos\phi + \sin\phi\sqrt{-1}).$$

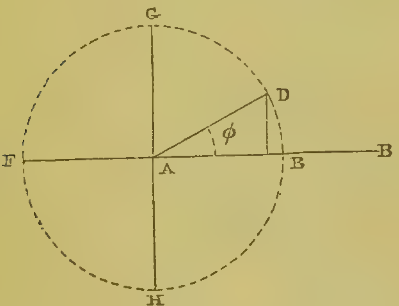


Fig. 1.

Let A be the origin of a system of polar co-ordinates, A B the initial line, and ϕ the angle made with it by any straight line A D. If now the length of the line A D be taken equal to M, then will the line A D fulfil two conditions, viz.: it is of a given length M, and makes with the initial line an angle ϕ, which conditions make up the *relation* of the line A D to the system.

The angle ϕ is called the *versor*, and the given expression represents the line A D, both in length and position; or, in other words, expresses the relation of the line A D to the system of polar co-ordinates.

If $\phi = 0$, the line takes the position A E, and the given expression reduces to M, and we have also $\text{M} = a$. This corresponds to our conventional system of representing a positive quantity by a straight line of definite length, estimated from a fixed point towards the right. If $\phi = 180°$, the expression becomes $-\text{M}$, and the line takes the position A F, which also corresponds to the same system, of $\phi = 90°$, the expression becomes $\text{M}\sqrt{-1}$, and the line takes the position A G. If $\phi = 270°$, the expression becomes $-\text{M}\sqrt{-1}$, and the line takes the position A H. In any given expression, the value of ϕ may be from the equation

$$\cos\phi = \frac{a}{\sqrt{a^2 + b^2}};$$

and this, together with the value of $\text{M} = \sqrt{a^2 + b^2}$, will serve to determine the relation of the radius vector to the system.—This method of representation conforms perfectly to every case of an expression of the form $a + b\sqrt{-1}$; it now remains to explain the results obtained by operating upon it by the rules of algebra. Let us consider the result of multiplying

$$a + b\sqrt{-1} \text{ by } c + d\sqrt{-1}.$$

Performing the multiplication, we have for the product,

$$(ac - bd) + (ab + bd)\sqrt{-1}.$$

Now, the angle which the line, whose relation is given by this product, makes with the initial line, an angle y, whose cosine equals

$$\frac{ac - bd}{\sqrt{a^2 + b^2}\sqrt{c^2 + d^2}},$$

that is,

$$\cos\gamma = \frac{ac - bd}{\sqrt{(a^2 + b^2)(c^2 + d^2)}}.$$

If we denote the angles made by the first and second lines with the initial line, by α and β, we shall have,

$$\cos\alpha = \frac{a}{\sqrt{a^2 + b^2}}, \quad \cos\beta = \frac{c}{\sqrt{a^2 + d^2}}.$$

$$\sin\alpha = \frac{b}{\sqrt{a^2 + b^2}}, \quad \sin\beta = \frac{d}{\sqrt{c^2 + d^2}},$$

we have also,

$$\cos(\alpha + \beta) = \cos\alpha\cos\beta - \sin\alpha\sin\beta$$

$$= \frac{ac - bd}{\sqrt{a^2 + b^2}\sqrt{c^2 + d^2}},$$

and consequently

$$\cos\gamma = \cos(\alpha + \beta).$$

Hence, the product represents the relation of the line A E to the system, whose length A E is equal to the length A C, taken as many times as there are units of length in A D, and making with the initial line an angle equal to the sum of the angles which the lines A C and A D make with it.

In general, the product of any number of factors of the given form, represents the relation of a line to the system, which is equal in length to the length of any one of the lines taken as many times as there are units in the continued product of the number of units in each of the other lines

taken separately, and which makes, with the initial line, an angle equal to the sum of the angles

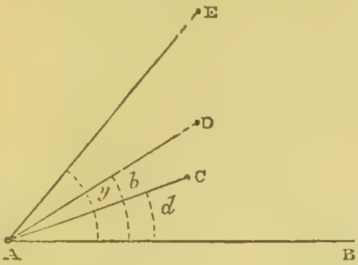


Fig. 2.

made by each line with the initial line. When this angle is any multiple of 180°, the product becomes real. The entire subject of imaginary quantities may be clearly explained, and as we see, without any impossible circumstances arising.

We see, then, that to interpret the expression $\sqrt{-a^2}$, we have simply to regard it as the representation of a straight line perpendicular to the initial line at the origin, and equal in length to a. Whilst the expression $-\sqrt{-a^2}$, represents a line equal and directly opposed to that represented by $\sqrt{-a^2}$. Thus interpreted, every idea of impossibility disappears from the mind, and the subject becomes as plain as the interpretation of negative results.

Phase. In any doctrine connected with the vibrations of particles, the term *phase* is used to designate the position of a particle in reference to the entire range of its vibration. When the particle has returned to its position of rest, or to any special position whatsoever, it is said to have vibrated through 360°: when it assumes the opposite position, it is said to have vibrated through 180°; and so of intermediate places.—It has been often stated in the text, that the more delicate and until recently the least understood classes of light-phenomena, are owing to changes impressed on the phases of rays or waves, by reflexion, &c.

Phosphorescence. A singular property belonging to all solid substances except the metals, although possessed by different substances in very different degrees. It consists in this,—taking as an instance the *diamond*,—suppose it placed for a short time under influence of the solar ray, or of any other light, and then removed to a dark room, *it will emit a strong light of its own* for a brief time,—*i. e.*, it is *phosphorescent.* The cause of this remarkable quality—which has long been known alike in fact and fable—was, until lately, only guessed at; and that we can now speak of it with some degree of certainty is undoubtedly owing to those persevering and well planned researches of Dr. Draper of New York. Setting out on the principle that some phosphorescent object should be subjected to examination, the nature of which precluded the possibility of our attributing the phenomenon to any collateral or non-essential property or merely concomitant change, Dr. Draper adopted as the most suitable body—the well known *fluor spar;* testing all special positive conclusions indicated through scrutiny of its phenomena, by the corresponding habitudes of other substances which are phosphorescent. Dr. Draper's general results are as follows:—(1.) A phosphorescent body when at its maximum of glow, has not changed its volume perceptibly. (2.) It is most probable, that, during phosphorescence, there is, in certain cases at least, a molecular modification of the shining surface. For instance, vapours condense on a surface during its phosphorescence, in a peculiar way. (3.) When a phosphorescent body glows, there is, along with the light, a very feeble emission of heat. The quantity is indeed extremely small, not measurable by the ordinary methods; but Dr. Draper, by very ingenious processes satisfied himself of its existence. (4.) Phosphorescence is not accompanied by any development of electricity. (5.) The intrinsic brightness of a phosphorus is very small; a fine specimen of chlorophane at its maximum of brightness, only yielding a light 3,000 times less intense than the flame of a very small oil-lamp. No wonder then, that the evolved heat is barely measurable, or that no expansion of volume is perceptible. (6.) It has long been known that when a phosphorescent body is heated, its phosphorescent power is thereby affected. Draper's law is this, "The quantity of light a substance can retain is inversely as its temperature." This principle explains many curious facts. For instance the Bolognian Stone shines brighter when exposed to the sky than to the sun,—*i. e.*, in the latter case the temperature of the stone rises, and the quantity of the light it fixes is less. Under violet and other glasses, stained with such colours as impede the warming effect, phosphorescence is even more vivid than when no glass has intervened. (7.) The quantity of light that a body can retain is directly as the intensity and quantity of light to which it has been exposed. The quantity of light emitted then, by the same body, under different circumstances, is "*directly* as the lighting power of the body that has acted on it, and *inversely* as its heating power." Dr. Draper concludes the paper in which these propositions are established, with the following general views. We give them *in extenso*, because they have an interest quite beyond their application to the special subject of phosphorescence:—"These various facts indicate, that when a ray of light falls on a surface, it throws the particles thereof into a state of vibration. An examination of the action of the differently coloured rays dispersed by a prism, shows that, in general, the greater the frequency of vibration of the impinging ray, the more brilliant is the phosphorescence. But in such a prismatic examination, we have constantly to bear in mind the disturbing agencies which are present, and especially the antagonizing effects of

heat; that this determines the amount of light that a phosphorus can receive, and also the rate of its subsequent extrication. In a letter which I published in the *Philosophical Magazine*, February, 1847, it was shown how the photographic action of light betrays the general principle of an interference of vibratory movements, and the production of antagonizing results in different parts of the solar spectrum. An argument is then brought forward to the effect, that as the violet end produces phosphorescence, and the red extinguishes it, this is a proof of opposition of action. In explaining this fact, M. E. Becquerel supposes the darkening power of the red rays to be due to the more rapid disengagement of the phosphorescence, by reason of the heat produced by these rays, and that the apparent antagonization is not attributable to the superposition of vibratory movements of light—rays of different frequency, but to the relations of caloric and light. The force of this explanation, however, disappears when it is understood that light and heat, the chemical and phosphorogenic rays are, according to the principles of this able experimenter, all manifestations of the same agent. It avails us nothing to say, that a want of phosphorescence at the less refrangible end of the spectrum is due to the heat-giving powers of those rays, when that very heat-giving power is, under the hypothesis, dependent on their comparative rapidity of vibration.—In the further explanation of phosphorescence, I abandon, therefore, expressions derived from the material theory of light, and present again the views alluded to in the letter in question, to the effect, that whenever a radiation falls upon a surface of any kind, it throws the particles thereof into a state of vibration; just as in the experiment of Fracastor, in which a stretched string is made to vibrate in sympathy with a distant sound, and yield harmonies and form notes. Such a view includes at once the facts of the radiation of heat, and the theory of calorific exchanges; it also offers an explanation of the connection of the atomic weights of bodies and their specific heats. It suggests, that all cases of the decomposition of compound molecules, under the influence of a ray, is owing to a want of consentaneousness in the vibrations of the impinging ray, and those of the molecular group, which, unable to maintain itself, is broken down, under the periodic impulses it is receiving, into other groups which can vibrate along with the ray.—If a hot body *a*, be placed in presence of a cold body *b*, the theory of the exchanges of heat teaches that the temperature of the latter will steadily rise until equilibrium take place. The molecules of *a* communicate their vibratory movement to the ether, and this in its turn imparts an analogous movement to the molecules of *b*. For as the ethereal medium is of vastly less density than the vibrating molecules, each of their oscillations will produce in it a determinate wave, which is propagated through it according to the ordinary laws of undulation, in such a way that the ether would be in repose after the wave had passed, were it not for the recurrence of the continuing vibration of the molecules. At each vibration the molecules of *a* lose a part of their *vis viva*, by the quantity they have communicated to the ethereal wave, the intensity or amplitude of the wave becoming less and less as this abstraction of force is going on. But, the ether being of uniform density and elasticity throughout, each of its particles communicates the whole *vis viva* it has received to the next adjacent, and would instantly come to rest were it not again disturbed by the vibrations of the material molecules.—These elementary considerations show how it is that a wave of sound passes through the air, or of light through the ether, and the particles of those media instantly come to rest; but a hot body, or a vibrating string, persists in its motions, which only undergo a gradual decline. If the vibratory molecule was in a medium of the same density, it would impart to it all its motion at once; and in the same way that a heavy molecule gradually communicates its motion to the ether, so, in its turn, does the ether to other systems of molecules.—Upon these principles one may explain the phenomena of phosphorescence. From a shining body undulations are propagated in the ether, and these impinging on a phosphorescent surface, throw its molecules into a vibratory movement. These in their turn impress on the ether undulations; but by reason of the difference of its density, compared with that of the molecules, they do not lose their motion at once, but it continues for a time, gradually declining away, and ceasing when the *vis viva* of the molecules is exhausted. —When a phosphorescent surface is exposed to the luminous source, it necessarily undergoes a rise of temperature, and the cohesion of its parts is diminished; but alter its removal from that source, as the temperature declines and irradiation goes on, the cohesion increases, and a restraint is put on those motions.—Now let the phosphorus have its temperature raised, and the cohesion of its molecules thereby weakened, and the restraint on their motions abated. At once they resume their oscillations, and continue them to an extent that belongs to the temperature used. When this has passed away, a still higher temperature will release them once more, and the growing will be again resumed.—What would be the result if we could cause the surface of a mass of water, in which circular waves are rising and falling, to be instantaneously congealed? It might be kept in that condition for a thousand years; and then, if instantaneously thawed, the waves would resume their ancient motion from the point at which it was arrested, and it would now go on to completion. So with these phosphori. Exposed to a light of suitable intensity, their parts commence to vibrate, but the freedom

of those motions is interposed with by their cohesion. Amplitude of vibration must always be attested by cohesion; and if the ray be removed, and the temperature permitted to decline, the restraint becomes greater and greater, and they pass into a condition somewhat like that which has just been illustrated. It matters not how long a time may intervene, rise of temperature will enable them to resume their motions.—These principles give us an explanation of all the facts we observe. We see how it is, that as we advance from one temperature to another, the phosphorus will resume its glow; and, that there is, as it were, for every degree, a certain amount of vibratory movement that can be accomplished, or, to use a different phrase, a certain amount of light which can be set free. It also necessarily follows, that different solids will display these motions with different degrees of facility, and hence shine for a longer or shorter time, and with light of different intensities.—But in liquids and gases, which want that particular condition of cohesion which is characteristic of the solid state, and whose parts move freely among each other, phosphorescence cannot take place, for it depends on the influence that cohesion has had in restraining the vibratory movements. Further, the condition of opacity does not permit the phenomenon to be established. The provoking ray cannot find access to disturb the interior layers of the mass, and even if it did, and phosphorescence ensued, how could we expect to be able to discern it through the impervious veil of the superficial layers? The light of the most brilliant phosphorus cannot be seen through the thinnest gold leaf. Its intensity is vastly too small. And these, therefore, are the reasons that no one has ever yet succeeded in detecting phosphorescence in metals or black bodies. It will be gathered from this explanation, that I am led to believe that all the facts of phosphorescence can be fully explained on the principles of the communication of vibratory motion through the ether; that, as when that theory, an incandescent body, maintained at incandescence, would eventually compel a cold body in its presence to come up to its own temperature, by making its particles exercise movement like those of its own, to the sunshine, as the flash of an electric shock compels a vibratory movement in the bodies in which its rays fall; that these movements are interfered with in the case of solids, but that they are instantly established, and almost as instantly cease, in the case of gases and liquids; that reducing the cohesion of a solid, by raising its temperature, permits a resumption of the movement; and that the condition of opacity, either melantic, or otherwise, is a bar to the whole phenomenon."

Photogalvanography. Those who are familiar with the expensive nature of the materials used in common photographic printing on paper embued with a sensitive salt of silver, and with the tedious series of washings, fixings, and toning processes, necessary before the finished picture is produced, will at once appreciate the value of any invention which promises to preserve the truthfulness of the photographic delineation, and at the same time to multiply the impressions by the same species of printing as is now used for common engravings—such a process is that named Photogalvanography, by its inventor, Mr. Paul Pretsch of Vienna. At a time when it is beginning to appear that the silver deposits, of which the common photographs consist, are subject to fade, and in many cases entirely to disappear through the action of causes difficult to discover and still more difficult to prevent, this new invention, which substitutes printer's ink for silver, is of double value. Mr. Pretsch, while superintending the Imperial Printing-office at Vienna, it would appear, has also been, for many years, devoting much time to the art of photography. During some experiments on the different substances which might be used as the vehicle of the sensitive materials, he was led to observe the singular effects which light produces on gelatine, which has been mingled with the bichromate of potash, and iodide of silver. These effects may be described thus:—When a plate of glass, coated with aqueous solution of gelatine to which has been added bichromate of potash and iodide of silver, is allowed to dry, and afterwards covered with any surface unequally transparent, and exposed to light, the gelatine film, on the parts where the light has acted, is found to have acquired the property of resisting the action of water, whereas the parts which have been protected from light readily imbibe water, swell up, and present a surface in relief. This raised part can be rendered permanent and hard by acids and varnishes, and might be used directly for printing from by inking and the action of a press in the ordinary way, although from the plate being of glass, and the type, so to speak, of gelatine, great pressure could not be applied, and therefore sharpness of impression could scarcely be expected. Pretsch, however, suggests that even by this simple process a single impression in greasy ink could readily be got and transferred to a stone, and afterwards multiplied to any extent by the well-known methods of lithography. Instead of this, however, which has not been yet much practised, the gelatine plate has a mould taken from it by partially softened gutta percha, so as to present a surface exactly resembling the original, but with the picture in the opposite species of relief. If a positive glass or paper photograph, that is one in which the lights are transparent when looked through, be used, the depressed parts of the gelatine surface would correspond to these lights. The gutta percha mould reverses this, and shows the lights of the picture in relief, the shadows being the sunk parts, similar to a common engraved copper plate, the picture being in

intaglio, as it is called. It is possible now to cover it with a film of black lead or other conducting substance, so as to fit it for the electrotype apparatus. By means of the electrotype a complete cast of the gutta percha mould is obtained in copper, having the parts, however, reversed, and the shadows in relief. This might be used like ordinary types for surface printing, but it is found to be preferable again to put this plate in the electrotype bath, so as to obtain an intaglio plate of such thickness as to stand the operations of the printing-press. In such a case it is obvious that, through the whole of the operations, the skill of the engraver's hand has not been called into requisition, and therefore the purity and absolute truth of the original photograph may be expected to be preserved. It is found, however, that in some cases it is preferable, where many impressions are wanted, instead of employing an intaglio plate with the degree of depth only given by the original swelling of the gelatinous surface, to build up the gutta percha mould with coatings of wax so as to increase the depth, and thus by sacrificing a little of the delicacy of detail to get relief enough in the electrotype cast, to permit the plate to be used as in surface printimg. Another plan, and a somewhat simpler one, has been successfully followed by Pretsch, for obtaining the printing block. The gelatine plate, after being subjected to the action of light and moisture, so as to give the picture in relief, as has already been described, is hardened by acids and varnishes, and is then covered by plaster of Paris, which gives a *mould* from which a block in type metal can be cast according to the methods of the stereotyper. This block, however, has seldom the degree of relief which would be requisite for clean printing; and the lights have therefore to be cut away by the tools of the engraver, thus leaving a surface as high in relief as that used in ordinary types for surface printing. From the outline which has now been given, the *rationale* of photogalvanography is obvious enough. It depends on the fact, that gelatinous films, in ordinary circumstances, become swollen when moistened, and that certain chemical substances set free from their combinations by the action of light, produce such a hardening of the gelatine, that the imbibition of water, and the consequent swelling, is prevented. For this purpose, bichromate of potash, or chromic acid, appear to be the most eligible. The bichromate, or the chromic acid alone, have no such effect, but when acted on by light it would seem that oxygen is set free, and that sesquioxide of chromium is produced, which, by other experiments, is known to have the power to convert gelatine into an insoluble leather. Under the photographic picture, then, at the parts where the light acts, this leather is produced, and it would seem that the action can be so regulated by tempering the time of exposure, as to reproduce the flat photograph with its mere lights and shades, as a surface in relief and depression such as is used in the old and long known methods of printing. The number of copies of the original photograph which can then be readily and cheaply produced is unlimited, as the electrotyper or the stereotyper can supply any quantity of plates. This new art, which depends, as its name indicates, on both photography and galvanism, the one for producing the original picture, and the other for the deposition of the electrotype plate, is but in its infancy. Difficulties have been met with in securing the uniformity of surface required, and it has been found necessary to call in the assistance of the engraver in perfecting the plates for printing. Future discoveries will no doubt remedy these defects. Mr. Fox Talbot in England, and Poitevin, Rosseau, and Musson, in Paris, are known to have been engaged for some time on similar researches, and it would seem with marked success. Pretsch, however, has been the first to make public his processes, and he is thus most justly entitled to the palm of the discoverer. For information regarding the ordinary photographic printing, the reader is referred to CALOTYPE, COLLODION, &c.

Polynome. Reference has been made in the earlier portion of the book to an article under this title, in which the author designed to elucidate some interesting speculations of the higher geometry. He was never able to carry out his intention. The reference was printed off before it was discovered that the blank thus left in his work could not be filled up by other hands.

Pulley. Through oversight, a notice of this simplest of the mechanical powers has been omitted in our text. Regarding the pulley abstractly, whether in its simple or composite state, whether as a combination of simple pulleys or in blocks or sheaves, it is easy to form a conception of the principle of its power. A single fixed pulley affords no *mechanical advantage*, as the tension of the cord being at all points the same, the power must be equal to the resistance. A single moveable pulley, on the other hand, will afford an *advantage* of 2 to 1, as the resistance is distributed equally on two folds of the cord, the one of which transmits its load to a fixed support, and the other alone is acted on by the power. If there are two moveable pulleys, the *advantage* will be 4, as there are four folds of the cord, and each fold sustains a fourth part of the resistance—three parts of which are transmitted to the fixed support, the remaining fourth part alone being taken up by the power. By the same reasoning it is proved that three moveable pulleys will give an advantage of 6, and n moveable pulleys an advantage of 2 n. In some systems of pulleys, when the cord is not continuous, this formula for the advantage will not apply; but in all cases the work done by the power must be equal to the work overcome in the resistance.

Screw, or Screw Propeller, a name generally, although improperly, given to all the forms of ships' propellers which have any resemblance to a true screw. It is usually placed at the stern of the vessel, sometimes at the bow, and always under water. The art of propelling ships by this means was ill understood, and made little progress till Mr. Francis Smith, along with others, in 1840, showed its successful application in the steamer Archimedes, a vessel of about 240 tons and 90 horse power. The propeller originally used was a true screw, the blade of which made one convolution on the axis. This was afterwards altered to two blades, making half a convolution; and latterly it was found that the best results were obtained when each blade made only a sixth of a convolution. The British Government, after satisfying themselves of the success of the experiments on board the Archimedes, introduced the screw propeller to the Navy, where its advantage over the paddle or side-wheel became speedily so apparent, that its general adoption was the result. The great naval review at Portsmouth, in 1856, showed the extent and variety of its application to the great line-of-battle ships of 120 guns, to the little high pressure gun-boats, and to the floating batteries, with their thick iron armour. The adoption of the screw propeller in the merchant service was more gradual, as it was generally thought that the same amount of speed could not be got with the screw as with the side-wheels. The construction, however, of the steam-yacht Fire Queen, for Mr. Thomas Ashton Smith, and of the Queen's yacht Fairy, proved that great speed was obtainable from screw propellers, without the expenditure of more power than the paddle-wheels require; for both these vessels were at least as fast as the fastest paddle vessels of their size. The merchants are now satisfied of its great advantages, especially for cargo vessels, and its adoption is all but general. The efficiency of propellers, the relation between the power expended upon them and the useful work they produce in propelling vessels, is found to vary with their immersion or the depth of water above them. Their diameter, and the pitch or distance between the convolutions of each blade being the same, the further the screw is immersed the more effective it becomes; — a fact observable in those screw vessels which, especially when lightly laden, are sailed with so much more of the body of the vessel out of the water forward than aft, or, as it happens, drawing so much more water aft than forward: Mr. G. Rennie proved the increasing efficiency with the depth on a model screw, about 18 inches diameter: the results of his experiments were laid before the British Association at Cheltenham in 1856. It is stated by some authors, and believed in by many makers of vessels, that a screw of large diameter, extending from the keel to the surface of the water, or it may be above it, is more effective than any smaller screw upon the same vessel; but this requires more proof before it can be accepted as true; for many facts, obtained from very successful screw vessels, lead to the belief that a smaller screw, immersed just so far below the surface as not to take down air in its revolutions, and, as it were, churn the water, is more effective than a larger one, which takes down air, and makes a great commotion. Had Mr. Rennie tried screws of different diameters in his experimental apparatus, it is probable he would have discovered the relationship, if any, between the diameter of a screw, on a vessel having a given draught of water, and its immersion below the surface, which would give the maximum efficiency. —The pitch has great influence upon the efficiency of the propeller. It is clear that if it were infinitely small, there would be no motion communicated to the vessel in the direction of the axis of the propeller; the whole force of the engine would be spent in giving the water which adhered to the propeller (now a disc) a motion at right angles to that axis. And if the pitch were infinitely great, the blades would be parallel with the axis; there would therefore be no motion in the direction wished. The power of the engine would be spent, as before, in giving motion to the water at right angles to the course wanted. Between these two infinite pitches, however, there is an infinite number which would make the vessel advance, and some one of that infinite number must be the most efficient. But although this most efficient pitch has not been determined for any vessel, and probably cannot be determined on account of the trouble and expense attending such trials, it appears that the most successful results have been obtained with screw propellers whose pitch is about equal to the diameter. The present fashion, however, of driving screw propellers direct from the engine, with the intervention of large cog-wheel and pinion, (formerly used to increase the revolutions of short-pitched propellers), renders these short pitches nearly impracticable, on account of the injurious speed at which the engine itself requires to go; and much longer pitches are consequently used with such engines. It is therefore an important question for those interested in the sailing of screw vessels, what are they to gain by having a direct acting engine? and what will they lose in consequence of being obliged to adopt, in all probability, a less efficient propeller?—In powerful sailing ships, making voyages that extend through calms to perhaps a half the circumference of the globe, it is possible that the most economical results may be obtained from a high pressure engine; for, as it can be driven fast enough for the most efficient screw, the smaller efficiency of engine, added to the greater efficiency of the short-pitched screw, may be more efficient than a more economical condensing engine, acting upon a longer pitched screw. For works on the subject, see *Traité de l'Hélice Propulsive*, by E.

Paris, Bourne; Halstead, Rawson, Atherton, &c., on the Screw Propeller.

Steam. Comparison of the result of Messrs. Fairbairn and Tate's experiments on the density of steam, with those of the theoretical formula (25 B), given in page 348, HEAT, MECHANICAL ACTION OF.

Temp. Fahr.	Ratio of vol. of Steam to vol. of Water. By Theor.	By Exper.	Temp. Fahr.	Ratio of vol. of Steam to vol. of Water. By Theor.	By Exper.
136°·88	8276	8262	244	936	896
160·016	4790	4911	245	920	890
171·55	3722	3710	257	756	751
175·15	3433	3426	262	698	684
182·32	2960	3045	268	635	633
188·09	2630	2621	270	616	604
197·48	2180	2147	283	506	499

Steam-Boiler. A close vessel, in which, by the action of heat, water is converted from the liquid to the vaporous condition. In STEAM-ENGINE, § 2, is a brief statement of the parts and appendages of a steam-boiler. The materials employed for steam-boilers are steel, cast-iron, wrought iron, and copper. The tenacity of a good wrought iron boiler plate is about 56,000 lbs. per square inch; that of a double rivetted joint about 34,000, and that of a single rivetted joint about 28,000, per square inch section of plate, according to Mr. Fairbairn. The tenacity of steel boiler plates is from 80,000 to 90,000 lbs. per square inch; so that they are stronger than wrought iron plates in the proportion of 8 to 5 nearly; and the same proportion probably holds for their rivetted joints. The tenacity of sheet copper is about 35,000; joints, 18,000. The tenacity of cast iron of a quality fit for boilers, should be at least 18,000 lbs. per square inch; but this material is considered dangerous in Britain. The first requisite of the boiler is strength sufficient to resist the pressure of the steam. The ultimate strength of the boiler should exceed the working pressure in a ratio which is called the *factor of safety.* The factors of safety employed in practice range from 5 to 10; from 6 to 8 may be considered a good medium. The only forms suitable for safely withstanding great pressures, whether from without or from within, are the sphere, the cylinder, and the flat plate bound with stays.—To find the ultimate strength, or bursting pressure, of a cylindrical boiler, let T be the tenacity of the material, allowing for joints; B the bursting pressure; r the external, and r' the internal radius of the cylinder; then

$$\frac{\mathrm{B}}{\mathrm{T}} = \frac{r^2 - r'^2}{r^2 + r'^2}.$$

Unless B is to be very large, a sufficiently close approximation is given by the formula

$$\mathrm{B} \div \mathrm{T} = m \div r,$$

where m is the thickness of metal. The strength of a hollow sphere is double that of a cylinder of the same radius and thickness. According to Mr. Fairbairn, the flat ends of a cylindrical boiler should be once and a-half the thickness of the sides, and should be stayed to each other by bars, or to the sides by *gussets:* the flat surfaces of locomotive fire-boxes require to be stayed by bolts at every 4 or 5 inches, screwed and rivetted; the thickness of the plates should be at least one-half of the diameter of the bolts: the tenacity of iron bolts is 60,000 lb. per square inch; that of copper bolts 36,000. Cylindrical wrought iron flues, pressed from without by the steam, tend to give way by collapsing; and the working pressure should not exceed one-sixth of the collapsing pressure. It has been found by Mr. Fairbairn (*Phil. Trans.*, 1858) that the following formula gives approximately the *collapsing pressure p*, in lbs. on the square inch, of a plate iron flue, whose length l, diameter d, and thickness t, are all expressed in *the same units of measure*:—

$$p = 9{,}672{,}000\ t^2 \div l\,d.$$

As the resistance of flues to collapse depends very much on their being exactly cylindrical, Mr. Fairbairn recommends that they should be made, not with lap joints, like boiler shells, but with butt joints and covering strips. Mr. Fairbairn having strengthened tubes by rivetting round them rings of T-iron, or angle iron, at equal distances apart, finds that their strength is that corresponding to the *length from ring to ring.*—To provide a sufficient exit for steam, the area of the safety-valve should be at least 0·006 square inch for each pound of water evaporated per hour. Care should be taken that the weakest part of a boiler is that whose bursting would cause least damage. The excellence of the furnace and boiler consists in evaporating the greatest quantity of water possible with a given quantity of fuel; and the ratio of the heat communicated to the water to the whole heat of combustion of the fuel may be called the *Efficiency of the Furnace and Boiler.* The following are some examples of the total heat of combustion of one pound of different kinds of fuel, expressed in thermal units, and also in foot-pounds.

	Thermal Units.	Foot-Pounds.
Hydrogen	62032	47,888,704
Pure carbon	14500	11,194,000
Olefiant gas	21344	16,477,568
Various liquid hydro-carbons, average	20000	15,440,000
Sulphur	4000	3,088,000
Coal and coke, various kinds	from 14000	12,352,000
	to 10000	7,720,000
Wood, average	6000	4,632,000

The *available heat of combustion* falls short of the total heat by losses from the following causes:—1. Evaporating moisture from the fuel. 2. External conduction and radiation. 3. Imperfect combustion. 4. Heating gases which ascend the chimney. The available heat of combustion is usually determined by ascertaining the quantity of water at 212° evaporated by one pound of fuel. At this temperature, each pound of water requires for its evaporation, 966 thermal units, equivalent to 966 × 772 = 745,792 ft.-lbs. 1. Loss of heat by evaporating moisture from the fuel is avoided by using dry fuel. 2. Loss by external conduction and radiation is prevented

by clothing the furnace with concentric shells of brickwork; by constructing the boiler so that the furnace is contained within it, and by clothing the boiler, as formerly mentioned (STEAM ENGINE). 3. Loss by imperfect combustion is prevented by so constructing the furnace, and so managing the fire, as to maintain a regular and copious supply of air to all parts of the burning fuel. The quantity of air *chemically* necessary for the combustion is about 12 lbs. for each pound of fuel; and Mr. C. W. Williams has shown, that, in order to dilute sufficiently the products of combustion, double that quantity of air ought to be supplied:—that is, 24 lbs. of air per lb. of fuel. The volumes occupied by this air at atmospheric pressure, and at various temperatures, are as follows:—

Temperature	32°	212	392	662	1832
Volume of 24 lbs. of air cubic feet	300	410	519	683	1395

A supply of air exceeding this increases too much the loss of heat up the chimney. Thorough combustion of the fuel is indicated by the absence of smoke, and of flame in the chimney. It is insured chiefly by having a large grate, over which the fuel is thinly and equably spread, and large flues and chimney; by supplying fuel frequently, and in small quantities at a time, and spreading it well (operations which require a careful and conscientious fireman); and by various special contrivances, too numerous to describe, for admitting and distributing air. In marine engines, and still more in locomotive engines, it is impracticable to have so large a grate as is attainable in stationary engines, and careful firing therefore becomes of increased importance. 4. Loss of heat carried off by the gases which ascend the chimney is prevented by having a large surface for the conduction of heat from the flame to the water in the boiler. In stationary engines, this can be attained by having a large boiler traversed by long and large flues; but in marine engines, and still more in locomotive engines, it is impracticable to use large boilers, and therefore in them a great heating surface within a small space is obtained by sending the flame through a great number of small parallel tubes, which traverse the water in the boiler. Tubular boilers are also used in some stationary engines. In a few boilers, the water is inside the tubes, and the flame outside. The following table gives some examples of the dependence of the efficiency of a boiler, (that is, the ratio of the available to the total heat of combustion), upon the area of grate and of heating surface.

Boilers.	Cornish and Lord Dundonald's.	Marine. Tubular	Locomotive.
Area of grate in square feet per lb. of fuel per hour..........................	0·1	0·0625 to 0·05	0·025 to 0·0125
Area of heating surface, in square feet per lb. of fuel per hour........	4 to $3\frac{1}{2}$	2 to $1\frac{1}{4}$	average about 1
Efficiency	0·85 to 0·8	0·7 to 0·6	0·5 to 0·4

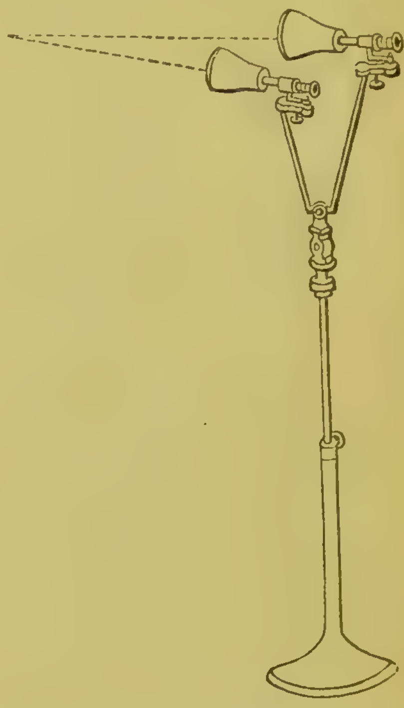
The nature and thickness of the metal of the flues and tubes has no influence appreciable in practice on the efficiency of the furnace and boiler. The following table shows the *available heat of combustion*, of one lb. of various kinds of coal, as ascertained by the experiments of Sir H. De la Beche, and Dr. Lyon Playfair, on a boiler whose efficiency appears to have been about 0·8 or 0·75.

	Thermal Units.	Foot-Pounds.
Anthracite......	10000 to 9000	7,720,000 to 6,948,000
Welsh	10000 to 7000	7,720,000 to 5,404,000
Lancashire......	9000 to 6000	6,948,000 to 4,632,000
Newcastle.......	9000 to 7000	6,948,000 to 5,404,000
Scotch	8000 to 7000	6,176,000 to 5,404,000

From most boilers a certain quantity of water is carried into the cylinder in the liquid state:—this is called *priming*. The exact amount of priming has not in any case been ascertained, owing to the difficulty of distinguishing between the water so carried over, and that produced by liquefaction of the steam in the cylinder. The feed-pump, in land engines, supplies from $1\frac{1}{10}$ to 2 times the water actually effective in driving the engine, the excess providing for loss by priming and by escape at the safety valves. In marine engines, the brine discharged from the boiler has also to be provided for. The brine pumps should not be less than $\frac{1}{3}$ of the capacity of the feed pumps, in order that the brine in the boiler may never be more than treble the strength of sea-water. Boilers are liable to become encrusted inside with a hard deposit of the minerals contained in the water. The most usual deposit is carbonate of lime; this can be prevented by dissolving sal-ammoniac in the water; for that salt and the carbonate of lime are mutually decomposed, producing carbonate of ammonia and chloride of calcium, of which both are copiously soluble in water, and the former is volatile. In some cases the deposit is prevented from hardening by diffusing some farinaceous substance in the boiler, such as potatoes; but this is a clumsy proceeding, and apt to lead to corrosion or overheating of the plates.—*Explosions* of steam boilers, so far as they are understood, arise and are to be prevented in the following manner:—1. From original weakness: this cause is to be obviated by due attention to the laws of the strength of materials in the designing and construction of the boiler. The general principles to which those laws lead have been sketched at the beginning of this article. 2. From weakness produced by gradual corrosion of the material of which the boiler is made.—This is to be obviated by frequent and careful inspection of the boiler, and especially of the parts exposed to the direct action of the fire. 3. From wilful or accidental obstruction or overloading of the safety-valve.—This is to be obviated by so constructing safety-valves as to be incapable of accidental obstruction, and by placing at least one safety-valve on each boiler beyond the control of the engineman. 4. From the sudden production of steam of a

pressure greater than the boiler can bear, in a quantity greater than the safety-valve can discharge. There is much difference of opinion as to some points of detail in the manner in which this phenomenon is produced; but there can be no doubt that its primary causes are, first, the overheating of a portion of the plates of the boiler, (being in most cases that portion called the *crown of the furnace*, which is directly over the fire), so that a store of heat is accumulated; and, secondly, the sudden contact of such overheated plates with water, so that the heat stored up is suddenly expended in the production of a large quantity of steam at a high pressure. Some engineers hold, that no portion of the plates can thus become overheated, unless the level of the surface of the water sinks so low as to leave that portion of the plates above it, and uncovered; others maintain, with M. Boutigny, that when a metallic surface is heated above a certain elevated temperature, water is prevented from actually touching it either by a direct repulsion, or by a film or layer of very dense vapour; and that when this has once taken place, the plate, being left dry, may go on accumulating heat and rising in temperature for an indefinite time, until some agitation, or the introduction of cold water, shall produce contact between the water and the plate, and bring about an explosion. All authorities, however, are agreed, that explosions of this class are to be prevented by the following means:—1. By avoiding the forcing of the fires, and the making of the boiler to produce steam faster than the rate suited to its size and surface. 2. By a regular, constant, and sufficient supply of feed-water, whether regulated by a self-acting apparatus, or by the attention of the engineman to the water-gauge; and, 3, should the plates have actually become overheated, by abstaining from the sudden introduction of feed-water (which would inevitably produce an explosion), and by drawing or extinguishing the fires, and blowing off both the steam and the water from the boiler. For authorities, see STEAM ENGINE.

Stereoscope Telescopic. A new form of the Stereoscope recently imagined by Mr. Elliot. The following is his own description of it:—" The object of the Telescopic Stereoscope is to unite *large* binocular photographs in a different way from that in which Professor Wheatstone's instrument does so, and, as I think, a much superior way. It is done by means of two small telescopes, with the lines of their axes crossing each other, so that the right hand picture is seen with the left eye, and *vice versa*. The pictures also are placed in a different order from that which they take in the common stereoscope, the right and left hand pictures interchanging places.—When the instrument is on a stand, several adjustments are necessary. These require a little trouble; and unless they are *all* carefully attended to, the effect is not fully brought out; but the trouble, when taken, is well repaid, as the effect is exquisite. The boughs of the hawthorn tree hing so loose and airy, you would think you could gather the haws from it.—When the instrument is intended to be held in the hand, it is simpler, being then nothing more than an opera-



glass, with its axes converging. Some ingenious artist may perhaps make an instrument to serve both purposes."—Conflicting views having been taken in connection with Mr. Elliot's name regarding the invention of the stereoscope, we publish the following letter from himself, so that these may be set wholly at rest, and his name and merits be disentangled from controversy. Mr. Elliot has long been closely related by the best ties of friendship to the Editor of this volume; nor can any doubt rest on the slightest of his assertions. Certainly, last of all, is he to detract from the incontestable merit of Professor Wheatstone:—"Edinburgh, 22d December, 1856. The statement of facts given by Sir David Brewster in his volume on the stereoscope, and in the *National Magazine*, was supplied by myself, at his request, and I am responsible for its accuracy; but from first to last I have maintained that I have no claim whatever to make *versus* Professor Wheatstone, and have, in fact, publicly and privately, as often as I have been asked, conceded everything to him. By a reference to my letter, in the *Philosophical Magazine* for May, 1852, it will be seen that, even when I believed that my instrument was made many years before his, I considered that I had forfeited every claim in consequence of not having made known my invention except to one or two friends. As soon as I learned the date of Wheatstone's invention, which occurred three or four weeks after the previous letter was written,

I again requested the Editor of the *Philosophical Magazine* to say so, and to withdraw every *appearance* of claim on my part. This he did in the number for June. Again previous to the late dispute on the subject, finding my name referred to, in several of the newspapers, as the inventor of the instrument, I immediately wrote a disclaimer to the *Courant*, and two days thereafter, more explicitly to the *Scotsman.* I did consider then, however, that I had the priority, although unimportant, in regard to the particular form of the instrument, which has been designated as the 'Ocular Stereoscope;' but, subsequently, when Professor Wheatstone brought forward some further information, showing the very early date of his first notice of the subject, and *that* in *both forms*, I wrote to the *Times*, making a still more sweeping concession as to Wheatstone's priority in everything; but expressly denying that, either in the first conception or in the subsequent construction of my instrument, I had borrowed a single idea from him. If, after my own invention, I heard of Wheatstone's instrument, it had so entirely escaped my memory, that in 1852 I read his description of it with the firm persuasion that it was newly brought out. I was led to the idea of my instrument solely from having been often for years reverting to a question on which I had formerly written an essay—'On the means by which the eye conveys the knowledge of distance to the mind.' I thought, if such are the means, the eye might be deceived by presenting to it fictitious data. I well recollect when the determination first crossed my mind to construct an instrument for the purpose. It was in walking along a footpath through a corn-field in Kent. I don't know the date; but as I left Kent in 1834, it could not be later; and I gave that date to Sir David, not as the actual date, but as the *posterior limit.* I probably mentioned my intention to one or two individuals at the time; but of this I am uncertain. More serious business pushed the purpose aside, and I never resumed it till some time probably in 1838 (I gave 1839 again as the *ulterior limit* from precise data), when my friend. Mr. Adie, asked me to contribute a paper to the Polytechnic Institution. I proposed that subject, and immediately constructed the instrument; but it gave him no satisfaction, and I laid it aside for the time, intending to bring it out again and to improve it. But I always regarded it as a thing that only a very few would take any interest in; and such would have been the case with all forms of the stereoscope, had not photography come to its relief, and given it new life. In renouncing all claim to the invention, in consequence of Wheatstone's priority, I may, however, make one exception. The first application of the stereoscope to landscapes, or to anything beyond geometrical figures, was *certainly* mine; and, indeed, I don't know that I ever thought of applying it to anything else than landscapes. Had the *Times* chosen to publish my letter, it would probably have ended the controversy; but ("*non sic placuit deis*), it pleased the said *Times* rather to let the great men fight it out."

Thermo-Electricity, table referred to p. 837:

ELECTRICAL CONVECTION OF HEAT.

	In Cadmium	Positive.
	Brass	Positive.
Order doubtful.	Copper	Positive.
	Lead, Tin	equal Positive.
	Zinc	Positive, zero, or negative.
	Gold	Positive, zero, or negative.
	Silver	Positive, zero, or negative.
Order doutbful.	Iron	Negative.
	Platinum	Negative.
	Nickel	Probably negative.
Probably nearly equal.	Palladium	Probably negative.
	Mercury	Negative.

Universal Instrument. The notice of this instrument, intended for the text, has been accidentally omitted. It is virtually an altitude and azimuth instrument of great power, but whose *solid* dimensions are such that it is quite portable. The special characteristic of it is this:—The telescope, instead of being a straight tube, as is usual in this country in all such instruments, is broken into two arms at right angles to each other. The break is in the middle of the length of the tube; and at the break a solely reflecting prism is placed, which turns the rays entering the object-glass in a rectangular direction. The eye-piece is, in this way, placed at the centre of the altitude circle; and the telescope becomes free to move through all altitudes. This instrument is much and very deservedly prized on the continent, although comparatively little known in Great Britain. For instance, Struve has employed it in all his large surveys. Its theory is not difficult, and its adjustments simple. A full account and discussion of it will be found in Struve's description of the great observatory at Poulkova, and indeed in every great foreign treatise on Practical Astronomy. Very fine instruments of this description are made by Ertel of Munich, and Repshold of Hamburgh. A few of them are in this country.

Ventilation.—At the close of HEATING OF BUILDINGS, page 431, reference is made to a sequel to it, which we had assigned to the heading VENTILATION. An admirable practical essay on this most interesting subject has just reached us from Mr. R. Ritchie, Civil Engineer, Assoc. Inst. C.E.L. (the author of article on HEATING), which we should most gladly have printed in addition to the general notice in the text, had space and the press of time permitted. It contains so full and scientific an analysis and criticism of all that has been done and proposed on this most essential subject, that we cannot pass the opportunity of expressing the hope that Mr. Ritchie will extend it, and publish it as a substantive work. Mr. Ritchie has already obtained many distinctions from learned societies on account of his labours in this very important field.

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